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GEORGE L. MADDOX

IN THIS ISSUE

ROBERT M. EMERSON is associate professor in the Department of Sociology at the University of California, Los Angeles. He has conducted field research on social control processes in a variety of institutional settings and is now studying institutions dealing with ex–mental patients in the community.

ERIC M. LEIFER is a graduate student in the Department of Sociology at Harvard University. His main interest is in group formation. He is currently testing a theory of markets and is also conducting research on sequential equilibria in strategic contexts, using chess tournament data.

MICHAEL HUGHES is assistant professor of sociology at Virginia Polytechnic Institute and State University. He is currently studying mental hospitalization and the effects of living arrangements on mental health. His main interest is in how the structure of the social environment is related to variation in mental well-being.

WALTER R. GOVE is professor of sociology at Vanderbilt University. He has just coedited a book on integrating biological, psychological, and sociological factors in explaining human behavior and is editing a reader on the sociology of mental health. He is collaborating with Michael Hughes on studies of mental hospitalization and the effects of living arrangements on mental health.

JOHN F. PADGETT is assistant professor of sociology at Harvard University. His current organization theory research focuses on plea bargaining.

STEVEN A. TUCH is a Ph.D. candidate in sociology at the Pennsylvania State University. His current research activities involve an examination of the effects of intergenerational and career occupational mobility on racial attitudes, particularly attitudes relevant to institutional racism.

KARL L. ALEXANDER is associate professor in the Department of Social Relations at the Johns Hopkins University. His primary research interests and activities involve school organizational effects on educational achievements and attainments.

THOMAS W. REILLY is a Ph. D. candidate in the Department of Social Relations at the Johns Hopkins University. His current work centers on the examination of family transition points.

ERIC WOODRUM is assistant professor of sociology at North Carolina State University. His major research interests are in racial and ethnic relations and the sociology of religion. He is currently compiling a data set on popular religious belief system change while continuing research on various aspects of Japanese American adaptations.

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Goode, W. J. 1967. "The Protection of the Inept." *American Sociological Review* 32: 5–19.

Moore, Wilbert E., and Arnold S. Feldman. 1960. *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt, ed. 1962. *The American College*. New York: Wiley.

Weber, Max. (1921) 1968. "Society's Problems." Pp. 12–16 in *Economy and Society*, edited by Guenther Roth and Claus Wittich. New York: Bedminster.

On Last Resorts[1]

Robert M. Emerson
University of California, Los Angeles

In a variety of social control settings, the use of extreme sanctions is held to be appropriate *only as a last resort*. This paper examines the nature, occasions, and accomplishment of such last-resort responses. Last resorts characteristically claim that there is no alternative but to invoke some dubiously valued sanction, a claim advanced against the backdrop of the "normal remedies" customarily used in a particular setting. Requested last resorts are justified by showing either that all such normal remedies are specifically inappropriate or that they have failed to contain the trouble. The conditions governing the latter procedure for justifying the turn to last-resort sanctions are analyzed in detail.

Under some circumstances, social control sanctions are appropriately invoked only as a "last resort." This paper explores the nature, features, and use of these sanctions or remedies. Initially the structure of last resorts will be distinguished from two other types of decision logics, those termed "first-resort" and those referred to as "dispreferred" responses. The analysis will then identify two distinct empirical procedures for justifying use of the last-resort measures. Finally, the paper examines the relatively less recognized of these procedures, showing how the construction of a distinctive "remedial past" precludes any alternative but the last-resort remedy. This analysis calls attention to the specific nature and use of last-resort decision logics and highlights the more general relevance of practical decision making of which they are an expression.

LAST-RESORT, FIRST-RESORT, AND DISPREFERRED REMEDIES

A number of social control responses to the troubled and difficult to manage in our society are viewed by those who invoke them with a peculiar sort of pragmatic skepticism: remedies are available and may have to be used, but

[1] This paper is deeply indebted to the many insightful readings and ever-sustaining enthusiasm of my friend and colleague, Melvin Pollner; indeed, his innumerable conceptual, substantive, and organizational suggestions make specific acknowledgment of each and every one impossible. Arlene K. Daniels has read several versions of the manuscript and has provided many substantive and editorial changes. William Darrough, Jack Katz, Edmund B. Rochford, and Gaye Tuchman also provided helpful and thoughtful readings. Requests for reprints should be sent to Robert Emerson, Department of Sociology, University of California, Los Angeles, Los Angeles, California 90024.

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they are considered likely to compromise or defeat the very ends they were intended to achieve. The use of such responses accordingly becomes restricted and hedged with caution. Under extreme circumstances, they may legitimately be invoked "only as a last resort."

Commitment to a mental hospital provides a contemporary illustration. Much prevailing opinion holds that the hospital provides effective help only in a small proportion of cases and that its indiscriminate use fosters unnecessary dependency and institutionalization. A movement toward "decarceration" (see Scull 1977) has gained momentum. Where mental hospitals have not been drastically reduced in size or abolished outright, they are seen as offering at best a limited, partial response to mental disorder. Thus, under prevailing law and policy recommendation, hospital commitments should be made reluctantly and only in extreme instances, and even then only if preventive measures to keep the disturbed person in the community are impractical.

Similar claims that particular remedies are appropriate only as a last resort arise in a variety of other social contexts. The medical world, for example, abounds with techniques considered so risky, uncertain, or morally questionable as to be properly employed only as last-resort measures. Thus, at least some within the medical profession hold that coronary artery bypass surgery, given its high risks and lack of documented effectiveness, should be undertaken cautiously, in something approximating a last-resort manner (Millman 1978).[2] Similar arguments have also been advanced against the use of disc surgery to correct lower back pain (*Los Angeles Times*, Sept. 1, 1978). In criminal law, the use of "deadly force" in self-defense is subject to a last-resort requirement: self-defense cannot be legitimately claimed "if a safe avenue of escape is available" (although there is no "duty to retreat" in one's home).

Finally, specific institutions may frame the use of extreme sanctions in last-resort terms. Consider, for example, the following defense of electric shock from cattle prods as negative reinforcement in a behavioral modification program for the institutionalized mentally retarded (*Los Angeles Times*, Feb. 8, 1976):

> [The medical director at Pacific State Hospital] told The Times, "There are 600,000 mentally retarded in the state of California, 10,000 of them in state hospitals, which means we have one in 60 of the most difficult to manage—70% are incapable of verbal communications and 50% are nonambulatory. Most admissions we get any more are behavior problems that

[2] Millman notes that proponents of this position, "sometimes called 'doves' by surgeons, . . . recommend surgery only in the cases of patients with truly disabling angina and severely hampered life-styles who have not been helped by medical therapy even after careful and thorough efforts to control pain with drugs" (1978, p. 228).

> can't be handled, or infirm—people who are difficult to care for in convalescent care homes." But he said electric shock is used only as a last resort—"only after all else has failed"—and then only after careful review and consent of a patient's parents or guardian.

This cautious, try-all-else attitude toward sanctions regarded as last-resort measures contrasts sharply with the optimistic, try-it-at-the-first-sign-of-trouble use of other remedies. The latter, which can be termed "first-resort" remedies, are regarded as the preferred, unequivocally best response to a particular sort of trouble. Ironically, many remedies now regarded as last resorts were seen as first resorts in other times: mental hospitalization, for example, possessed such first-resort character when asylums were first created in the 19th century and belief in the efficacy of "moral treatment" remained unshaken: "Institutionalization was not a last resort of a frightened community. Quite the reverse. Psychiatrists and their lay supporters insisted that insanity was curable, more curable than most other ailments. Spokesmen explained that their understanding of the causes of insanity equipped them to combat it, and the asylum was a *first resort,* the most important and effective weapon in their arsenal" (Rothman 1971, pp. 130–31; emphasis added).[3]

Similarly, in the past, corporal punishment provided schools with a first-resort sanction, one routinely and preferentially employed in dealing with normal classroom discipline problems, whether or not lesser remedies had already been tried. But the use of corporal punishment in the schools has become more questionable in recent times. At both state and local levels efforts have been made to restrict physical punishment to those occasions "when milder disciplinary measures have failed" (as ordered by the Los Angeles School Board, reported in the *Los Angeles Times,* Nov. 23, 1971). This policy reflects the view that the personal and moral indignities im-

[3] While it is beyond the scope of this paper to analyze how first resorts evolve into last resorts (apparently the more common sequence, rather than the reverse), several initial considerations can be offered. First, such a transformation often occurs because of disillusionment with the ***rehabilitative effectiveness*** of social control responses: critics showed that prisons, reform schools, mental hospitals, and disc and coronary bypass surgery simply do not cure as claimed (or hoped). Along these lines, Magaro (1974) suggests that the overly optimistic expansion of an initially successful psychiatric treatment to inappropriate populations produces a recurring cycle of hope and disillusionment: "A method is born, it demonstrates its truth, it cures its recipients, it expands dramatically, it becomes criticized, it is shown not to be effective, it is replaced, and it enters the annals of quaint humor" (p. 61). Second, some demonstration of hitherto unappreciated ***risk*** may accompany the questioning of efficacy: prisons and mental hospitals not only fail to rehabilitate, but also foster stigmatization, institutionalization, and recidivism. Emphasizing the high risk of surgery has similar effects for bypass and disc operations. Finally, some first resorts suffer decline from changes in the ***moral legitimacy*** rather than the instrumental efficacy of their means: moral considerations increasingly prohibit extreme or unusual punishments, for example.

posed by such practices as "swatting" outweigh their effectiveness as a routine control measure.[4]

In other words, first resorts represent the best way to manage a particular sort of trouble. First-resort decisions are typically presented as what *should* or *ought* to be done, regardless of practical contingencies and local limitations: Given the efficacy of "moral treatment," for example, each and every person found insane should be sent for cure to the asylum. In contrast, last-resort decisions are typically framed in an idiom of *necessity:* The claim is that "we *have* to hospitalize," that there is no alternative but to turn to this particular response. Or, as a truant officer emphasized in explaining that he took formal action in juvenile court only as a last resort: "Only when it's impossible. You try everything and get nowhere. . . . The only time's where nothing works. Then you have to do something" (Emerson 1969, p. 53).

First- and last-resort responses differ in revealing ways from what can be termed preferred and dispreferred responses.[5] By way of illustration, consider Bittner's (1967) analysis of the police use of arrest on skid row. In this setting, arrests are appropriate only when alternative ways of dealing with a specific peace-keeping problem are not available or too costly. As Bittner emphasizes, "Whenever there exist means for controlling the troublesome aspects of some person's presence in some way alternative to an arrest, such means are preferentially employed, provided, of course, that the case at hand involves only a minor offense" (p. 710). However, this reluctance to arrest can be set aside on purely pragmatic grounds, as when the practical "costs" of arrest to the patrolman decrease, or the practical costs of the usually preferred alternatives increase. By implication, any skid-row arrest can be justified by establishing that action as the practically "best" alternative available in those particular circumstances. Expediency then provides ample grounds: that the paddy wagon happened along, for example, can overcome the patrolman's reluctance to arrest (as cited by Bittner, p. 713). Thus, while there is a presumption against using arrest, this response nevertheless continues to be weighed relative to other

[4] This tendency is reflected in a North Carolina court decision (Baker v. Owen, 395 F. Supp. 294, MDNC, 1975), upheld without comment by the U.S. Supreme Court, defining the conditions for schools to use corporal punishment in the following terms: "[E]xcept for those acts of misconduct which are so anti-social or disruptive in nature as to shock the conscience, corporal punishment . . . should never be employed as a first line of punishment for misbehavior. The requirements of an announced possibility of corporal punishment and an attempt to modify behavior by some other means—keeping after school, assigning extra work, or some other punishment—will insure that the child has clear notice that certain behavior subjects him to physical punishment" (395 F. Supp. at 302).

[5] The terms preferred and dispreferred are adapted from the conversational analysis of Sacks (1973) and Schegloff (1979).

available alternatives and under changed circumstances may be preferred over those alternatives.

Last-resort decisions, in contrast, advance the claim that the contemplated action is the only available alternative. Thus, to warrant the use of a last-resort remedy, it is necessary to show that there is no alternative response available, not merely to provide grounds for ranking that response as "better" than other, still viable alternatives. Were arrest a last-resort response of skid-row patrolmen, its actual use would be justified by accounts showing that it provided the only available course of action, not by accounts locating grounds for preferring it over other possible responses.

The justification of the use of cattle prods to control the behavior of retarded inmates also illustrates this difference. One might have defended the use of these instruments as the most effective way to discipline hard-to-manage patients. Such a defense would imply that, while remedial alternatives such as verbal reprimand, restriction of privileges, or other standard "negative reinforcements" had some effect in controlling misconduct, the effects of shocks from cattle prods were more immediate, long lasting, and efficient. Here shock would provide the best alternative on the basis of its effectiveness compared with other feasible options. In fact, however, shock was justified not as the best, but as the only viable alternative, given the character of the inmates and the inability of other responses to control their behavior.

In general, then, last-resort responses are presented as the outcome, not of choice between options, but of the unavailability of any choice. A last resort is discontinuous with alternative responses and is warrantably invoked only when such alternatives have been excluded as feasible possibilities. As noted, "necessity"—in the sense of "no alternative"—is the key to last-resort rationales. A response is depicted as a last resort only when it is made to stand as the only course of action available, as an outcome that "cannot be otherwise" (McHugh 1970). In a last-resort situation, then, the impending response does not simply rank at the top of the list of possible options; it is the sole course of action remaining open.

NORMAL REMEDIES AND LAST-RESORT RESPONSES

Demonstrations that there is no available course of action except a last-resort sanction proceed against the backdrop of some institutionally honored set of *normal remedies*. "Normal remedies" refer to the routinely considered, conventionally applied responses to the "normal cases" or normal troubles (Sudnow 1965; Cavan 1966; Daniels 1970) regularly encountered in and processed by a particular control institution. Within any given control setting, both the kinds of responses appropriately employed under

given circumstances and the sequence in which they are properly used become conventionalized. As a result, (*a*) certain routine responses are properly employed with particular sorts of cases, and (*b*) for any particular case managed over time, responses are properly invoked in a specified order, with those regarded as milder preceding the more severe.[6] For example, in juvenile court, the sequence of normal remedies includes: informal probation without a finding of delinquency, routine probationary supervision, more intensive probationary supervision, probation while on a suspended sentence, and incarceration. Or, as a parole agent described his normal remedies leading up to the last resort of a parole revocation:

> "In an average case, I'd start by giving the client a friendly warning. If that didn't work, I'd write an official warning. If the official warning didn't work, I'd try to place the client in a special caseload where he could receive individualized treatment. If there were no individualized treatment available, I might request a warrant. Then I'd try to reason with the client, and if we could work something out, I'd drop the warrant. If not, I'd still get one more chance to work something out at the hearing. One possibility might be an order that increased my revocation powers. Now that's about half a dozen steps, right? It's a six-step escalation that I can stop at any time." [McCleary 1978, p. 138][7]

To invoke a last-resort sanction successfully, it must be shown that normal remedial responses either (*a*) are specifically inappropriate, or (*b*) have been tried and have failed to contain the trouble. There are, then, two distinct procedures for warranting the use of last-resort sanctions corresponding to these alternatives: the *inappropriateness* of normal remedies is established by highlighting the seriousness of the offense/behavior of the alleged culprit; the *failure/exhaustion* of normal remedies is established by constructing a history of prior remedies showing that no course of action but the last-resort sanction remains viable.

[6] This analysis draws heavily, here and elsewhere, on Goffman's treatment of "corrective cycles" and the contingencies of escalating from informal to formal social controls (1971, pp. 345–62). Note further that my unit of analysis is the institution. Related issues arise at the level of institutional systems: Hawkins and Tiedeman (1975, p. 232), for example, discuss the function of prisons and mental hospitals as "final agencies" in a "sequential system" of social control. Yet even such last-resort institutions may have, and may need to have, their own further last resorts—perhaps isolation cells and electroshock therapy in the instances above.

[7] In other control settings, the appropriate use of lesser remedies may not be clearly sequenced. Thus, while behavioral modification programs may hold that "painful, aversive modifiers should be used sparingly and only as a last resort" (*Los Angeles Times,* Dec. 3, 1979), positive reinforcers need not be employed in a prescribed order. Similarly, a surgeon who recommends disc surgery as a last resort held that such surgery "should be done only after bed rest, an appropriate analgesia, physical therapy, exercise and weight loss if needed has been tried for a number of weeks and failed" (*Los Angeles Times,* Sept. 1, 1978). In both cases, a specifiable range of remedies should be tried before turning to the last resort, even if the specific order in which these remedies should be employed remains open.

Last-resort decisions of the first type characteristically appear with "serious cases." Here the heinous nature of the offense and the culpability of the offender, or the severely disturbed behavior of the actor, justify—indeed, require—the use of the severe, last-resort response. Procedures that accentuate the seriously deviant quality of the act (e.g., McHugh 1970) and/or the essentially pathological character of the actor (e.g., Garfinkel 1956; Emerson 1969) provide this sort of warrant. Under these circumstances, control agents often talk as if they have little or no discretion in the matter, as if the seriousness of the act/actor unequivocally requires a severe, last-resort response.

In contrast, last-resort decisions of the second type typically involve more ambiguous and minor (in the local scheme of social types) troubles, particularly those with recognized discretionary aspects. In these situations, it is not sufficient to show that sanctions *could* legitimately be invoked (e.g., that an offender did violate the law and hence could be incarcerated, or that a disturbed person is obviously a danger to self and others and could legally be hospitalized). It is also necessary to show that such actions *need* to be taken, where need is demonstrated less by the facts of the present incident than by the lack of any other way of dealing with an incorrigible trouble.[8] Hence last-resort justifications of this sort focus on establishing a case's distinctive "remedial horizon": the failure of prior remedies to manage the matter satisfactorily identifies the situation as one for which an impending last-resort response is the only one available.

Several empirical examples illustrate these two procedures. In earlier research (Emerson 1969, 1974), I observed a juvenile court whose staff was reluctant to commit delinquent youths to state reform schools except under the most extreme circumstances. Assuming that these institutions served purely custodial and restraining functions, court personnel preferred to seek out alternatives to incarceration. Incarceration was in fact invoked under the following circumstances:

a) When the offense committed was extremely serious—prototypically, a major felony attracting the attention of law enforcement circles and the local press. Such an offense put the court in the position of "having to do something," at least to take the offender "off the streets." Thus the seriousness of the offense and the inferred dangerousness of the offender eliminated as reasonable or proper alternatives any lesser responses than incarceration.

[8] Similarly, Bittner argues that the problem confronting the patrolman on skid row "is not which drunks, beggars, or disturbers of the peace should be arrested and which can be let go as exceptions to the rule. Rather, the problem is whether, when someone 'needs' to be arrested, he should be charged with drunkenness, begging, or disturbing the peace" (1967, p. 710). Bittner also emphasizes that this discretionary reluctance to arrest holds only for minor offenses: with the two major offenses he observed, patrolmen did not weigh remedial alternatives but arrested immediately and without question.

b) When a delinquent had moved through the standardized court procedures for youthful troublemakers until only incarceration remained as a viable alternative. While committing major offenses might speed up this progression, the exhaustion of alternatives could proceed independently of such behavior. In one case, court staff talked about their decision to commit a youth to reform school in the following terms:

> Both judge and probation officer emphasized how much the mother was to blame for the boy's behavior, feeling that she was very "disturbed" and "disoriented." But he was disturbed also, and hence could not be placed in the same home with his mother. In addition, the mother was insisting that the court get rid of the boy. Originally the court had worked out a placement for the boy at the Roberts School, but he had run away from there and they could not get him back in. In the face of this situation the court felt compelled to commit the boy to the YCA [Youth Correction Authority and reform school]. As the probation officer stated: "There's nothing else we can do." The boy could not stay at home and had messed up the court's effort to place him. Under these circumstances the only thing they could do was send him to the Youth Correction Authority, even though they did not feel the boy was potentially or actually criminal. [Emerson 1969, pp. 97–98]

Here, a last resort is advocated not on grounds of a youth's offense, or on grounds of his delinquent potential, but with the claim that no alternative course of action was available to the court. In the words of the probation officer, "There's nothing else we can do."

Similar concerns appeared in the way psychiatric emergency team (PET) members concluded that involuntary mental hospital commitment was required (Emerson and Pollner 1976*a*, 1976*b*). Under the influence of community mental health ideology, PET workers viewed the mental hospital as a last-resort measure to be avoided if at all possible. Yet people exhibiting extreme disturbance, particularly with potential for dangerous behavior, required hospitalization, not because such action would cure or even help them, but because their severe disturbance precluded the normal remedies of outpatient therapy and board-and-care-home placement. In less disturbed cases, however, the lack of any viable alternative loomed larger than the patient's mental condition in the decision to hospitalize. In several instances, PET workers hospitalized patients whom they characterized as "not particularly crazy" but whose living or interpersonal circumstances demanded major action. The plaint, What else can we do? often accompanied such commitments.

Sociological analyses have considered offense/offender-based procedures that would justify the use of severe sanctions. Garfinkel's analysis of the process of status degradation (1956) provides the most notable example. In this paper, Garfinkel analyzes the "communicative work whereby the public identity of an actor is transformed into something looked on as lower

in the local scheme of social types" (p. 420). He identifies eight structural requirements (pp. 422–23) for successfully redefining an actor as essentially "strange," outside the normal moral order. Clearly, those whose total identity has been so transformed and discredited—for example, "murderers" or "spies"—become appropriate objects for extreme, last-resort sanctioning. These identity transformations effectively wash away the normal compunctions we might feel about such drastic measures as execution. Finally, in identifying the conditions necessary for successful identity transformation, Garfinkel also shows how it is possible "to render denunciation useless" (p. 424): defeating or undermining any of the required conditions will prevent transformation of total identity, thereby making unmitigated into understandable or even excusable wrongdoing.

In a somewhat similar vein, McHugh (1970) analyzes the qualities that lead us to attribute deviance to acts. He argues that when we view an actor's misconduct as the result of choice (i.e., as "behavior which . . . might not have been or might have been otherwise") and knowledge (i.e., the actor is seen "to have known what he was doing") (p. 170), we thereby attribute deviance to acts and responsibility to actors and perceive them as meriting severe sanctioning.

As noted, last resorts may be justified not only through procedures that transform an actor's total identity, or that link that identity to a serious offense, but also in less dramatic ways, through procedures establishing that all normal remedies have been exhausted. The remainder of the paper treats this second, less appreciated procedure for warranting last-resort measures.

PROCEDURES FOR WARRANTING THE EXHAUSTED-REMEDIES CATEGORY OF LAST RESORTS

In more routine troubles, last resorts are legitimated by showing that the entire range of normal remedies for cases of this sort has been exhausted, leaving "no alternative" but to turn to this last, most extreme measure. To invoke successfully a last-resort remedy in this way, one must specifically establish that (1) the locally recognized set of normal remedies has been properly employed; (2) these normal remedies have failed, and in ways directly attributable to the actor; and (3) some further more severe response to the case is necessary; concretely, that it is impossible either to reimpose some previously used normal remedy, to invoke any special remedy, or simply to "do nothing" in the matter.

I argue that these properties are *sanctioned features* of successful last-resort requests. That is, these three features will regularly be demonstrated by those requesting last-resort sanctions, and when these features are not established, the request will fail. As with the conditions for successful denunciation, these features also show where last-resort requests are vulnerable to defeat.

1. Documenting the Prior Use of Appropriate Normal Remedies

Where there is no major offense, requests for a last resort require demonstration that all those responses that should have been applied in cases of "this kind" have in fact been applied. In juvenile court, a probation officer who wants a delinquent youth incarcerated must show that normal court practices for managing probationers have been conscientiously tried. Thus the committable delinquent's court biography is constructed to show that he or she has run the gamut of normal probationary controls, up to and including the most severe measure—intensive probationary supervision while on a suspended sentence.[9] Furthermore, if a probation officer wants to incarcerate a probationer not yet on a suspended sentence, questions will arise about the appropriateness of that request. Unless something further can be provided (e.g., evidence that there was a new, especially serious offense that would warrant jumping the usual remedial increment), the request will be seen as inappropriate. Thus, one can defeat last-resort requests by establishing that not all normal remedies, and in particular not the ultimate normal remedy, have been tried.[10]

Similarly, in junior high school discipline proceedings,[11] teachers' referrals from the classroom are routinely evaluated for appropriateness, for deans feel that at least some teachers send students from the classroom for the mildest sorts of misconduct. The "mildness" of misconduct, however, is determined by discovering how the teacher could (and should) have remedied the problem without having made the referral:

> Carlos, a Chicano boy frequently in the counselors' offices, came into the office early in the third period with a discipline slip stating that he had no pencil and had been disturbing the class. Counselor asked what he had done to disturb the class. Carlos said he had no pencil and had been told to leave. Counselor: "You didn't make any fuss about leaving?" Carlos

[9] Again, this demonstration is "easier" if the probationer has committed a relatively serious offense. Roughly speaking, a violent offense would "jump" the offender a step or two along in the normal sequence of remedies.

[10] Calls to a suicide prevention center (SPC) are processed under similar constraints, as analyzed by Sacks (1967). He found that SPC staff established callers' status as legitimate recipients of the agency's service by determining that they had "no one to turn to." This requirement made callers accountable for showing that none of the significant others to whom people usually take deep problems were actually available (or, if such a role incumbent did exist, that some specific feature of the current trouble eliminated that person). Thus SPC staff assumed that role relations constituted a set of normal remedial possibilities for personal problems. Furthermore, they assumed that a really suicidal person would not call a "stranger" about such problems unless normal resources were either nonexistent or exhausted. The legitimacy of any particular call would be in question unless it could be shown that indeed the caller lacked, or could not appeal to, these normal remedies.

[11] These observations are taken from a participant-observation study of discipline procedures in two California junior high schools in 1969. The Russell Sage Foundation and the Center for the Study of Law and Society supported the research.

> denied that he had. Then she asked him: "Can you go back without disturbing the class?" Carlos made no comment, but counselor sent him back to the class for the rest of the period.
>
> After the boy had left, counselor explained to me that it was unusual to send a kid back to class like that. But this teacher sent anyone without a book or pencil to the counselor. "I think he can handle that pencil business as well as I can." But generally: "This is a cardinal rule: you don't send a kid back when he comes in with a pink slip."

In this case, the counselor justifies giving what she acknowledges as exceptional treatment to this routine discipline matter by reference to an unused normal classroom remedy.

To request some further sanction without having attempted prior remedies not only invites denial of that request as "premature," but also brings into question the diligence and/or competence of the requester. Referrals for matters that could well have been handled by lesser remedies led to assessments of teacher unwillingness or inability to control their classrooms competently:

> A teacher sent a seventh grade boy to the counselor for "goofing off"—"drawing attention" to a dirty word someone had written on the board. Although the counselor held the boy out of class for the rest of the period, he complained bitterly about having to do so and about the teacher's low competence in the classroom: ". . . And why can't you just tell him, 'Okay, we'll erase this. Now knock it off! I don't want to see any more about this.' Isn't that enough?"

Indeed, one of the major aspects of the work of socializing newcomers is teaching just what types of remedies are appropriate in particular circumstances, and just what remedies should be tried before seeking a last-resort measure.

The requests of parties outside the setting for some last-resort sanction frequently dramatize the locally enforced and sanctioned character of specific normal remedies. Such requests are likely to be turned down on grounds that a normal alternative to the last resort still remains. Requests by private social agencies to incarcerate delinquent youths, for example, often appeared premature to the juvenile court, as under the court's (but not the agency's) conceptions of normal remedies, standard responses remained untried (see Emerson 1969, p. 72). Similarly, families might request hospitalization of a disturbed member with the claim that they were at the end of their rope and had exhausted all their remedial possibilities. Yet PET workers might evaluate these requests in the light of their own conception of the normal remedies appropriately employed before turning to hospitalization and come to divert such requests by locating untried but still feasible options.

Control agents or others who are familiar with local normal remedies

possess a further resource for obtaining last resorts: they can shape present actions to produce a record that will document at some future point the adequate, conscientious provision of just that sequence of normal remedies that should have been tried before turning to the last resort. For example, in juvenile court, officials occasionally decided that youths were "gone," that is, irrevocably committed to delinquent activities making progression into serious criminal pursuits inevitable. Such delinquents usually were not immediately incarcerated, but were kept "on the streets" under close surveillance for new incidents of trouble. The probation staff expected such incidents to occur soon enough and thus to justify an increase in the restrictiveness of court response. Eventually, it was anticipated, a record showing the diligent use and failure of every court remedial resource would accumulate, enabling the probation officer to argue easily and successfully for incarceration.[12]

The work of parole agents often comes to trade heavily on forms of record keeping that are prospectively oriented toward having to justify requests for warrants or revocation petitions. McCleary (1975), for example, has shown how parole officers are particularly careful in their dealings with "marginal" parolees (those who might possibly make it on parole, but who are hardly sure bets) to create a record that can be used to demonstrate repeated, conscientious, remedial efforts. In the words of one officer (p. 216): "Say like you're doing up an interview with a marginal man. You want to tell him certain things for the record. Well, my supervisor knows what those things are, so I get him to help me write up my reports. Then when the marginal man screws up, my reports make it look like I busted my ass for him." In this way, competent social control work often requires orienting toward a future accounting of the proper use of normal remedies.

2. Documenting the Failure of Normal Remedies

A second condition necessary for successfully invoking the exhausted-remedies category of last-resort sanctions is demonstration of the *failure* of those normal remedies that have been applied. This demonstration is routinely

[12] Similar processes appear in junior high school responses to students regarded as chronic troublemakers. Since permanent expulsion required a prior 20 days of suspension, students the school wanted to expel would draw suspensions for behavior which if committed by the less troublesome would lead only to reprimand, detention, or at most a call to parents. Thus records showing the recurrent use of normal remedies were constructed prospectively. Wittig (1976, p. 201) notes an additional use of the records so produced—to discredit parents contesting expulsion efforts: "In extreme situations, such as attempted expulsion from school, the parent's legal position is weakened because school records contain information on efforts to 'help parents and child' which were refused." For a more general analysis of retrospective-prospective concerns in the construction of records, see Cicourel (1968, pp. 162–69).

made by pointing to the recurrence of the trouble or, even better, to its acceleration, despite the use of appropriate normal remedies (see Emerson and Messinger 1977, pp. 122–23). In juvenile court, for example, leaving a youth "on the streets" under ordinary probation supervision became a documentably ineffective control measure only when the youth got into trouble again. Usually such "new trouble" required actual apprehension for another delinquent offense; it was not ordinarily sufficient to show that the probationer was simply uncooperative with the probation officer and resistant to "supervision."

In other settings as well, control agents point to the persistence of the trouble in order to establish the failure of a current remedy to manage the problem. This procedure underlay the defense of electric shocks as negative reinforcement at Pacific State Hospital noted earlier. As the *Los Angeles Times* (Feb. 8, 1976) quoted the medical director of that hospital: "The basic emphasis should always be the development of more normal behavior in a resident . . . and the resident's record must reflect the fact that less restrictive methods of modifying or replacing the behavior have been systematically tried and *have been demonstrated to be ineffective*" (emphasis added).

A similar concern with documenting the failure of normal remedies in order to justifiably invoke a last-resort sanction is apparent in the counselor's response to a junior high school discipline referral which read: "Does not want to work; demands too much attention":

> "Well, this one yesterday, I went to the teacher and talked to her. She explained what the situation was. *It was a continuing thing.* He started beginning of the period *one thing and then he went to something else. She took it up three or four times.* This is why it demands too much attention. . . . I told her I would hold Philip out of class for the rest of the week." [Emphasis added]

Here the initial referral does not allude to the teacher's prior remedial efforts. Without some such indication, the counselor is uncertain about the appropriateness of the referral. After making a special inquiry into the situation, he comes away convinced that there is a problem requiring more than in-the-classroom response. The decision is made by detecting that the teacher's persistent efforts fail to remedy the problem.

Under some circumstances, defining which behaviors constitute "new trouble," and hence evidence for the failure of a particular remedy, may involve a radical retrospective reconstruction of how well or poorly particular remedies are working. Irwin provides a striking example from parole supervision (1970, p. 172):

> When the parolee, who has been given verbal approval to drive but was warned that the agent could not "back him up" is involved in an accident,

> the agent may write a violation report for driving without permission. When an incident such as this occurs and there is a possibility of closer scrutiny of the parolee's behavior by other members of the agency or the parole board, the agent often includes in his reports or consultations with superiors a great deal of behavior in violation of the conditions which he had implicitly or explicitly condoned and had banked for emergencies.

That is, behaviors that were not initially treated as trouble in the ongoing relationship between parole officer and parolee can come to be redefined as "more trouble," and hence as evidence of the failure of those initial remedies, when reexamination of the case becomes necessary.

Not just any new trouble, however, provides adequate grounds for claiming that a particular remedy has failed. If a new trouble occurs too soon after the implementation of the remedy, for example, it may be thought that the remedy was given insufficient time to provide a true test. Similarly, if the new trouble arises directly out of the effort to implement a new remedy, and represents a sort of trouble routinely anticipated from such effort, failure need not be inferred. Consider, for example, the following remarks by a probation officer asked if he intended to enforce a threat to haul back into court a probationer flagrantly "uncooperative" during an initial interview (Darrough 1975, p. 112): "Hell no! Of course I couldn't go to court. If I did, the judge'd probably be nice and polite to me until the kid was out of the room and then give *me* hell. I mean it would be a little ridiculous, a kid just on probation, no new charge, and here I am, 'Your honour, he won't talk nice to me' " (laughs). Here, the new trouble is too recent, too trivial, and too obviously implicating the control agent's effectiveness to establish that the attempted remedy had failed.

In general, for the recurrence of trouble to stand as evidence of the failure of remedies, it must appear that they have been applied competently and conscientiously. This conscientious effort forces the inference that features of the case make it resistant to anything short of last-resort treatment. The repeated failure of medical treatment thus indicates the irremedial nature of the symptom and provides justification for the last-resort measure. In other situations, establishing the resistant character of the case turns on establishing the actor's responsibility for the failure of the normal remedies. In juvenile court, for example, incarceration often requires highlighting the prior "chances" given the delinquent: that is, that the ultimate normal remedy (the suspended sentence) has been used not once but a number of times, with each subsequent use represented as an occasion where the delinquent could justifiably have been incarcerated but instead received "another chance." These "chances" are thus cited as evidence that normal remedies have not only been attempted, but have also been followed conscientiously and perhaps even too persistently, and that their failure

derives from the intransigence of the delinquent (Emerson 1969, pp. 137–38). Furthermore, probationary supervision is structured so that the recidivist probationer can be held accountable for having knowingly and with full responsibility offended again. Cicourel's analysis of probationary supervision (1968, pp. 130–64), for example, describes two routine procedures that produce this result. First, delinquents are regularly "lectured" about the future consequences of further misconduct. In the event of new trouble, this process allows the claim that the juvenile was apprised of the likely repercussions. Second, as part of this lecturing style, the probation officer lays out the proper, legitimate ways for the probationer to behave as if they were readily available and viable courses of action. Then, any further misconduct is attributable to knowledgeable choice on the part of the delinquent, since alternative courses of action existed.[13] Any claim that the delinquent did not know what he was doing in an organizationally relevant sense is thus made more difficult to sustain.

3. Documenting the Impossibility of Alternative Remedial Actions

Even if it has been shown that all normal remedies have been tried and have unequivocally failed under circumstances in which the actor can be deemed responsible, several further conditions may have to be met before last-resort sanctions will legitimately be granted. First, the control agent may have to show that it is not possible simply to reinstitute some prior normal remedy; that is, the agent must show that some escalation in response is necessary. Second, the control agent may have to discount the feasibility of any *special remedies* that come to be proposed or considered as an alternative to escalation to a last resort. That is, impending or requested last resorts are defeasible (see Hart 1965) by showing that a lesser or a special remedy is possible and preferable. And so the kinds of communicative work involved when this possibility arises are typically reactive, occurring in response to any attempts to counter or undermine sought-for last-resort sanctions.

In the first situation, those controlling access to last-resort sanctions may entertain the possibility of reemploying some prior normal remedy rather than invoking the dubiously valued last resort. In juvenile court, for example, a delinquent facing incarceration may ultimately be kept "on the streets" on probation precisely because court staff continue to see incarceration as an avoidable outcome. This action may be taken reluctantly and pessimistically (i.e., without believing that a response that has failed in

[13] My debt to McHugh's (1970) analysis of conventionality and theoreticity should be apparent here.

the past will work now) and is pervaded by a sense of "making do." A successful last-resort request will thus require some showing that it is now *necessary* to invoke that last-resort sanction.

At an extreme, "making do" may entail "doing nothing" in preference to turning to a last resort. Under these circumstances, to "do nothing" or to "let it go" are not so much negligent inaction as a considered preference for what is seen as the lesser evil. In fact, as the remedies considered grow more severe, "doing nothing" as a residual alternative becomes increasingly viable. In this sense, the progressive exhaustion of normal remedies tends to polarize remaining options between the extremes of the last resort and "letting it go." As an example, PET teams, especially at that point where they have become convinced of the exhaustion of normal remedies, will turn to hospitalization only upon concluding that it is impossible just to walk away from the trouble.

Last resorts, then, will be successfully invoked only when reuse of some normal remedy, including this latent alternative of "doing nothing," can be eliminated or shown to be inappropriate.[14] Highlighting the potential for violent or dangerous behavior provides an effective mechanism for so eliminating this less restrictive alternative. Parole officers, for example, try to ensure successful outcomes in their revocation hearings by "building a case" against the parolee. This process involves "creating a dossier that incorporates only incidents that are compatible with the qualities of 'dangerous,' revocable 'criminality' " (McCleary 1978, p. 144). The potential for dangerousness thus provides a need to invoke the last resort: if something less were done, serious harm is likely to result.

The same outcome can be attained in less dramatic ways as well. If it can be shown that the prior remedy is simply no longer available, it becomes impossible to do nothing. For example, when a client has been expelled from the apartment or board-and-care home he previously occupied, PET teams define the situation as one where "something has to be done"; the likelihood of turning to the last resort of mental hospitalization increases.

A second and perhaps more frequent response to the questionable use of an impending last resort, however, is to seek out some special or exceptional remedy for this particular case. Again, as a last resort grows imminent, responses may be proposed that stand outside the local set of normal remedies, responses that are thus recognized as special or exceptional. In juvenile court, for example, such special remedies usually involved an offer

[14] Consider, for example, one of the conditions specified by the American Friends Service Committee (1971) to safeguard the last-resort use of criminal sanctions to remedy social problems: "There is some substantial basis for assuming that the imposition of punishment will produce greater benefit for society than *simply doing nothing*" (p. 66; emphasis added).

to "sponsor" a youth in some special, one-time course of action that was intended to stave off impending incarceration. Such offers of sponsorship were most apt to arise from some party outside the court willing to make some special effort on behalf of the youth. Such effort might range from personally working with and supervising the youth to arranging and following through on some appropriate referral to another program.

McCleary's description of parole officers' uses of "special programs" for treating particular type of parolees (1978, pp. 141–45) reflects these same processes. As he notes, "the general attitude among PO's is that the special treatment programs are an *alternative* to reimprisonment." This means that when parole revocation is impending the violator who can be made to qualify for a special program is less likely to be reimprisoned than one who cannot.[15] As one parole officer explained, having noted that there were special programs for addicts but none for thieves, "If an addict and a thief are busted for petty theft, the thief's going back to prison. The addict will be transferred to a special program and continued on parole. He gets a second chance that he wouldn't have had before we started the special program" (p. 142).

In both juvenile court and parole situations, provision of such a special remedy can defeat the imposition of a last resort, undermining the claim that there is no alternative but to invoke this extreme sanction. Thus, successfully invoking a last resort requires defeating any proposed special remedies. This can be done in the following ways: (*a*) by establishing that the proposed special action has *in fact* previously been attempted, or that efforts have been made to attempt it, and these have failed; (*b*) by reviewing the history of prior remedial efforts, including the provision of a number of last chances, to demonstrate that what is being proposed now has *in effect* already been tried, and without success; (*c*) by undermining the credibility, competence, and/or motivation of the sponsor who wishes to implement the proposed course of action; and (*d*) by showing that the actor is so dangerous, uncontrollable, and otherwise unmanageable that this something special is too risky.

CONCLUDING REMARKS

By way of conclusion, I would like to explore some of the wider implications of the preceding analysis of last resorts.

First, locally sanctioned normal remedies are of critical significance in

[15] In this case, this preference reflects less the undesirability than the uncertainty and extra work of obtaining the last resort of revocation. As McCleary quotes one parole officer, "I don't believe that any of these special treatments are going to work, but what else can I do? I want to avoid revocations because they're messy and time consuming" (1978, pp. 142–43).

social control decision making involving last resorts. For the claim that one has no alternative but to turn to a last resort is essentially residual and hence tenuous in character. Such claims are then inherently susceptible to the counterargument that there are in principle other alternatives that have not been considered or tried. In fact, in the social control settings considered here, locally honored normal remedies provide a situationally restricted set of remedial actions to which someone seeking a last resort can appeal and be held answerable. Probation officers, for example, can gain access to last-resort sanctions not by showing that they have tried each and every possible remedy for saving a youth from serious delinquency, but by showing that they have conscientiously attempted those remedies that are normal and expected in that setting. Last resorts, then, are justified not by showing that there is literally no alternative, but by showing that there is no alternative "for all practical purposes" (Garfinkel 1967).

The difficulties encountered when private parties rather than officials advance last-resort arguments dramatically underscore the advantage inherent in a conventionalized set of normal remedies. Consider the following brief examples:

1. A family seeking the last-resort hospitalization of a disturbed member may not be able to convince hospital gatekeepers to credit their assertion that no alternative remains open to them. For the family's insistence that they have tried everything and are at the end of their rope can easily be countered by pointing to untried alternatives, or to the family's own part in generating the disturbance. In effect, the family cannot count on seeing their claims about what remedies they have undertaken, how conscientiously and for how long a time, honored by screening officials.

2. Jacobs (1970, p. 234) summarizes materials from suicide notes suggesting that suicide tends to be depicted and justified as a last-resort action: "In brief, the potential suicide feels that necessity of taking his own life because, from his perspective, he has attempted (to no avail) every other possible alternative available to him for dealing with his problems. Death is seen as the only answer, the only possible resolution to the unbearable and insoluble problems of life."

Yet those who make such last-resort claims, it should be clear, cannot count on others to honor either their assertions about the lack of other remedial options or their versions of their practical circumstances affecting those options. In contrast, those making last-resort arguments in social control settings can rely not only on some shared notion of normal remedies but also on some honoring of their own versions of their practical circumstances and attendant remedial possibilities. Teachers, for example, can count on deans' sympathy about the need for classroom order, maintenance of respect for the teacher's authority, and the need to make an example of

those who engage in particularly flagrant forms of misconduct. Personally (as opposed to institutionally) claimed last resorts are thus considerably less likely to receive acceptance and legitimation.

Second, in this analysis I have sought to emphasize how last resorts provide a justificatory decision logic. Specifically, when control agents contend that they use certain extreme sanctions "only as a last resort," they are offering not so much a factual description of their own procedures as an *account* (Garfinkel 1967) demonstrating (or at least asserting) the rational, necessary character of those decisions. Thus, last resorts are socially constructed outcomes; that is, they are *accomplished* by use of the procedures and practices discussed above.[16] Yet various sociological analyses have accorded last resorts a factual rather than an accomplished, justificatory character. Gove (1970, p. 879), for example, ascribes last-resort qualities to mental hospitalization, contending that typically in the event of hospitalization "it is only when the situation becomes untenable that action is taken."

The problems with such an approach are twofold: In the first place, when last-resort decisions are accorded factual status, the analyst implicitly or explicitly credits the decision makers' claims that these decisions are indeed "rational," "necessary," and hence justified. In contrast, the approach taken here allows for a more critical stance toward these decisions, thus allowing examination of the grounds for the decisions rather than accepting these grounds as givens.[17]

In the second place, to conceptualize last resorts as facts rather than as achievements obscures some critical social processes. Pursuing Gove's concern, consider the analysis of the oft-noted "toleration of mental illness."

[16] It is critical to emphasize that control agents themselves do not necessarily experience the use of last resorts as their own accomplishment, as a matter of subjective or definitional preference. Clearly, in some situations control agents treat last resorts cynically and manipulatively and are obviously aware of going through the motions in order to create the situation in which all alternatives are exhausted. But more commonly in my experience, the use of last resorts confronts such agents as an objective requirement determined by factors beyond their control and choice, and it is accompanied by a deep sense of closure and frustration at the evaporation of any possible alternative. But whether cynical or sincere, the nature of the control agent's attitude toward a last resort is not relevant to the issues of the structure of last-resort decisions addressed here.

[17] While this analysis has focused on social control settings, these issues arise wherever last-resort justifications are advanced. For example, both individual and collective physical violence typically generate last-resort claims: Driver (1961, p. 157) notes that homicides in India seem to occur in situations where all normal means for redressing interpersonal grievances have failed; a Dutch military attack that led to the deaths of two of 55 hostages held by South Moluccan terrorists was headlined a "last resort" (*Los Angeles Times,* June 12, 1977); and Melvin Pollner has suggested in personal conversation that wars are often presented and justified in last-resort terms. The ways in which such claims are advanced and sustained, and the ways in which particular sorts of actions are delineated as feasible to particular audiences, would prove fruitful areas for investigation.

"Toleration" is identified in such tendencies as, for example, families' willingness to live with even severely disturbed behavior by one of their members for considerable lengths of time, relying almost entirely upon intrafamilial remedies (e.g., a variety of "accommodation practices"). Under these circumstances, hospitalization occurs when tolerance collapses, when the "symptoms . . . become impossible to handle in the community" (Gove 1970, p. 877). Such studies then focus on the conditions under which symptoms become impossible to handle, in this way assuming that the failure of normal remedies occurs prior to, and independent of, the request to turn to mental hospitalization.

This paper has suggested a further possibility, namely, that precisely in order to justify their decision to turn to the hospital, family members will point to their "record" of having "tolerated," "normalized," or "accommodated" severely disordered behavior with the remedial resources at their disposal. Showing that symptoms have become unmanageable with the responses available to the family establishes both the necessity for hospitalization and the good-faith efforts of the family in having tried to avoid it. Empirically, attention is thereby directed to how families at the point of hospitalizing a member provide accounts graphically showing how much they have to put up with, how many times they have turned to lesser remedial alternatives, how present circumstances overtax their patience and accommodative skills. Conceptually, such an approach treats "impossible to handle" symptoms not as factual (and causal) descriptions but as accounts providing for the inadequacy of prior home remedies.

Finally, throughout this paper I have sought to highlight the fundamentally practical processes in social control decision making, processes of constructing and choosing between alternative courses of actions. The concepts of normal remedies and last resorts direct attention to locally specified ranges of practical courses of action and to the processes whereby certain alternatives are rendered acceptable and unacceptable in specific cases. What has been done previously about a case is not (or need not be) of incidental or accidental concern to social control agents. Instead, it provides a key resource for establishing what is now to be done. That is, cases or troubles are organizationally understood by reference to the efforts that have been made to remedy them. Any current definition of a trouble will tend to draw upon some understanding (often reconstructed) of what has been done to manage it in the past. This approach contrasts sharply with those that assume that decisions are directly determined by characteristics of the offense or the offender: it is not offense and offender per se, but how offense and offender articulate with prior and prospective remedies that provides decision makers with distinctive grounds for choosing or recommending particular outcomes.

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Competing Models of Political Mobilization: The Role of Ethnic Ties[1]

Eric M. Leifer
Harvard University

Two models of political mobilization are examined. The developmental model stresses class-based dynamics and does not take the group formation behind mobilization to be problematic. The reactive ethnicity model concurs with the developmental model on the importance of class-based dynamics but contends that ethnic ties serve as facilitating conditions for mobilization in a cultural division of labor. A crucial test, one that differs from past efforts, is formulated and applied to British historical data. The test avoids the problem of overlapping class and ethnic interests in a cultural division of labor by treating ethnic ties as facilitating conditions, and not as interests. The resulting contextual formulation allows an unambiguous use of aggregate data and renders the problem of selecting an "outlet" (party) for ethnic interests irrelevant. Results show ethnic ties to be important, but their conjunction with economic disadvantage was not sufficient to trigger concerted political mobilization. A missing necessary component is hypothesized—a leadership component—that would allow more accurate prediction of the strength and direction of the means adopted to resist collective oppression (voting in national elections being one such means).

Any development must also entail the destruction of old forms. Among the victims of economic development, social theorists have long ranked ethnic groups high on the list. Economic development means an increase in the scope and centrality of economic markets. This expansion of economic markets promotes greater exchange and communication among diverse ethnic groups and diffuses a common array of goods and services, information, and opportunities. It was thought that similarities in the conditions under which diverse ethnic groups live and interact would undermine their tradition-based ethnic attachments, but the effect of these superimposed commonalities has by no means proved as clear as was once predicted. Ethnic attachments have persisted in many highly developed countries, often producing serious obstacles to national integration. The problem

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addressed in this paper is: Given the progress of economic development, when will ethnic attachments persist, and what role will they play in political mobilization?

The literature offers two perspectives[2] (or "models," when predictions are at issue) that address this problem: the developmental and the reactive ethnicity perspectives. Both will be examined critically and subjected to a "crucial" test. Attempts to test the predictions each makes are numerous in the literature (see Hechter 1975; McRoberts 1979; Nielsen 1980; Paige 1978; Palloni 1979; Ragin 1977, 1979; Ritter 1979). Many versions of the two perspectives, and many test formulations, have emerged from these efforts, and I will try to simplify the complexities that have arisen. Particularly, I will try to undo the confusion created by Hechter's use of Weber's distinction between class and status (or here, ethnic) interests. This distinction was not part of Hechter's (1975) theoretical exposition but was introduced to help justify his use of residuals from regression analyses (see his chap. 7). Subsequent critics, particularly Ragin (1977), have rejected Hechter's test formulation but have let the distinction between class and ethnic interests become entangled in their theoretical expositions. This has made it very difficult to avoid the problems of Hechter's test formulation. In order to break this vicious circle, I have resurrected Hechter's insightful theoretical exposition and formulated a test true to its intentions that does not involve a distinction between class and ethnic interests. The theoretical exposition is given below and the test formulation follows. The central theme in both sections is the status accorded ethnicity in political mobilization, as it is on this dimension that the two models diverge.

THE THEORETICAL ALTERNATIVES

Within the present range of concerns, the developmental perspective stands at an important extreme—it treats the persistence of ethnic attachments as a "residual" phenomenon (Parsons and Smelser 1956; Lipset and Rokkan 1967; Butler and Stokes 1969). In this perspective, extensions in the scope and centrality of the market unequivocally undermine ethnic attachments. With the extension of the market, the dominant orientation becomes one of economic rationality. Ethnic attachments recede as the universalistic (achievement) criteria that demarcate this rationality replace the previously dominant particularistic (ascriptive) criteria. Since ethnic identities have no role in the mechanics of the market, they should lose their meaning in the orientation of individuals. Thus the developmental model predicts that ethnic attachments will diminish with the progress of economic development. Empirically, ethnic attachments that do exist in developed countries are thought to be sentiments that have so far escaped the inevitable conse-

[2] A recent third alternative, the "ethnic competition" theory, is not treated here (see n. 5).

quences of economic development. Since the process of development is never completely uniform, ethnic attachments that vary inversely with the regional level of economic development are not necessarily anomalous (i.e., when the ethnic group is isolated from the processes of development).

This "residual" treatment of ethnic attachments means that they cannot be given an independent role in the dynamics of political mobilization in economically developed regions. To do so would undermine the status given economic rationality and attendant class considerations. It is in this light that attempts by developmentalists to deal with persisting ethnic attachments must be viewed. For instance, Smelser (1969) argues that ethnic attachments are most likely to persist when ethnic boundaries and occupational cleavages coincide (what will shortly be called a cultural division of labor). This special case may reconcile ethnic attachments with the developmental framework, but it is crucial for the latter to deny an independent role to the former in the dynamics that result. These dynamics must be based on the occupational (i.e., class) cleavages, while the ethnic cleavages must serve only as a "facade" for the more fundamental class-based mechanics. That is, ethnic attachments will appear as the basis for political conflict only because they overlap with the actual class basis. Smelser goes beyond this position in claiming that "the important structural feature of such an arrangement is that economic, political and racial-ethnic membership *coincide* with each other. Thus, any kind of conflict is likely to assume racial overtones and arouse the more diffuse loyalties and prejudices of the warring parties" (quoted by Nielsen 1980, p. 78). The second sentence suggests a greater role for ethnicity than can be admitted within the developmental framework. In fact, the role suggested corresponds to the role given ethnicity within the reactive ethnicity perspective. When ethnic attachments are given an independent role in intensifying conflict, they are no longer "residual" to the theoretical framework.

The developmental perspective has come under much attack in the past decade. Its detractors claim that the residual status it gives ethnicity is unable to account for the profusion of "ethno-regionalist" movements in recent times (e.g., movements in Quebec, Catalonia, the Basque regions of Spain, Brittany, Eritrea, the Shaba province, and, of most concern here, the "Celtic fringe" in highly developed Britain [see Hechter 1979]). The persistence of ethnic attachments in these cases cannot be explained by geographical or social isolation, as would be required by the developmental perspective. In critical reference to this perspective, Barth (1969, p. 10) was one of the first to note that "ethnic distinctions do not depend on an absence of social interaction and acceptance, but are quite to the contrary often the very foundations on which embracing social systems are built. . . . Cultural differences can persist despite inter-ethnic contact and interdependence."

Ethnic attachments can persist in even the most developed countries and can function as an independent source of political malintegration. Thus many theorists have come to view the diminution of ethnic attachments as problematic, even when considerable economic development has occurred.

These considerations motivated the reactive ethnicity perspective, in which ethnic ties are given an independent role in political mobilization, in contrast to their residual status in the developmental perspective (Hechter 1975). The reactive ethnicity perspective contends that developmentalists have misunderstood both the process of economic development and its effects. Economic development does not necessarily entail decreasing salience of ethnic distinctions in the mechanics of the market (especially the labor market). In fact, it can produce the opposite: ethnic groups can be subjected to an "internal colonialism" in their subjugation to a "core" (Hechter 1975; Frank 1967; Wallerstein 1974). Here, the maintenance of ethnic distinctions in the stratification system is not viewed as a temporary aberration from universalistic norms, as developmentalists might claim, but as a systematic arrangement between core and periphery.[3] Hechter calls this arrangement a "cultural division of labor" (CDL), which he defines as a "system of stratification where objective cultural distinctions are superimposed upon class lines" (1975, p. 30). Given that a CDL exists, the persistence of ethnic attachments no longer seems anomalous (as Smelser recognized). If the stratification system links ethnic identity with economic status, it confers a meaning to the identity that the ethnic can hardly ignore. Ethnic identity cannot be detached from one's economic and political interests within the system. Thus it should not be expected that ethnic attachments will recede in highly developed countries, if it is understood that economic development can produce a CDL.

This claim does not itself distinguish the reactive ethnicity perspective from the developmental perspective. Within the developmental framework there is no reason to expect ethnic attachments to recede in a CDL (or pattern of coinciding cleavages), should one exist.[4] What is needed to distinguish the reactive ethnicity perspective is an independent role for

[3] See Nielsen (1980, p. 78) for this distinction in the basic "historical scenarios" of the two perspectives. Nielsen argues that their historical scenarios and the effects they attribute to a cultural division of labor (CDL) must be dealt with separately. For purposes of discourse I have accepted this separation, but it must be kept in mind that the two are very related.

[4] On the surface, a CDL is anomalous within the developmental framework, since ethnic distinctions must be irrelevant within the stratification system. There are some ways around this, however. A CDL could represent a chance fluctuation in the distribution of status positions among ethnic identities—a temporary (or statistical) aberration. Or a genetic argument could be used to rationalize the inferior position of certain ethnic groups—the stratification system is using universalistic norms in its selection processes, only the ethnic group fares poorly on these norms. Thus the link between economic and ethnic position would be spurious. In either case, a CDL would be congruent with the developmental framework, which is the only point I wish to make here.

these ethnic attachments in political mobilization. In the literature, this role has been based on Hechter's (1975) use of Weber's distinction between class and ethnic interests (see Weber 1968, pp. 926–38). These supposedly distinct interests are used as the basis of political mobilization in the two perspectives. It is straightforward that political mobilization revolves around class interests in the developmental perspective. In the reactive ethnicity perspective, ethnic identities will be perceived as the basis for a *collective* oppression when a CDL exists. The resulting salience of ethnicity will produce a range of interests tied directly to the ethnic identity, such as the maintenance of bilingual education or more extreme "separatism." Thus the effects of a CDL will include mobilization around specifically ethnic interests, contrary to the predictions of the developmental model.

The residual analysis used by Hechter (1975) is based on this distinction between class and ethnic interests. The proportion of votes for a party (by county) was regressed on class-related variables, and the residuals were interpreted as the component of mobilization due to ethnic factors. In this interpretation, Hechter (1975, p. 215) made the necessary technical assumption that class and ethnic factors in political mobilization were unrelated. This technical assumption, of course, was based on Weber's theoretical distinction between class and ethnic interests. Yet the independence of class and ethnic interests seems incongruent with the conditions of a CDL, for these must overlap by definition. Ethnic groups largely define class groups in a CDL, and "ethnic" interests have direct implications for class relations, and vice versa. The developmental claim that the "ethnic" component in mobilization is merely a "facade" for the pursuit of class interests cannot be easily rejected. In fact, Hechter does not appear to reject this claim when he asserts that ethnic solidarity is a political response of an ethnic group seeking to alter its position in the social structure. Can political action, stripped of its rhetoric, be anything else? One need only try to disentangle class and ethnic interests in a concrete CDL, such as Quebec or Wales, to see how futile this effort can be.

Hechter (1976) has defended his use of the distinction between class and ethnic interests with the following illustration. The "successful black businessman" faces a dilemma at the polls in the CDL America. In casting a vote he must decide between his business interests (Republican party) and his minority interests (Democratic party). His decision will reflect the relative importance of his class and ethnic interests. Hechter lets his thesis rest on the prediction that the man will vote Democratic, which clearly is not what the developmental model would predict. At this individual level, the distinction between class and ethnic interests appears valid. However, two points must be made. First, the "success" of a black (subordinated ethnic) in a CDL is problematic by definition. If too many blacks are successful, there is no CDL. Thus Hechter is letting his model rest on the

behavior of the exceptions to the requisite conditions (i.e., a CDL) needed for his model to be applicable. Second, if we accept the importance of these "exceptions"—and this is plausible—the problem arises of isolating their behavior in the aggregate data available. Reliable inferences from aggregate data here are impossible. Ragin's (1977) claims, for instance, concerning the role of "local dominant strata" within the ethnic regions of Britain cannot be clearly supported by the aggregate data he used, despite his claims to that effect. These difficulties are the basis of my contention that, in this particular application, the distinction between class and ethnic interests is a fruitless one.

If both models agree that ethnic attachments may persist in a CDL and are not best distinguished by the interests pursued by subordinated ethnic groups, on what basis do the models give a different role to ethnicity in political mobilization? The real difference between the two models lies in the implications each attributes to ethnic attachments in the process of political mobilization. In the developmental perspective, ethnic attachments do not play an independent role in political mobilization. Because they overlap with class cleavages in a CDL, they will appear intertwined in political mobilization. Yet ethnic attachments lend nothing to this mobilization; it would proceed along the same (class) lines with the same force were there no CDL. This is an implication of the residual status accorded ethnicity in the developmental perspective.

In contrast, within the reactive ethnicity perspective the implications of ethnic attachments are crucial. Two components are necessary for political mobilization. First, individuals must share the same interests. Second, suitable conditions must exist for mobilization—conditions essential to group formation. The reactive ethnicity perspective is distinguished by the contention that the first component alone is insufficient for concerted political mobilization. A central insight in Hechter's (1975, p. 42) theoretical exposition is that "the aggregation of individual perceptions of economic inequality alone is insufficient for the development of collective solidarity." The developmental perspective errs, according to Hechter's framework, in not drawing the analytical distinction between the formation of individual grievances and the formation of groups around such grievances. The group formation involved in political mobilization is inherently problematic, as the recurrent failure of "class consciousness" to form in situations of economic oppression indicates. Besides conditions that generate common interests, conditions that facilitate group formation are also necessary. It is here that ethnicity plays a crucial role. Ethnic ties among economically disadvantaged individuals (i.e., a CDL) play an independent role as *facilitating conditions* for the group formation essential to political mobilization. Ethnic linkages are a facilitating condition because they define a common discourse (language, beliefs, and practices) which is required for

any form of concerted action. Thus in the reactive ethnicity perspective, ethnic attachments retain their salience through the workings of the stratification system and, in turn, serve as a condition which facilitates concerted efforts to alter the stratification system.

This simple logic has been obscured by the distinction between class and ethnic interests. For instance, Hechter (1975) sometimes implies that common ethnic interests promote political mobilization more readily than common class interests do. But it is not the interests themselves that facilitate political mobilization, it is the social context in which they are imbedded. When this context is distinguished by ethnic commonalities, the ability to pursue common interests, whatever they may be, will be greatly enhanced. The shift from *interests* to *conditions*, or context, as the center of attention thus offers to shed more light on the actual process of mobilization rather than on its attendant rhetoric. The advantages of making this shift will become even more evident in the test formulation section.

A schematic summary of the differences between the developmental and reactive ethnicity perspectives is contained in figure 1.[5] In the developmental model, economic development produces "functional" societies. Cleavages in functional societies are class based, and group formation around class interests is not taken to be problematic. Should a CDL exist, ethnic identities may persist, but these should have no significant role, separate from class interests, in the process of political mobilization. In contrast, the reactive ethnicity model takes group formation to be problematic. Where no CDL exists, both models concur that economic development will result in a functional society (although the reactive ethnicity perspective holds that group formation around class interests will be problematic). When a CDL exists, the reactive ethnicity model posits that ethnic attachments

[5] An "ethnic competition" perspective, rooted in ecological theory, has been developed by Hannan (1979), Ragin (1979), and Nielsen (1980). It is based on the notion that "groups" compete for resources, unless they are separated into distinct "niches." In application, groups are ethnic groups and niches are occupational statuses. These usages are at odds with both models in this paper; in them, significant competition occurs between status positions, and groups are classes in the developmental model and taken as problematic in the reactive ethnicity model. Based on these different meanings, ethnic competition predictions flipflop those of the reactive ethnicity model: the more rewards and social roles among ethnic groups are similar (i.e., no CDL exists), the more ethnic conflict should result (Nielsen 1980, p. 79). From the standpoint of both other models, it is not clear why ethnicity should remain salient in this situation. Why should ethnics unite across status positions to aid in their struggles *within* them? What becomes of class conflict? The treatment of ethnicity as fundamental—it remains meaningful even when all recognition of ethnic identity ceases in the stratification system—represents a quantum leap in the role of ethnic ties from the other two models. Though this puzzling (from a rationalistic standpoint) contention warrants consideration, I doubt that aggregate voting data is the best place to begin. In this respect, the inferences Ragin (1979) and Nielsen (1980) made in their data interpretation seem tenuous. Since I was unable to find a less ambiguous use of aggregate data for testing this model, and also was unable to compare it formally with the other models (owing to its much different framework), I have excluded it from the present test.

(made salient by the stratification system) will play an independent role in facilitating group formation around the pursuit of political interests. Thus an "interactive" society is predicted, in which the conjunction of economic and ethnic subordination is required to produce concerted political mobilization. Specific predictions can now be derived and tested.

Divergent Predictions of the Developmental and Reactive Ethnicity Models

The first step is to specify the conditions under which the two models differ. Many empirical researchers have ignored the fact that the models yield identical predictions in many settings and have produced some questionable applications as a result (see Ritter 1979 and Palloni 1979). Quite simply, only when economic development has occurred *and* a CDL exists do the two models differ in their predictions, as depicted in figure 2. The logic behind this restriction was explained above.

The next step is to address the problem of empirically isolating the effect of ethnic ties in political mobilization. If such an effect is found, the reactive

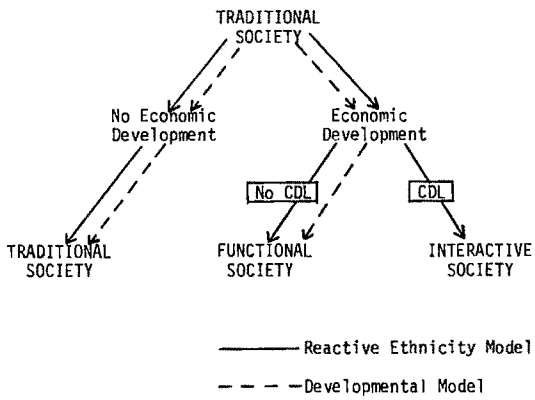


FIG. 1.—A schematic summary of the two models

Do the developmental and reactive ethnicity models make divergent predictions?

		Economic Development	
		Has occurred	Has not occurred
CDL	Exists	YES	NO
	Does not exist	NO	NO

FIG. 2.—Conditions under which the developmental and reactive ethnicity models diverge in their predictions.

ethnicity model must be considered an improvement over the developmental model. Isolating the effect of ethnic ties is, however, no simple matter, as the literature will testify. The problem lies in separating these ties from class linkages in a CDL, where ethnic and class boundaries overlap. If either ethnic or class ties are viewed separately, either will appear as the foundation for political mobilization (I hope!), which largely accounts for the fact that both models have found support from the same case (Britain). To avoid further confusion, I will develop the needed comparative logic by using a hypothetical example.

Suppose a "pure" CDL existed—one in which class position and ethnic identity were perfectly correlated. In this case, it would be impossible to separate the effects of ethnic and class ties on political mobilization. In order to test the two models, one would need to compare this case with a case(s) where no CDL existed (i.e., compare the upper left and lower left cells in fig. 2). Ceteris paribus, the developmental model provides no basis to distinguish the intensity of political mobilization between the two cases, while the reactive ethnicity model predicts that the "pure" case (upper left cell) will mobilize more readily owing to the ethnic ties among the disadvantaged class. The "ceteris paribus" clause, however, poses obvious problems for comparisons across stratification systems, especially when political processes are at issue. All the "crucial tests" that I am aware of have been confined to single stratification systems. The comparative logic described above must somehow be translated to work within a single stratification system.

In order to do this, clearly there must be contextual variation within the stratification system. Regions must vary in the degree to which they mirror the CDL of the larger system. In some, the subordinate (in the larger system) ethnic group must be economically advantaged, while in others the dominant ethnic group (in the larger system) must be economically disadvantaged. (The subordinate-dominant ethnic distinction has meaning only in the context of the larger stratification system—it must be a CDL to some degree.) That is, regions must fall within each cell of figure 3, although a correlation must exist between economic and ethnic status. This typology assumes that *regions* can be given a status on each dimension, which means

		Economically	
		Advantaged	Disadvantaged
Ethnically	Dominant	I (Core)*	II (Provincial)*
	Subordinate	III (Dissenting)*	IV (Peripheral)*

* Hechter's (1975) terminology.

FIG. 3.—A typology of regions

that ethnic groups must be somewhat segregated geographically (which also ensures that economic advantage is segregated in a CDL).[6] This geographic segregation should not be confused with isolation in the pre–economic development sense. A geographically segregated ethnic region can be very much a part of the industrialization of the larger stratification system. Given the typology of figure 3, the reactive ethnicity model predicts that political mobilization will be the most concerted among cell IV regions, while the developmental model, in focusing exclusively on the effects of class position on political mobilization, does not distinguish between cell II and cell IV regions.

The comparative logic for a single-stratification system differs from that for the multiple-stratification system example in one important aspect. Within the single system, the comparison is between homogeneous ethnic groups, while in the multiple-system case it is between a homogeneous ethnic group and a mixture of ethnic groups (where no CDL exists) in the disadvantaged class. Should not ethnic commonalities among the dominant ethnic group that is economically disadvantaged perform the same function as they do for the subordinate ethnic group? The answer is "no" if a system-wide reference is assumed. Only among the subordinate ethnic group will ethnic identity be linked to economic disadvantage, thus lending salience to the identity in the orientation of the group. Ethnic ties facilitate political mobilization only when the ethnic identity is salient in the orientations of group members, and this salience is achieved in developed settings when the ethnic identity is perceived as the basis of a collective economic oppression. This perception has no objective grounding among economically disadvantaged members of the dominant ethnic group.

The comparative logic outlined above was based on contextual variation and thus is capable of being directly tested with aggregate data. Within the present formulation, the two models diverge in the contexts, or conditions, they predict will facilitate political mobilization. Previous formulations also used contextual variation to test the models with aggregate data, but the theoretical use of ethnicity as an *interest* opposed to class interests made the

[6] It is essential to note the implications of classifying *regions*, and not individuals, on both dimensions. With this aggregate classification, economic "disadvantage" is a regional inequity in wealth distribution, and not a lower-class occupational position. Within a disadvantaged region, the whole gamut of occupational positions will exist, though perhaps in different proportions than exist in an advantaged region. The term "class" is retained for the developmental model, but it assumes the broader meaning of a regional social structural position within the aggregate classification, while past formulations based on the distinction between class and ethnic interests generally employed the narrower, traditional meaning of "class." The logic of the developmental model is the same for both meanings. Likewise, it must be recognized that a "subordinate ethnic" region will not be completely homogeneous in its ethnic composition, though it will be as close as a dichotomous classification allows. The interaction of the heterogeneous mixtures within each cell type (on both dimensions) may add important nuances to the merely rough predictions of this formulation, but until these rough predictions are verified, it is pointless to get bogged down in the nuances.

meaning of the contextual variation ambiguous in its relation to individual interests. This problem disappears when ethnic ties are instead conceived as a facilitating condition for political mobilization.

Within the present framework, three hypotheses can be derived and tested. The first concerns the adequacy of the developmental model; the second whether the (possible) inadequacy of this model can be accounted for by a lack of economic development in some regions; and the third whether the reactive ethnicity model can account for the (possible) discrepancies of the developmental model.

1. Hypothesis 1 (the structural break hypothesis): *If the developmental model is adequate, socioeconomic variables should account for regional variation in political mobilization, regardless of the ethnic status of the region.*

In developed settings, the developmental model predicts that political behavior will be a function of class interests. Socioeconomic variables (listed below) were used to depict these interests at the contextual (i.e., structural) level.[7] The absolute adequacy of these variables is not important here, just their relative utility among the different ethnic regions. Both models predict that they should be adequate within dominant ethnic regions (cells I and II in fig. 3). But the developmental model predicts that this should also hold in subordinate ethnic regions (cells III and IV), while the reactive ethnicity model predicts that the higher potential for mobilization lent by ethnic ties will "confound" the (linear) effects of socioeconomic variables expected in dominant ethnic regions. Thus the reactive ethnicity model predicts a "structural break" between the different ethnic regions, while the developmental model predicts that the same structural dynamics will be operative in each region.

2. Hypothesis 2 (the isolation hypothesis): *If the developmental model is adequate, any "structural break" observed in the test of hypothesis 1 should be attributable to a lack of economic development in subordinate ethnic regions.*

Even when a CDL exists, the two models make divergent predictions only when economic development has occurred.[8] Thus a "structural break" would be predicted by both models if some ethnic regions had yet to experience economic development. A lack of economic development means that the ethnic group has remained "isolated" from the predicted impacts of development, so it would not share the same political dynamics.

[7] See n. 6. The use of socioeconomic variables was justified by the shift in the meaning of "class" in going from the individual to the regional level. Ragin (1977) criticized the use of socioeconomic variables for "class" interests by Hechter, but in doing so failed to recognize the broader meaning of "class" being used by Hechter, the same as the one used here. Ragin's use of the narrower meaning (in using proportions in selected occupations) with aggregate voting data does not permit an unambiguous interpretation.

[8] A CDL is not functionally possible in an undeveloped setting since, by definition, multiple groups cannot be absorbed into a single-stratification system. Yet the data might *appear* to represent a CDL in showing economic inequalities across ethnic groups. If these have not been incorporated into a single-stratification system, however, no CDL actually exists.

3. Hypothesis 3 (the interaction hypothesis): *If the reactive ethnicity model is adequate, the observed "structural break" must be accounted for by the tendency for concerted political mobilization to be confined to subordinate ethnic regions that are also economically disadvantaged (cell IV).*

This is the hypothesis upon which the validity of the reactive ethnicity model rests. It posits that economic disadvantage alone is insufficient to produce concerted political mobilization, also needed are ethnic ties that serve as a facilitating condition for political mobilization. The meaning of "concerted" will be made explicit in the next section.

DATA AND MEASUREMENT

British census and election data will be used to test these hypotheses. Britain is especially well suited as a test case, for it has undergone marked economic development *and* possesses a well-delineated cultural division of labor.[9] In addition, proponents of both models have used British data to claim support for their perspectives (see Alford 1963; Butler and Stokes 1969; Hechter 1975; Ragin 1977; Paige 1978). Data in this study, the same as those used by Hechter (1975), matched election results of 1885, 1892, 1900, 1910, 1924, 1931, 1951, and 1966 with census variables from the nearest corresponding period. The data were at the county level of aggregation and were available for all 86 counties in Britain (40 English, 13 Welsh, and 33 Scottish counties). Data for Ireland were excluded because of Ireland's "atypical" pattern of agricultural development and its geographic insulation from the British mainland. Additional information concerning the data can be found in Hechter's (1975) study and Paige's (1978) critical article.

Because of data limitations and history's shifting demarcations, classification of the counties into the categories in figure 3 was the most difficult measurement problem. For example, in determining ethnic status it would be misleading to assign automatically all counties in Wales and Scotland a subordinate ethnic status, since some have been assimilated into English culture. This is especially true for lowland Scotland (see Hechter 1975). On the other hand, classification based solely on quantitative variables may prove hypersensitive to measurement error, invariably present in historical data. The delicate balance needed between a priori and a posteriori determination was achieved with discriminant analysis, a potentially useful technique for quantitative historical research.

Discriminant analysis required an initial classification of the counties whose ethnic status was least in question. Here, English counties were classified as dominant, while Welsh and highland Scottish counties were

[9] The subordinate economic position of the Welsh and Scottish peoples vis-à-vis the English is well known. Few cases better exemplify a cultural division of labor.

assigned subordinate ethnic status. "Discriminating" variables were then chosen that could serve to differentiate the two categories of ethnic status. These variables were percentage Celtic speaking, percentage Nonconformist, and percentage in Church of England. They were regressed on the "a priori" classification to form a linear equation that best fit the initial classification. All counties were then reclassified according to this equation, which allowed the classification of lowland Scottish counties as well as the reassignment of any "deviant" counties in the initial classification.

The level of economic development was also determined with the aid of discriminant analysis. The initial classification was based on the proportion of workers in agriculture. Thirty-five percent of the counties at each extreme were classified, leaving the middle 30% undetermined. Five discriminating variables were drawn from Hechter's (1975) factor analytical determination of socioeconomic variables that loaded high on an "industrialization" factor. These variables, which will also be used to test the structural break hypothesis (hypothesis 1), are percentage in manufacturing, percentage in middle class, decennial population growth, percentage aged 65 and over, and percentage in urban areas.

The level of economic development was controlled for in measuring economic status. All counties below the overall mean (for each time period) in per capita income at both the developed and the undeveloped levels of economic development were classified as disadvantaged, and those above the means as advantaged. Unfortunately, measures of per capita income were not published for the 1910, 1924, and 1931 census periods, so these periods were excluded from the analysis when this variable was involved. For an explanation of the limitations of the per capita income variable, consult Hechter's study (1975, pp. 161–64).

The classification procedures employed here differ markedly from those of Hechter, so a brief comparison is in order. For his classification of ethnic status, Hechter used residuals formed by regressing socioeconomic variables on proportion of Nonconformists. Counties with positive residuals were assigned subordinate ethnic status. The justification for this circuitous procedure is not altogether clear (see Hechter 1975, p. 329), nor are some of the results it produced. For instance, the county of London was classified as peripheral (cell IV in fig. 3), as were 11 other English counties in 1951. In that same year, Scotland had more core (cell I in fig. 3) counties and fewer peripheral counties than England, which is not congruent with Hechter's historical analysis that treats Scotland as an "internal colony." His classification results are included in table 1, along with my own. The use of residuals is always hypersensitive to measurement error and thus is capable of producing these peculiar results.

The dependent variable, political mobilization, was measured by the proportion of votes received by a political party at the county level. This

measure is standard in the literature. Considerable disagreement has emerged, however, over *which* party provides the outlet for the concerted political mobilization of the periphery. Hechter (1975) used the Conservative party as a negative outlet and specified the Labour party as the positive outlet, although he excluded the Labour party from his quantitative analyses. Ragin (1977, p. 442) has pointed out that, where more than two parties exist, lack of support for the Conservative party does not necessarily imply support for the Labour party. Such was the case for Britain after World War I. Ragin proceeded to show that Labour support was best explained by class-related variables, and that Hechter's ethnic status distinction was not needed. Neither gave empirical attention to the Liberal party, as this would only have confused matters further within their conceptualizations.

Within the present formulation, the problem of specifying an outlet is irrelevant. Its earlier relevance hinged upon the distinction between class and ethnic interests, which I have rejected. Parties were conceived as

TABLE 1

NUMBER OF COUNTIES IN EACH CONTEXT (See Fig. 3), BY YEAR AND REGION (Hechter's Results in Parentheses)

	England ($N=40$)	Wales ($N=13$)	Scotland ($N=33$)	Total
1885				
Core	24 (20)	1 (2)	5 (7)	30 (29)
Provincial	16 (16)	1 (1)	8 (5)	25 (22)
Dissenting	0 (2)	2 (1)	6 (8)	8 (11)
Peripheral	0 (2)	9 (9)	14 (13)	23 (24)
1892				
Core	23 (15)	1 (2)	6 (6)	30 (23)
Provincial	17 (21)	1 (0)	6 (4)	24 (25)
Dissenting	0 (1)	2 (5)	7 (4)	9 (10)
Peripheral	0 (3)	9 (6)	14 (19)	23 (28)
1900				
Core	24 (12)	1 (1)	8 (7)	33 (20)
Provincial	16 (22)	0 (0)	4 (3)	20 (25)
Dissenting	0 (1)	1 (1)	8 (6)	9 (8)
Peripheral	0 (5)	11 (11)	13 (17)	24 (33)
1951*				
Core	24 (12)	0 (0)	15 (14)†	39 (26)
Provincial	15 (13)	0 (0)	14 (8)	29 (21)
Dissenting	0 (3)	2 (4)	1 (4)	3 (11)
Peripheral	1 (12)	11 (9)	3 (7)	15 (28)
1966				
Core	21 (11)	0 (0)	16 (11)	37 (22)
Provincial	18 (18)	1 (0)	13 (10)	32 (28)
Dissenting	0 (7)	3 (3)	0 (4)	3 (14)
Peripheral	1 (4)	9 (10)	4 (8)	14 (22)

* The per capita income variable necessary in this classification was not published in the 1910, 1924, and 1931 censuses, so these years were excluded from relevant analyses.

† The historical antecedents of Scotland's shift to dominant cultural status are discussed by Hechter (1975, pp. 164–207). Statistically, this indicates a precipitous increase in Church of England membership and a decrease in number of Celtic-speaking inhabitants (mainly in lowland counties).

representing one or the other of these interests, so a county's voting behavior revealed the nature of its interests. Here, parties are conceived as means for the pursuit of political ends, whatever these may be. Ethnicity is not an interest, but a facilitating condition for mobilization in the pursuit of political interests. The historical content of political struggle, along with its rhetoric, is not important within this framework. Solely at issue is the capacity to mobilize. The reactive ethnicity model predicts that this capacity is enhanced by the existence of salient ethnic ties among an economically disadvantaged group. These ties facilitate *concerted* political mobilization. Evidence of *concerted* political mobilization thus becomes no more or less than the presence of the interaction effect predicted by hypothesis 3 (the interaction hypothesis). There must be concerted political mobilization for one political party—it does not matter which one—for confirmation of the reactive ethnicity model.

TESTS AND RESULTS

In the first hypothesis the developmental model rules out a structural break between ethnic status regions (upon which the reactive ethnicity model relies). To test this hypothesis, party vote was regressed on the five socioeconomic variables and on these five variables multiplied by a dummy coded one if the county was ethnically subordinate. Thus, if ethnic status is a viable distinction in determining structural parameters, the five interaction terms should contribute significantly to the reduction of unexplained variance. The appropriate F-test is:

$$\frac{(SSR_{12} - SSR_6)/(12 - 6)}{SSE_{12}/(86 - 12)} \sim F_{12-6,\ 86-12}.$$

A significant F indicates a structural break.[10] From this test, separate regression equations were obtained for each ethnic status region, along with $\bar{R}^2$'s adjusted for the differing degrees of freedom in each equation ($\bar{R}^2$). The structural break assumes the form predicted by the reactive ethnicity model if the $\bar{R}^2$ is greater for the dominant than for the subordinate ethnic regions.

The results are given in table 2. Regressions with Conservative vote demonstrate a clear structural break. For each of the eight time periods the distinction between ethnic status regions proved significant at beyond the .01 level (F-test results). In addition, each period produced an $\bar{R}^2$ larger for dominant than for subordinate ethnic regions. In no case was the $\bar{R}^2$ significant for a subordinate ethnic region (at the .05 level), while in only one case

[10] For details concerning this test, consult Kmenta (1971, pp. 370–71). An interaction term has been included for the intercept since there is no a priori reason why the intercept should remain the same in both regions.

TABLE 2

STRUCTURAL DYNAMICS OF POLITICAL BEHAVIOR WITHIN THE DIFFERENT ETHNIC STATUS REGIONS, FOR EACH POLITICAL PARTY

	Intercept a	Population Growth b_1	Proportion in Middle Class b_2	Proportion in Manufacturing b_3	Urban Population Proportion b_4	Proportion over Age 65 b_5	$\bar{R}^2$	F-Test for Structural Breaks[a] $F_{6,74}$
Conservative Vote								
1885:								
Dominant.....	.54*	.114	.444	−.451*	.109	−.992	.30	5.946
	(.27)	(.19)	(.30)	(.14)	(.08)	(1.68)		$P<.01$
Subordinate...	1.64	−1.16*	.542	.354	−.206	−4.76*	.16	
	(.66)	(.57)	(.70)	(.27)	(.17)	(2.20)		
1892:								
Dominant.....	.93*	−.441*	1.38*	−.768*	−.058	.362	.18	3.796
	(.26)	(.22)	(.46)	(.17)	(.13)	(1.6)		$P<.01$
Subordinate...	1.19*	−.417	−.520	−.113	.057	−3.97*	.09	
	(.33)	(.29)	(.63)	(.18)	(.16)	(1.52)		
1900:								
Dominant.....	.52	−.068	1.40*	−.141	.112	1.66	.26	3.836
	(.33)	(.29)	(.43)	(.15)	(.13)	(1.67)		$P<.01$
Subordinate...	.74	−.298	−.066	−.209	.481*	.534	−.03	
	(.75)	(.69)	(.80)	(.45)	(.25)	(4.96)		
1910:								
Dominant.....	.61	−.135	1.52*	−.137	−.009	−.998	.23	6.781
	(.38)	(.33)	(.53)	(.25)	(.14)	(1.81)		$P<.01$
Subordinate...	.67	−.506	.741	.073	.244	−.808	.16	
	(.53)	(.44)	(.45)	(.23)	(.15)	(1.87)		
1924:								
Dominant.....	.68*	.040	.309	−.428*	−.054	−.583	.37	5.887
	(.22)	(.21)	(.24)	(.11)	(.10)	(.75)		$P<.01$
Subordinate...	.05	.426	−.110	−.053	.029	−1.76	.00	
	(.40)	(.37)	(.73)	(.29)	(.19)	(2.01)		
1931:								
Dominant.....	.25	.363	.350	−.185	.003	−.622	−.00	4.826
	(.39)	(.33)	(.37)	(.21)	(.17)	(1.63)		$P<.01$
Subordinate...	1.33	−1.03	−.429	.520	−.266	−2.60	−.12	
	(1.5)	(1.56)	(.91)	(.49)	(.32)	(4.0)		
1951:								
Dominant.....	.63*	.059	.177	−.309*	−.074	−.470	.45	7.836
	(.11)	(.08)	(.12)	(.05)	(.05)	(.40)		$P<.01$
Subordinate...	−.89	1.03	−.274	−.190	.232	2.15	−.09	
	(.76)	(.63)	(.67)	(.39)	(.30)	(2.16)		
1966:								
Dominant.....	.42*	.080	−.023	−.346*	.046	.586	.28	7.170
	(.20)	(.16)	(.17)	(.07)	(.07)	(.60)		$P<.01$
Subordinate...	1.45	−1.14	.424	−.107	.039	−.559	−.26	
	(1.1)	(1.0)	(.37)	(.25)	(.19)	(1.8)		
Labour Vote[b]								
1924:								
Dominant.....	−.01	−.018	−.487	.330*	.358*	.845	.37	2.084
	(.33)	(.32)	(.36)	(.17)	(.15)	(1.12)		N.S.
Subordinate...	.004	.310	−.226	.260*	.151	−1.61	.58	
	(.19)	(.17)	(.34)	(.13)	(.09)	(.94)		
1931:								
Dominant.....	.46*	−.330*	.122	.367*	.076	−1.41*	.53	1.940
	(.17)	(.15)	(.16)	(.09)	(.07)	(.72)		N.S.
Subordinate...	−.76	.373	.328	.550	.448*	2.96	.54	
	(.96)	(1.0)	(.58)	(.31)	(.21)	(2.6)		
1951:								
Dominant.....	.35*	.063	−.109	.231*	.106	−.897*	.49	2.852
	(12)	(.08)	(.14)	(.06)	(.055)	(.45)		$P<.05$
Subordinate...	1.10*	−.121	−.761	.354	−.002	−3.81*	.44	
	(.53)	(.44)	(.47)	(.27)	(.21)	(1.5)		
1966:								
Dominant.....	.46*	.061	.071	.208*	.115	−1.98*	.46	1.894
	(.21)	(.17)	(.17)	(.07)	(.07)	(1.61)		N.S.
Subordinate...	.96	−.039	−.199	−.097	.261	−3.49	.06	
	(1.5)	(1.3)	(.49)	(.33)	(.25)	(2.4)		

[a] Explained in the text.

[b] The Labour party failed to acquire more than 3% of the vote in any election before 1924, when it obtained 27%. The Liberal party, for the most part, showed results similar (but inverted) to the Conservative party results.

* Significant at the .05 level (standard error in parentheses).

(1931) was the $\bar{R}^2$ insignificant for a dominant ethnic region. It can therefore be concluded, with respect to Conservative vote, that socioeconomic variables account for more variation in dominant ethnic regions than in subordinate ethnic regions. A similar, but slightly weaker, pattern was found for Liberal vote (not included in table 2).

Regressions with Labour vote give a different picture. Here the distinction between ethnic status regions proved significant only once (at the .05 level). Comparison of the $\bar{R}^2$ does not tend in either direction. These results, however, do not nullify the results found for Conservative and Liberal vote, as perhaps Ragin (1977) would contend. A structural break need be evidenced only in one party's results for doubt to be cast on the developmental model, for concerted political mobilization can be directed only toward one party, whatever party this may be. When more than two parties exist, the dynamics of at least two can be independent.

The isolation hypothesis (hypothesis 2) was designed to determine whether the structural break observed above is due to a lack of economic development in subordinate ethnic regions. If so, the structural break would not be anomalous within the developmental model. Since the overall rate of industrialization in Wales and Scotland has been slower than that of England (see Hechter 1975, p. 141), this possibility must be taken seriously. To test the isolation hypothesis, dummy variables were constructed for both the ethnic status and economic development dimensions. These were multiplied to form an interaction dummy: a variable assigned "1" if the county was ethnically subordinate *and* economically undeveloped, and "0" if not. Party vote was regressed on these dummies:

	Economically Undeveloped (=1)	Subordinate Ethnic Status (=1)	Interaction Term	
Party Vote =	$a + b_1X_1$	$+ b_2X_2$	$+ b_3X_1X_2$.	(1)

If the structural break observed in the test of the first hypothesis is the product of low economic development, b_3 should be significant. That is, to be consistent with the developmental model, the "noise" in the structural dynamics of predominantly developed societies must be confined, significantly, to undeveloped regions.

The results in table 3 indicate that for every time period and each party (except Liberal vote in 1951) the contribution of the interaction dummy was insignificant. These negative results can be interpreted in the following manner: the structural break in the relationship between socioeconomic variables and voting behavior was *not* found to be the result of a tendency for subordinate ethnic regions to be economically undeveloped. Thus the developmental model is unable to account for the "anomalous" results. Whether the reactive ethnicity model provides an adequate explanation for this "anomaly" remains to be seen by testing the interaction hypothesis.

TABLE 3

TEST OF THE ISOLATION HYPOTHESIS: IS THE SOURCE OF THE STRUCTURAL BREAK CONFINED TO ECONOMICALLY UNDEVELOPED REGIONS?

Party Vote	1885	1892	1900	1910	1924	1931	1951	1966
Conservative vote..	−.066	−.038	.010	−.035	−.126	−.023	−.082	−.069
	(.065)	(.068)	(.097)	(.088)	(.079)	(.139)	(.059)	(.067)
Liberal vote.....	−.049	.038	.003	.066	.137	.070	.158*	−.023
	(.068)	(.069)	(.095)	(.088)	(.099)	(.148)	(.071)	(.081)
Labour vote.....	No significant Labour party existed				−.011	−.048	−.063	.095
					(.064)	(.079)	(.055)	(.075)

NOTE.—Cells contain the interaction coefficient (δ_3) in eq. (1), with the standard error in parentheses. Statistical significance was required for an affirmative answer.

* Statistically significant at the .05 level.

In conceiving ethnic ties as a facilitating condition for political mobilization, the reactive ethnicity model predicts that mobilization will be greatest when such ties exist among a disadvantaged social structural position (measured here by per capita income). The interaction hypothesis was designed to test whether concerted political mobilization was indeed produced by the conjunction of economic disadvantage and subordinate ethnic status. Each of these conditions alone (i.e., cells II and III in fig. 3) should not be sufficient to produce concerted political mobilization. To test this interation hypothesis, a procedure identical with the test of the isolation hypothesis was used, only the economic development dimension was changed to an economic advantage-disadvantage dimension. Dummy variables were constructed for the ethnic and economic dimensions, along with a multiplicative interaction dummy (assigned "1" if the county was ethnically subordinate and economically disadvantaged, and "0" if not). Party vote was regressed on these dummies:

	Economically Disadvantaged	Subordinate Ethnic Status	Interaction Term	
Party Vote =	$a + b_1X_1$	$+ b_2X_2$	$+ b_3X_1X_2$.	(2)

The intercept and slope coefficients (a, b_1, b_2, b_3) can be interpreted as the mean proportion of votes for a party for each cell type when combined as in figure 4. The reactive ethnicity model hinges on the interaction coefficient (b_3). A significant interaction coefficient will indicate that ethnic ties do play an independent role in political mobilization when they are conjoined with economic disadvantage, contrary to the predictions of the developmental model.

The results in table 4 offer no support for the reactive ethnicity model. Slope coefficients of the interaction terms were statistically insignificant for all periods and parties, except Liberal vote in 1885. Even in this case, the direction of the coefficient was not congruent with expectations about the content of party issues—before the Labour party became a viable option, the periphery mostly supported the Liberal party. And though the coefficients assume the expected direction after 1892, insignificant magnitudes and missing time periods prevent any definitive statement on possible trends. Thus these results demand the conclusion that the conjunction of economic

		Economically
	Advantaged	Disadvantaged
Ethnically Dominant	a	$a + b_1$
Ethnically Subordinate	$a + b_2$	$a + b_1 + b_2 + b_3$

FIG. 4.—Guide to interpreting equation (2) coefficients

and ethnic subordination is unable to produce concerted political mobilization in the manner predicted by the reactive ethnicity model.

As a prelude to the much-needed discussion to follow, the results in table 4 should be examined further. It should be noted that the distinction between ethnic status regions observed in the results of the structural break hypothesis test reappear in this test. The salience of ethnic status can be found in the magnitude of b_2 for economically advantaged regions and b_2 plus b_3 for disadvantaged regions, as these represent the mean proportion of vote differences across ethnic status regions (see fig. 4). Ethnic status is most salient for Conservative and Liberal vote, but not for Labour vote, which is consistent with the structural break hypothesis test results. Con-

TABLE 4

TEST OF THE INTERACTION HYPOTHESIS: THE EFFECT OF DUAL SUBORDINATION ON POLITICAL MOBILIZATION

	Intercept a	Economic Disadvantage b_1	Cultural Subordinate b_2	Interaction (Dual Subordination) b_3	R^2	F-Test of Interaction Significance: F	Level[a]
			Conservative Vote				
1885	.476*	−.090	−.163*	.046	.28	.463	.498
	(.025)	(.037)	(.054)	(.067)			
1892	.552*	−.027	−.144*	.038	.15	.265	.608
	(.028)	(.042)	(.058)	(.073)			
1900	.688*	−.061	−.137	−.109	.27	1.263	.284
	(.037)	(.059)	(.079)	(.101)			
1951	.513*	.007	−.066	−.122	.29	2.617	.110
	(.018)	(.027)	(.067)	(.076)			
1966	.458*	−.048	−.111	−.021	.24	.076	.784
	(.019)	(.027)	(.068)	(.077)			
			Liberal Vote				
1885	.522*	.090*	.163*	−.142*	.13	4.009	.049
	(.026)	(.039)	(.058)	(.071)			
1892	.443*	.026	.136*	−.025	.14	.111	.740
	(.028)	(.042)	(.059)	(.074)			
1900	.308*	.062	.122	.127	.29	1.627	.206
	(.036)	(.058)	(.078)	(.100)			
1951	.048*	−.002	.026	.171	.24	3.483	.066
	(.022)	(.033)	(.081)	(.092)			
1966	.114*	.009	.066	.070	.12	.522	.472
	(.023)	(.034)	(.085)	(.097)			
			Labour Vote				
1951	.436*	−.001	.034	−.047	.01	.331	.567
	(.019)	(.030)	(.073)	(.082)			
1966	.422*	.014	−.015	−.015	.01	.024	.877
	(.023)	(.034)	(.085)	(.096)			

[a] Equivalent to the F-test in table 2, but with 1,82 degrees of freedom.

* Significant at the .05 level (standard error in parentheses).

versely, the economic status distinction has little salience for either cultural status region (b_1 and b_1 plus b_3). Thus the reactive ethnicity model appears convincing in its recognition of ethnicity as a salient factor in political mobilization, but something is askew in the specification (or measurement) of the factors that instigate this mobilization (i.e., the social structural conditions). This point will be further elaborated below.

DISCUSSION

A short summary of the theoretical and empirical results will be given before addressing the problems these results have raised. First, the long-held notion that class and ethnic interests were the distinctive predictions of the developmental and reactive ethnicity models, respectively, was discarded. Instead, the models were shown to diverge on the question of group formation. The developmental model does not take group formation in the pursuit of political interests to be problematic. The reactive ethnicity model does take group formation to be problematic and argues that ethnic ties, made salient by a CDL, serve as a facilitating condition for concerted political mobilization. Economic disadvantage alone is insufficient to produce a concerted effort in the pursuit of political interests. Ethnic ties provide a common form of discourse essential to any group action.

The shift from viewing ethnicity as an interest to seeing it as a facilitating condition helped resolve many recurrent problems in formulating a consistent test of the two models. First, it allowed an escape from the problem of overlapping class and ethnic boundaries in a CDL—the dually subordinated ethnic member no longer had to reveal which form of subordination, economic or ethnic, he was most interested in escaping. Second, and much related, it allowed a consistent test formulation using aggregate data. With ethnic ties seen as a condition and not an interest, the focus shifted to the context where individuals were situated, which is discernible with aggregate data. Third, it allowed the bypass of the historically specific content of particular campaigns and party platforms, the exclusion of which is essential to a general model. The dependent variable changed from the expression of a particular interest (i.e., ethnic or class) to the purely quantitative indication of concerted political mobilization, which I have equated here with the interaction coefficient in a regression equation.

Unfortunately, the improved formulation failed to yield conclusive empirical results. A structural break was observed in the dynamics of political behavior between the two ethnic status regions, but the explanation for this break provided by the reactive ethnicity model was not found adequate. Dual subordination (the conjunction of ethnic subordination and economic disadvantage) failed to produce concerted political mobilization. Further examination showed that the ethnic status distinction was salient (suggest-

ing ethnicity be given some role in political mobilization), but the economic status dimension lent little to further explaining variance in political mobilization.

Aside from data and measurement problems, the question must be raised whether the economic status dimension was adequate for our purposes. Within the present formulation, economic disadvantage was used to indicate an inferior social structural position occupied by a region within a larger social system (see n. 4). The inferior social structural position provided a common interest for the regional inhabitants, namely, to alter the inferior position, which could be pursued in the political arena. Thus the present use of "economic disadvantage" served to indicate regions where inhabitants had a common interest in the political arena.

From this point, the problem was to predict when and how the disadvantaged inhabitants would mobilize concertedly. To reiterate, the reactive ethnicity model here made the distinction between disadvantaged regions where salient ethnic ties exist among the inhabitants, and disadvantaged regions where no such ties exist. The assumption was that a CDL made ethnic ties salient among economically disadvantaged ethnic groups, whereas economic development would destroy the salience of ethnicity in all other situations (both models make this assumption). Salient ethnic ties serve as a facilitating condition for political mobilization. In their absence, mobilization is problematic.

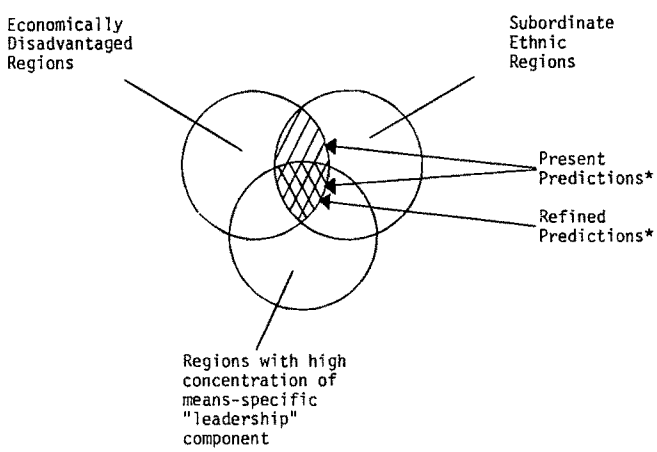
This, of course, led straight to the empirical failure. It is thus necessary to go back to see whether anything was left out en route. To do this, I must refer briefly to action theory (see Parsons 1949), which informs the logical frameworks of both models. Action theory is constructed upon conditions, means, and interests (ends), which are defined not absolutely, but relative to each other. Interests (political) and conditions (salient ethnic ties) are central concepts in the present formulation, as they have been in others (though with different meanings). But what about means? The "means" in all past and present tests of these models have been taken to be political mobilization in support of a national party. Given conditions of dual subordination and an interest in escaping this position, is this the only "means" available? Certainly not. Revolutionary tactics are possible, or even an escape from the whole orientational framework with an "otherworldly" religious movement. Or even more feasible, what about a separatist movement? Was not this, after all, the impetus for developing the reactive ethnicity model? Yet it was excluded from quantitative analysis in Hechter's work (see Hechter 1975, 1979).

The possibility of alternative means suggests that a variable factor is needed to explain the particular means that are employed. There is no hiding the fact that to a large extent data limitations confine empirical research to national elections, but this should not stifle the attempt for theoretical

closure. If the factor(s) that determines the means employed is specified, more precise predictions can be formulated when any given means is focused on, including political mobilization in support of a national party. Thus I conjecture that the present empirical failure was due to the exclusion of a logically necessary factor needed to account for the means focused on in this test.

This missing factor must be linked to the leadership component in mobilization, as it is this component that determines the alternative directions available. Each possible mobilization alternative is distinguished by a different type of leadership component. For example, a leadership component that directs mobilization toward the national political arena is generally developed within the educational institutions of the dominant ethnic group. Members of the subordinate ethnic group trained in these institutions have a tendency to direct their political energies back toward the national "center" in seeking political change. A counter logic can be applied to those trained in local centers, who are more likely to press for "separatist" political platforms.

For purposes of illustration, assume that these assertions have received adequate empirical support. In selecting an alternative means to focus on, one is then obligated to use the incidence of the requisite leadership component as a further condition for concerted political mobilization. For example, the proportion of adults educated in institutions of the dominant ethnic group might have to be incorporated as an independent variable when national party vote is being focused on. This is illustrated with the aid of a Venn diagram (fig. 5). The potential for refining predictions concerning



* Regions where concerted political mobilization is predicted.

FIG. 5

the incidence of concerted political mobilization is obvious. The merit in moving in this research direction is that this third dimension is not a mere appendage added to explain more variance, but a logically necessary component in the larger theoretical framework—action theory—in which the reactive ethnicity model is situated. Furthermore, the component is consistent with the general nature of the models under consideration. Too often anomalous regression coefficients are "explained" by historically specific arguments, leaving the general models, which generated the coefficients, in highly ambiguous positions.

CONCLUSIONS

This paper has shown that important similarities between the developmental and reactive ethnicity perspectives have been ignored and important differences misconceived. The distinction between class and ethnic interests has allowed a sharper division between proponents of the two models than was theoretically or empirically justifiable. Reactive ethnicity theorists have found ethnic interests everywhere from Alaska (Ritter 1979) to southern Italy (Palloni 1979). Developmental theorists, in turn, find only concrete economic motives behind the "facade" of ethnic rhetoric. Both are "correct," but only because they are looking at the wrong things. Throw away the distinction between class and ethnic interests and suddenly the data begin speaking only one language. This is not the language of economic motives or passionate ethnic appeals, but the language of political mobilization. The aim of a model of political mobilization is to predict the degree of mobilization that will occur in concrete settings. The developmental model tries to do this without treating group formation as problematic; the reactive ethnicity model treats group formation as problematic and conceives ethnic ties as facilitating conditions in the formation of groups that underlie concerted political mobilization. The relative success of each model was reported, along with suggested directions for improvements.

Both models are based on the postulate of economic rationality, but only the reactive ethnicity model attempts to imbed this rationality into a sociological context.[11] Sociological context is introduced by recognizing that the problem of organizing behind shared interests involves factors outside the scope of economic rationality. I believe that the only way to reconcile the apparent contradiction between the recent upsurge of ethnic-based political movements and the steady homogenizing impact of economic development—both powerful forces in the modern world—is to develop a better understanding of the sociological context of economic rationality.

[11] I am indebted to Harrison White for the belief that economists may have something to learn from sociologists. His recent work on markets as induced role structures lends convincing support to this conviction (White 1981).

The reactive ethnicity perspective makes an effort in this direction by specifying necessary conditions for group formation, although its empirical and theoretical shortcomings leave considerable room for further work.

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Living Alone, Social Integration, and Mental Health[1]

Michael Hughes
Virginia Polytechnic Institute and State University

Walter R. Gove
Vanderbilt University

This study is an examination of the effects of living alone on mental health, mental well-being, and maladaptive behaviors. The findings may be summarized in three basic points. First, there is no evidence that persons who live alone are selected into that living arrangement because of preexisting psychological problems, noxious personality characteristics, or incompetent socioeconomic behavior. Second, contrary to what would be predicted by structural functionalism or symbolic interactionism, the data analysis in this study shows that unmarried persons who live alone are in no worse, and on some indicators are in better, mental health than unmarried persons who live with others. Furthermore, divorced and never-married persons who live alone have more in common with married persons, in terms of their mental health characteristics, than do such persons who live with others. Third, unmarried persons who live alone show a slight tendency to be more likely to engage in maladaptive behaviors such as drug and alcohol use than are unmarried persons who live with others. These findings have implications for our thinking about the effects of social integration on mental health; the results raise the possibility that socially integrated relationships which provide not only (1) direct social rewards through reinforcement and increased meaning in life but also (2) regulation of behavior through mechanisms of social constraint, obligation, and responsibility, may entail not only rewards but also costs. For persons who live in socially integrated relationships, if the decrements to mental health produced by social regulation are not balanced by social rewards through some as yet unknown process, such social integration may help create psychological distress.

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During the past few decades, the proportion of persons living alone has increased substantially. In the past few years this proportion has been increasing at an accelerating rate. For example, from 1960 to 1978, the number of persons in the United States who live alone more than doubled from 7.1 million to 16.7 million (U.S. Bureau of the Census 1979, p. 3). As Michael, Fuchs, and Scott (1980) note: "Growth in the propensity to live alone appears to be a pervasive phenomenon in the United States in the postwar period. By 1976 more than one out of every ten adults lived alone; among elderly (65+) women the proportion was more than four out of ten and among elderly women without a spouse two out of three" (p. 49). At present, slightly more than one in every five households consists of a person living alone (U.S. Bureau of the Census 1979, p. 2). It has long been known that living alone is associated with a number of pathological behaviors. This fact has frequently been taken as support for social integration theory, since it suggests that social isolation is associated with pathological behaviors. While living alone is not a perfect indicator of social isolation, structurally it produces a certain amount of social isolation, tending in particular to prevent the development of the type of close social relationships that are typically viewed as the key component of social integration. In this paper we review briefly the literature on social integration and the evidence that social isolation, particularly as indicated by living alone, is associated with pathological behavior. We then explore, in a normal population, whether living alone (1) is a consequence of social selection and (2) is associated with pathological behavior. This analysis points to some changes that are needed in our conceptualization of social integration and its effects.

THEORETICAL BACKGROUND

Virtually all the theorists responsible for the establishment of sociology, somewhere in their writings, affirmed the vital nature of close social bonds, contrasting them to alienation, anonymity, rationality, and social isolation. This affirmation is reflected in Marx's concern for the effects of capitalism on the family, in Comte's proposed "positive" society, in Tönnies's concern for the demise of *Gemeinschaft* society, in Simmel's discussion of sociation and his analysis of dyads, in Durkheim's profoundly influential work on suicide, and even in Weber's concern about the increasing anonymity produced by rational forms of organization.

Modern sociology shows a similar tendency to view close affective ties and familistic living arrangements as important for personal well-being. This is evidenced early, in the writings on the family by Ogburn (1938) and Burgess and Locke (1945), and later in the writings of symbolic interactionists and structural functionalists. Though structural functionalism

and symbolic interactionism are substantially different theoretically, there appears to be agreement that close social relations are critically important for personal well-being and development. Thus, Parsons and Bales (1955) view family life as necessary for the stabilization of adult personalities and the socialization of children, and symbolic interactionists emphasize the importance of significant others in the development and maintenance of the self (Blumer 1966; Mead 1937; Cooley 1902) and maintain that such processes are not confined to primary socialization but are continually evolving throughout one's life (e.g., Scheff 1966; Becker 1973). Such views are also consistent with status integration theory, which assumes that, where social relations are unstable and not durable, there will be high rates of pathology, including suicide (Gibbs and Martin 1964), psychiatric symptoms (Jackson 1962), psychiatric hospitalization (Martin 1976), chronic physical disorders (Dodge and Martin 1970), and crime (Gibbs 1966).

In summary, a key theme in both classic and modern sociological theory is that close social bonds involving continuing primary interaction are critically important in developing and maintaining an individual who functions effectively and is psychologically healthy.

SOCIAL ISOLATION AND MENTAL HEALTH

Although a very extensive literature asserts that close primary relations are extremely important for the psychological well-being of the individual, the literature dealing directly with the detrimental effects of a relative lack of social interaction (i.e., social isolation) on the well-being of the individual is limited. In the classic sociological literature, Simmel's (1950) discussion of "the stranger" comes the closest to dealing with the effects of social isolation. Park's (1928) discussion of social marginality, which draws heavily on Simmel's model, focused attention on social isolation as a research problem for American sociologists. The work of Faris (1934, 1938, 1952; Faris and Dunham 1939) provides the earliest fairly clear, empirically supported, theoretical formulation that social isolation is related to mental illness, particularly schizophrenia. In Faris's (1934) first theoretical formulation, schizophrenia was seen as arising out of social isolation. In his words, "Any form of isolation that cuts the person off from intimate social relations for an extended period of time may lead to this form of mental disorder" (Faris 1934, p. 157). While Faris does not see social isolation as the only causal factor in the development of schizophrenia, he clearly considers it a prime element, as a review of his 1934 and 1938 publications demonstrates. This view was given a systematic empirical dimension by Faris and Dunham (1939), who found that the areas of the city closest to its center which were the most disorganized and had the

highest rates of schizophrenia were the areas with the highest number of persons living alone.

In his book on social psychology, Faris (1952) indicates why social isolation may result in schizophrenia: "The organization of mind and behavior which constitutes the normal personality is achieved through experience in a group life of an organized character" (p. 352). In reference to the isolation generally observed to have occurred prior to hospitalization for schizophrenia, Faris notes, "There is then a perpetual failure to develop a completely satisfactory organization of a conventional sort, and instead the development of a certain amount of purely personal and virtually incommunicable private mental organization" (p. 353). The reactions of the social environment to this incomprehensible person often result in more isolation and apparently more incomprehensibility (pp. 353, 355).

From the time of Faris's first formulation until the mid-fifties, Faris's conception of the link between social isolation and schizophrenia was central in psychiatric sociology (Weinberg 1956, 1967). For example, Jaco (1954) reasoned that isolation may be a precipitating factor in schizophrenia by showing that on 13 (of 19) measures of social isolation, community areas in Austin, Texas, with high rates of schizophrenia also had high levels of social isolation.

The first critical evaluation of Faris's position was a study by Kohn and Clausen (1955). They compared a sample of 45 schizophrenics and 13 manic depressives with a set of controls who were individually paired with the patients in terms of age, sex, and occupation (or father's occupation). They assessed the extent to which the patients and controls were socially isolated from their peers when they were 13–14 years old. They found "a significantly larger proportion of both the schizophrenics and the manic depressives than the controls [to] have been isolates or partial-isolates" (Kohn and Clausen 1955, p. 268). This, of course, is precisely the relationship predicted by Faris. The data, then, appear to be generally consistent with Faris's formulation, particularly with regard to isolation. We find this fact to be rather impressive, given that they are dealing with the recall of childhood events after a time period that considerably exceeds 16 years (Kohn and Clausen 1955, p. 268).[2]

[2] Kohn and Clausen (1955) concluded that their data directly contradict Faris's formulation. Their findings do not, however, provide unequivocal evidence that Faris's formulation is incorrect. In fact, they appear to differ from Faris on only one point. Faris suggests that isolation is related to mental illness. Kohn and Clausen find that it is. Faris suggests that certain types of parental behavior are related to mental illness. Kohn and Clausen find that they are. Faris suggests that presentation of psychiatric symptoms in conventional social situations may cause an individual to be experienced by others as incomprehensible, leading to more pronounced social isolation. Kohn and Clausen concur, concluding that mentally ill persons are isolated because of difficulties associated with being mentally ill. The only substantive difference between Faris and Kohn and Clausen is over the perceived cause of the child's isolation.

An article by Lowenthal, "Social Isolation and Mental Illness in Old Age" (1964), is commonly taken as evidence that social isolation is not linked to mental illness among the aged. It is clear from data in her article that the population of 534 persons 60 years of age or older who were admitted to the psychiatric screening wards of San Francisco hospitals in 1959 were more isolated than a random sample of community residents (the sample was stratified by sex and living conditions in order to be comparable to the hospital population on those variables). It is also clear that, in the community sample, the isolates were more likely to score impaired on a psychiatric symptom inventory than nonisolates. However, Lowenthal finds that socioeconomic status is related to both isolation and mental illness (and psychological distress) and concludes that there is no causal link between isolation and mental illness. She also finds that isolates are more likely to have psychological impairment arising from organic disorder or alcoholism than from psychogenic disorder. Her overall conclusions are that (1) extreme lifelong isolation does not necessarily result in mental disorder (pp. 68–69), (2) there appears to "be no relation between age-linked isolation and types of mental disorder in old age," and (3) "relative isolation may be more a consequence than a cause of mental illness in old age." Thus, Lowenthal finds that isolation is related to mental distress but concludes that the relationship is due to selective factors. Unfortunately nothing in the data she presents would enable us to evaluate directly the importance of selective processes.

Pearlin and Johnson (1977), using as a measure of isolation a scale combining (1) length of time in the neighborhood, (2) having close friends nearby, and (3) belonging to voluntary associations, have shown that isolation is related to depression for both the married and the unmarried but that there is interaction between marital status and isolation, with the unmarried becoming more severely depressed. This is consistent with Gove (1972), who suggests that one of the reasons the married are in better mental health than the unmarried is that they tend to be better integrated into their community.

In the literature reviewed above, social isolation has been variously (and often unclearly) defined. Nevertheless, the literature on the relationship between social isolation and mental illness presents a consistent pattern showing a fairly strong positive correlation between these two variables, with some ambiguity in regard to whether isolation is causal or produced by factors associated with mental illness. We would also note that studies of animal behavior (Allee et al. 1949; Galle and Gove 1978) and sensory deprivation among humans (Gove 1970) suggest that isolation has detrimental effects. In the remainder of this paper we are going to focus on a very specific type of isolation, namely, the social isolation that is produced by living alone.

LIVING ALONE AND "PATHOLOGICAL" BEHAVIOR: A REVIEW OF THE LITERATURE

In a study of London, Sainsbury (1955) showed that a higher proportion of suicides than of other persons lived alone and that if one arranged London boroughs in order of percentage living alone, this order showed a remarkable similarity to that produced by arranging the boroughs in order of the suicide rate. He suggests that the relationship is probably one of cause and effect, that a solitary life-style is causally related to suicides because of loneliness (Sainsbury 1955, p. 76). Shortly thereafter, Schneidman and Farberow (1957) showed that persons who live alone in Los Angeles also have a much higher suicide rate than the general population.

A recent study by Kobrin and Hendershot (1977) looked at the relationship between living arrangements and mortality rates. They used data from the National Mortality Survey which accumulated fairly detailed individual data on other studies. They found that the married, and particularly the married who had children, had the lowest mortality rate. They found that "among unmarried men mortality was lowest for family heads, increased for family dependents, and was highest for men living alone" (p. 16) and that this was particularly true for younger men. Among unmarried women they found that "mortality was also lowest for family heads but was next highest for women living alone, and highest for family dependents" (p. 16). However, among unmarried females in their youngest age category (35–44 years), those who lived alone had the highest mortality rate.

Redick and Johnson (1974) have published data on living arrangements from a representative national sample of all persons in the United States admitted to public mental hospitals and outpatient psychiatric clinics. Although psychiatric treatment is clearly different from the general mortality rate, the patterns of relationships are similar to those found for mortality. Persons living alone in general have much higher rates of psychiatric treatment than the general populace, and the rates are much higher for the young than the old and for males than for females.

A study by Webb and Collette (1975) dealing with living alone in New Zealand shows that the purchase of stress-alleviative drugs (tranquilizers, antidepressants, etc.) is very strongly related to the percentage of persons living alone in urban areas. They interpret this as indicating that persons living alone are much more likely than others to be treated by doctors for psychological problems.

SOCIAL SELECTION AND ISOLATION

All of the studies reviewed have have shown that social isolation, however defined, is strongly related to a pathological state among individuals.

As was indicated earlier, there are substantial theoretical grounds for assuming that these relationships are largely a consequence of lack of social integration. However, such an interpretation can be criticized (correctly), for the studies have been unable to rule out the social selection (or "drift") hypothesis (Myerson 1940). In fact, as noted above, some of the studies (Kohn and Clausen 1955; Lowenthal 1964) interpret their results as being a consequence of drift. This hypothesis suggests that problematic characteristics of individuals may be responsible both for persons being identified as having psychological problems and persons being channeled into situations of relative isolation and low levels of social integration. As the extensive review in Gove and Hughes (1980) makes clear, there are substantial grounds for believing the process of social selection occurs. This same kind of reasoning has also been suggested to explain why the married are in better mental health than the unmarried (Rushing 1979) and as an alternative to the hypothesis that stress is responsible for the high rates of mental illness among the lower class (Turner and Wagenfeld 1967; Grunfeld and Salverson 1968; Turner and Gartrell 1978; Dunham 1965).

Gove and Hughes (1980), building on the methodological framework developed by Hammond (1973), Hanushek, Jackson, and Kain (1974), Irwin and Lichtman (1976), and Firebaugh (1978), examined the relationships between living alone and both suicide and mortality due to alcoholism (cirrhosis of the liver), controlling for drift through the process of randomization. They found that living alone was a very strong predictor of these two types of mortality. This study demonstrated a direct relationship between living alone and both suicide and mortality due to cirrhosis of the liver that is not due to drift. In an attempt to interpret their findings, Gove and Hughes (1980, p. 1173) proposed a number of processes by which living alone may be related to suicide, alcoholism, and other forms of pathological behavior, some of which deal with a lack of social integration which results in the sense that one's life is meaningless and empty, while others dealt with very mechanical processes whereby the simple presence of others would minimize problems which, if the individual were alone, might escalate until they overwhelmed the individual.

THE RELATIONSHIP AMONG SOCIAL INTEGRATION, SOCIAL ISOLATION, AND LIVING ALONE

Social integration is conventionally thought to be an attribute of social situations arising from the density of social interaction and to be characterized by strong common sentiments and by intensification and fortification of the meaning of life, of social regulation, and of constraint. Following Durkheim, social integration is sometimes seen as a two-part process involving both social integration per se and regulation (often alluded to by

means of the corresponding terms "egoism" and "anomie"). There is a growing tendency to see these two phenomena as part of a single process of social integration (called by Pope [1976] "integration-regulation"). In his restatement and refinement of Durkheim's theory of suicide, Pope (1976) includes a clear statement of the basic factors related to the emergence of social integration:

> The complete theory of suicide is produced by joining the theory of egoism-anomie with that of altruism:
> 1) the morphological structure of society determines
> 2) number of people in a given group or area, which determines
> 3) rate of social interaction, which determines
> 4) strength of collective sentiments, which determines
> 5) level of integration-regulation (social control), which determines
> 6) means-needs proportionality (meaning of life) (theory of egoism-anomie), meaning attached to continued existence (theory of altruism), which (in combination) determine the
> 7) social suicide rate. [Pope 1976, p. 60]

If we accept this view, social integration is essentially a product of the rate of social interaction and the resulting emergence of common sentiments.[3]

It is reasonable to assume that those who live alone are, as a category, more isolated and experience less social interaction than the category of persons who live with others. The fact that no other persons are living with them means that: (1) although their working and recreational time may (but does not necessarily) involve social relations with others (as is true of those who live with others), the hours persons who live alone spend sleeping, engaging in personal maintenance, preparing and consuming meals, watching television, and so on are much less likely to be spent with others than is the case for those living with others; (2) the social conditions are such that it is much less likely that persons who live alone can build to the same level of commitment to other people as is possible for persons who live with others, and thus it is less likely that they will have personal relationships of the same closeness and intensity as those who live with others (Homans 1950, p. 133); and (3) persons who live alone are therefore much less likely to be engaged in situations characterized by mutual obligations and mutual reinforcement of a primary sort, particu-

[3] Such a view of social integration, while explicitly stated, does tend to leave to the imagination the specific processes which link dense interaction to common sentiments, constraint, and meaning of life, and thus to low suicide rates. This may be a useful, generalizable framework for explanation, but its abstraction also unavoidably results in an explanation which is somewhat vague. The same vagueness is reflected in the work of researchers in the quality-of-life tradition, who demonstrate empirically the connections between such things as the domain of family life and life satisfaction (Campbell, Converse, and Rogers 1976) or connections between family or home satisfaction and life satisfaction (Andrews and Withey 1976).

larly social relationships involving daily obligations and reinforcement. This is because persons who live alone are isolated from the network of social and economic ties (i.e., privileges, duties, and obligations) which are centered on the dwelling place, typically occupied in our society by a family. They lack involvement in the standard form of social economy through which most persons obtain both the requirements for survival (food, clothing, shelter, etc.) and emotional gratification and personal satisfaction. In summary, persons who live alone are apt to be more socially isolated. More important, this means they are not part of the key microenvironment of social and economic ties that typically serves individual needs directly and links the individual to society (i.e., some form of the family). Thus they totally lack the most basic network of social and economic ties characteristic of most persons in our society. This network of commitments, obligations, and privilege forms the functional component of social integration as traditionally conceived. Persons who live alone, by that attribute alone, experience less social integration than persons not so situated and experience it in a fundamentally different way.

SUMMARY

The literature indicates consistently that social isolation or the lack of social integration, including isolation produced by living alone, is strongly associated with a variety of pathologies. Furthermore, the evidence indicates that at least some of this relationship is not due to drift, but the relative importance of drift has not been established. An important aspect of these studies, particularly those dealing with persons living alone, is that they tend to deal with very extreme cases of "pathology," and it is not clear what they indicate regarding the mental health of most persons who live alone. And, in fact, to date no study has focused on the relationship between living alone and mental health among a normal population. This means that most of the evidence for social integration theory must be viewed as resting on a tenuous empirical base.

We shall now turn to the relationship between living alone and mental health. Our analysis will begin with a careful consideration of the issue of selectivity. Clearly, selectivity (or drift) is important with regard to certain forms of severe pathology, but it is not clear that it is a significant factor in respondents drawn from a normal population.

DATA AND METHODS

The data for this study were collected from a stratified random sample of 2,248 respondents aged 18 years and over and residing in the 48 contiguous states during the winter of 1974–75. As the study was designed to

investigate relationships among sex roles, marital roles, and mental health, the widowed and divorced (particularly males) were oversampled (11,397 households were screened). Although there is a weighting procedure we could use to correct this, it is used only in the analyses of median income and median education below. In the analyses of the relationship between living alone and other factors, the weighting procedure is not used because to do so would reduce the degrees of freedom and make it less likely that we would find significant differences between living alone and living with others. The oversampling is not a problem in these analyses, since we did analyses within marital status categories and we control age and sex. We ran all the analyses with the weighting procedure in effect, and the results were, in substance, virtually identical to those presented below.

The interview schedule was developed by the second author, and sampling, interviewing, and coding were done by Leiberman Research, Inc. Besides the initial attempt to contact a potential respondent, two callbacks were made if no one was at home. The average interview lasted for one hour and 20 minutes. There was an 8.8% refusal rate at the time of the screening interview and a 14.5% refusal rate (including break-offs during the interview) after the household member to be interviewed had been randomly selected.

The cell sizes obtained are shown in the unnumbered table below.

	Married	Never Married	Widowed	Separated/ Divorced
Men.....	623	213	152	151
Women...	602	209	149	149

Of the never married, 140 were living alone, as were 213 of the widowed and 120 of the divorced.

For reasons which will become obvious later in the analysis, we will be using a direct control for marital status. This control will take two forms, one which includes the married in the analysis and (more often) one which does not. Thus, when the married are included in the analysis, we will have a seven-category independent variable which incorporates marital status and living alone. This will enable us to make an overall comparison of the relationship of marital status and living arrangement with the various dependent variables after adjusting for the relevant demographic variables, but it will not allow us to make significance tests of the effects of living alone. When specifically assessing the impact of living alone, we will perform the analyses within each marital status category, this time using as the prime independent variable a simple dichotomy—(1) living alone, (2) not living alone—and adjusting the means for relevant demographic variables.

Multiple Classification Analysis (MCA) (Andrews, Morgan, and Sonquist 1969) is used to analyze the data so that we can employ categorical independent variables, control for the effects of other variables, and present the results in comprehensible form.

THE SELECTION PROBLEM

The strategy of the analysis is based on the assumption that in order to observe the relationship between living alone and mental health, we must compare those who live alone with those who do not, while controlling for extraneous factors. One of the most important factors and clearly the most difficult, which may limit our ability to come to firm conclusions, is selectivity (drift). As explained above, selectivity refers to the fact that a person may drift into an isolated living arrangement because of a severe disorder, and when that occurs it would be incorrect to suggest that living alone is a causal factor in the production of mental distress. There are at least three ways in which selectivity might occur.

First, persons whose upward mobility is severely limited may be perceived as undesirable living companions and as a consequence may be especially likely to live alone. Furthermore, the personal characteristics which are related to a lack of mobility may be related also to behavioral and mental health problems. Second, persons who have had stressful childhood experiences may have psychological problems which make them difficult to live with or cause them to avoid close interpersonal relationships. Third, in the absence of other factors, there may be persons in our society who have personality characteristics which, either in themselves or because of the societal reaction they produce (such as rejection or isolation), are causally related to a lack of mental well-being or to psychological distress. In all of these cases, a relationship between living alone and poor mental health would be largely spurious. With cross-sectional data it is difficult to determine with certainty the extent to which selectivity affects the relationship between living alone and mental well-being. However, there are measures in the survey which can be analyzed to see whether selectivity appears to pose a serious problem.

Examination of data on income and education is a good place to start. If persons who live alone tend to have much lower incomes and less education, then selectivity may be an important variable. It is also possible with these data to compare a respondent's education with that of his or her parents. By comparing those who live alone with those who do not, we will obtain some idea of their relative social mobility. The survey also contains retrospective data concerning problematic childhood experiences and adolescent desire to get married and to have children. If persons who live alone show a strong tendency to report such experiences and to indicate

in high proportions that as adolescents they did not want a spouse or children, there is a basis for assuming that selection may be a factor. In addition, the national survey contained a checklist of 22 self-reported personality traits. If persons who live alone are appreciably more likely to report personality traits which we may reasonably assume to be related to (1) some underlying pathology, (2) difficulties in interpersonal relationships, or (3) an inability to cope with the day-to-day stresses of life, this also suggests that living alone may be partly a consequence of social selection.

In summary, if the data indicate that, in comparison with others, persons who live alone have a low income, have little education, are lacking in upward mobility, and had an unhappy childhood and/or have personality characteristics that either make it difficult to interact with others or are indicative of emotional difficulties, then we may say with some confidence that the major reason persons live alone is social selection. In contrast, if those who live alone show no indication of any of these processes, we have no basis for rejecting a null hypothesis that selection processes are not a major determinant of living alone.

Very few persons who report they are married live alone. The national probability sample used in this study includes only two, and for clarity of presentation they have been eliminated from the analysis. The vast majority of persons who live alone occupy one of the other marital statuses—never married, widowed, or divorced/separated. Since numerous other studies (e.g., Gove 1972, 1979; Pearlin and Johnson 1977) have demonstrated great variation in mental health across marital status categories, in all of the analyses of living alone a direct control for marital status is clearly needed.

Table 1 presents the medians for personal income, per capita income, and household income of persons who do and do not live alone within marital status categories. In this analysis the weighting procedure was used so that the figures would be representative of the nation as a whole. Household income appears to favor those living with others, but of course their median income includes income generated by others in the household. Per capita income appears to favor those living alone; however, because such persons have no dependents in their households, this may not be an entirely valid indicator of income. Respondent's income, probably the best measure for assessing the selection argument, shows that those who live alone generate more income if they have never been married or are divorced but not if they are widowed. However, if we control age directly and look at differences between the most typical widowed persons, those over 55 who live alone and those who do not, the median income for those who live alone is higher ($3,045 for those not living alone, $3,813 for those living alone). This is because those who are both young (and thus more

TABLE 1

MEDIAN INCOME BY MARITAL STATUS AND LIVING ARRANGEMENT

Variable and Living Arrangement	Married	Never Married	Divorced/ Separated	Widowed
Respondent's income:				
Not alone	$8,906	$4,497	$5,090	$4,428
Alone	...	5,725	7,728	3,491
Difference	...	−$1,228	−$2,638	$ 937
Per capita income:				
Not alone	$3,664	$3,500	$2,337	$2,344
Alone	...	5,227	7,775	3,109
Difference	...	−$1,727	−$5,438	−$ 765
Household income:				
Not alone	$12,922	$12,836	$6,764	$6,858
Alone	...	5,227	7,775	3,109
Difference	...	$7,609	−$1,001	$3,749

likely to be earning an income) and widowed are more likely to live with others. Overall, these data suggest that persons who live alone are not deficient in their ability to generate an income as compared with those with the same marital status who live with others.

If we compare those who live alone with those who live with others in regard to education (with the weighting procedure in effect), persons who live alone are higher in median years of education if they are widowed (alone = 9.3; not alone = 8.4), are no different if divorced (alone = 11.4; not alone = 11.4), and are slightly lower if they have never married (alone = 11.6; not alone = 12.3). However, if we control age, there is no difference in regard to education between the never married who live alone and those who do not.

We also investigated the possibility that persons who live alone have been less upwardly mobile than those who live with others. We constructed a crude measure of social mobility by subtracting the education of the respondent's same-sex parent from that of respondent, and we found that persons who live alone tend to have greater upward mobility than those living with others (the difference was significant for the divorced before controls) but that the differences were not significant after controls were entered for age and sex (table on request).

Table 2 presents the means (adjusted for sex and age by MCA) of items measuring childhood experiences and adolescent desires about establishing a family. The "negative background index" is an additive scale of four items, each coded 1 if a respondent indicated: (1) one parent who had been in a mental hospital; (2) one sibling who had been in a mental hospital; (3) parental divorce; or (4) parental drinking problem. There

TABLE 2

MEAN SCORES ON MEASURES OF VARIOUS BACKGROUND CHARACTERISTICS BY MARITAL STATUS AND LIVING ALONE, ADJUSTED FOR AGE AND SEX

	Never Married		Divorced/Separated		Widowed	
Variable	Not Alone	Alone	Not Alone	Alone	Not Alone	Alone
Negative background index	.434	.486	.587	.530	.273	.259
Childhood happiness*	3.30	3.18	3.20	3.12	3.44	3.33
Wanted children as teenager†	.804	.777	.782	.848	.717	.778
Wanted as teenager to marry‡	2.62	2.48	2.85	2.72	2.70	2.72

* Codes were: 4 = very happy, 3 = pretty happy, 2 = not too happy, 1 = not at all happy.

† Question was worded: "When you were a teenager did you want to have children someday?" Yes = 1 no = 0.

‡ Question was worded: "When you were a teenager how much did you want to get married someday?" A great deal = 4, a fair amount = 3, not too much = 2, not at all = 1.

are no significant differences between those who live alone and those who do not. Furthermore, neither of the two retrospective questions concerning the adolescent desire to have a family shows significant relationships in the direction of the selectivity argument. Persons who live alone are no less likely to say that as teenagers they wanted to get married and have children than are people who live with others. In fact, for the divorced the relationship is in the opposite direction.

We also did an analysis comparing those who live alone with those who do not on an array of self-reported personality characteristics. For each trait, we calculated the proportion of persons within each mental status and living arrangement who indicated that the trait described them. None of the characteristics which we can reasonably expect to be related to instrumental abilities (competitive, logical, hard-working, well-organized, lazy, tough, persistent, strong, disorganized, ambitious, and intelligent) showed differences for those living alone which are supportive of the selection argument. Furthermore, virtually none of the characteristics which it seems reasonable to assume are related to one's desirability as a living companion (sympathetic, helpful, flexible, considerate, and [non]stubborn) showed a relationship with living alone which supports the selection argument. A lone exception to these general findings was the lower degree of reported helpfulness among the widowed who live alone. This should not give much pause, however, as it is likely that those who live alone (particularly the widowed whose roles outside the home may be quite limited because of age) have limited opportunities to be helpful. Furthermore, the rate of reported helpfulness was quite high for both those who live alone and those who do not. Finally, none of the personality measures related to mental health characteristics (i.e., emotional, nervous, frustrated, contented, and self-confident) show any relationship with living alone which

could be construed as supporting the selection argument. With the lone exception noted above, none of the relationships were significant either before or after controls (table on request).

In summary, these data provide no evidence that factors relating to preexisting psychological tendencies play a major role in channeling people who live alone into that living arrangement. In fact, if we look carefully at these findings, we see that persons who live alone have slightly more of the characteristics which would facilitate participation in social and economic spheres than of the characteristics which would inhibit it. Thus, although our measures should properly be viewed as providing only rough assessments of the individual's economic and social viability, the data clearly suggest that in a representative population, a process of social selection related to poor mental health does not appear to play a significant role in determining who will live alone.

MENTAL HEALTH AND LIVING ALONE

In this section we will be dealing with the effects of living alone on two theoretically and empirically distinct sets of indices of mental health, namely, (1) psychological well-being and (2) disruptive behavior. These two sets differ from each other, not only in that one deals with the respondent's affective state and the other with the respondent's behavior, but also in the degree to which each set can be conceptually linked to forms of mental disorders. The treatments for the two kinds of phenomena tend to be quite different, as is the etiology for them (Gove 1968, 1978; American Psychiatric Association 1968, 1978; Klein and Davis 1969, p. 63). This distinction between psychological status and disruptive behavior has significant implications with respect to the importance of social control. Gove and Hughes (1980) have suggested that, while it seems clear from their ecological analysis that living alone is causally related to suicide and alcoholism, it is not clear whether this is because persons who live alone lack close meaningful relations with others (and thus experience excessive stress) or because they lack the simple physical presence of others. It is altogether possible that the social integration resulting from living with others has positive effects, not because the others provide close meaningful social relations, but because the others prevent those they live with from engaging in problematic, maladaptive, or "deviant" behaviors.

MEASUREMENT

It is now very clear that the psychological state of the individual has a number of relatively distinct dimensions and that efforts to measure psychological well-being should use a variety of indices (Bradburn 1969;

Cherlin and Reeder 1975; Gove 1978; Gove, Hughes, and Galle 1979). In our analysis we will use: (1) a scale of psychiatric symptoms; (2) a depression scale (which is in fact a subscale of the full psychiatric symptoms scale); (3) the experience of a nervous breakdown in the past year; (4) a positive affect scale; (5) a manifest irritation scale; (6) a self-esteem scale; (7) a happiness item; (8) a life satisfaction item; (9) a home satisfaction item; (10) a belonginglessness scale; (11) a scale indicative of a bleak outlook on life; and (12) a scale dealing with the sense one has control over one's life.[4] In addition to tapping many of the dimensions which the literature suggests are important, this large set of indices gives us a number of opportunities to find some relationships associated with the state of the individual's psychological well-being.

Our indices of disruption are not related as directly to disruptive behavior (as conventionally conceived) as are our measures of psychological well-being to a theoretical understanding of mental health. We have instead a set of items taken from the interview schedule which measure what could be called problematic or maladaptive behaviors. What makes this set different, overall, from our measures of affect is that these items all refer to specific behaviors which are presumably amenable to social control. The measures are (1) number of drinks of alcohol per month, (2) the presence of alcohol problems in the respondent's life, (3) the use of sleeping pills and tranquilizers, (4) taking excessive risks, and (5) thinking about suicide in the past year.[5]

[4] The psychiatric symptom scale, the positive affect scale, and the self-esteem scale are described in Gove and Geerken (1977). The manifest irritation scale is described in Gove et al. (1979). The depression subscale is composed of three of the depression items from the psychiatric symptom scale. The nervous breakdown item is the proportion of persons in each category who report that during the past year they had felt they "were going to have, or were close to having, a nervous breakdown." The happiness item read, "Taking all things together, how happy are you these days, very happy, pretty happy, not too happy, or not at all happy." The home-life satisfaction and life satisfaction items were worded as follows: "How satisfying do you find your (home) life, very satisfying, pretty satisfying, not too satisfying, or not at all satisfying." The belonginglessness scale is an additive scale of responses to the following items (positive response coded 0 and negative response coded 1): "Most people really do care what happens to the next fellow; Most people are just naturally friendly and helpful; I hardly ever feel awkward or out of place." The "bleak outlook" scale is an additive scale of the following items (positive response coded 1 and negative response coded 0): "I am lonely; I feel as if my life just isn't complete; I feel all alone in the world; My life often seems empty; I feel as if I'm not interested in anything." The control scale is an additive scale of items, coded the same way the bleak outlook items were coded, as follows: "I'm able to do the things I want to do; I seem to have a lot of control over what happens to me."

[5] "Suicide thoughts" is the proportion of persons in each category who report that they have seriously thought of committing suicide in the *past year*. "Sleeping pills/tranquilizers" is a two-item scale coded 2 if the respondent reports taking both sleeping pills and tranquilizers, 1 if he or she takes either, and 0 if he or she takes neither. The alcohol problems scale is a three-item scale on which the respondent scores one point for each of the following he or she reports having experienced in the past year: (1) drink-

Table 3 reports the effects of living alone on (1) the 12 measures of psychological well-being, and (2) the five measures of problematic behaviors. For each measure there is a row of means comparing those who live alone with those who do not for each of the unmarried statuses after controlling for age and sex by MCA. The overall findings of table 3 are summarized in table 4. The most striking thing about this table is that there is no consistent relationship of significant differences. Only two comparisons in the entire table show that persons who live alone are significantly worse off than persons who live with others: home satisfaction and pill taking among the widowed. The second most striking thing about the table is that, in five of the 51 comparisons, those who live alone are significantly better off ($P < .05$) than those who live with others. On the psychological scales (1–12), those who live alone are "better" off (either significantly or not) in 21 of the 36 comparisons. The area in which those who live alone do seem to show evidence of problems is what we are calling problematic behaviors. Here the statistically significant problem is pill taking among the widowed. With the exception of taking risks, all of the other comparisons are in the direction of those living alone being "worse off," but these relationships are not statistically significant (11 out of 15 comparisons). In all marital statuses, those who live alone are *less* likely to take risks and be careless.

Table 4 summarizes the overall findings by marital status. There is a slight tendency for living alone to be worse for the never married than for the widowed and worse for the widowed than for the divorced. It is clear from this figure that for both the widowed and divorced more comparisons indicate a positive relationship between living alone and psychological well-being than a negative relationship. On balance, those who live alone seem to show a slight tendency to engage in more problematic deviant behavior, but as noted above the relationships are not strong, and those who live alone are consistently less likely to take risks.

As noted above, a very substantial body of evidence indicates that marriage is related to the psychological well-being of those who are married, and there are both theoretical and empirical grounds for believing that the major determinant of the relationships is the nature of the marital bond (see Gove, Style, and Hughes 1980). Since unmarried people tend to have poorer mental health than the married, one would anticipate that those

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ing more than planned; (2) failing, because of drinking, to do something that should have been done; or (3) being told that drinking was hurting his or her health. The analysis of this scale includes only those who drink alcohol. The number-of-drinks scale is the estimated number of drinks the respondent consumes each month. The risk-taking scale is a two-point scale of the following items (positive response coded 1 and negative response coded 0): "I sometimes take risks I shouldn't such as driving too fast or other things that might endanger others; I sometimes get careless and have accidents around the house, driving, on the job, etc."

TABLE 3

MEAN SCORES ON MENTAL HEALTH AND PROBLEMATIC BEHAVIOR SCALES BY LIVING ARRANGEMENTS AND MARITAL STATUS, ADJUSTED FOR SEX AND AGE (Married Excluded)

VARIABLE	NEVER MARRIED			DIVORCED/SEPARATED			WIDOWED		
	Not Alone	Alone	Beta	Not Alone	Alone	Beta	Not Alone	Alone	Beta
Mental well-being:									
1. Psychiatric symptoms	6.16	6.62	.043	7.30	6.80	.051	5.02	4.56	.052
2. Positive affect	5.96	6.08	.025	5.36	5.57	.051	4.60	4.02	.094
3. Nervous breakdown	.118	.178	.073	.231	.224	.008	.078	.111	.050
4. Manifest irritation	1.48	1.24	.065	1.58	1.18	.131*	.638	.515	.059
5. Depression subscale	1.31	1.36	.015	1.70	1.39	.099	1.11	.980	.045
6. Self-esteem	6.40	6.37	.007	6.25	6.47	.061	6.27	6.74	.147*
7. Happiness	2.15	2.14	.004	1.84	1.97	.072	1.84	1.86	.011
8. Life satisfaction	2.22	2.13	.059	1.97	2.15	.117*	2.00	2.05	.033
9. Home satisfaction	2.21	2.21	.000	2.14	2.09	.027	2.34	2.08	.148*
10. Belonginglessness	2.05	1.80	.107*	1.91	1.77	.065	1.41	1.55	.062
11. Bleak outlook	.843	1.05	.060	1.29	1.46	.048	1.05	1.43	.109
12. Control	1.15	1.28	.073	1.01	1.07	.040	1.02	1.03	.009
Problematic behaviors:									
13. Suicide thoughts	.050	.074	.044	.081	.089	.013	.020	.020	.001
14. Sleeping pills and tranquilizers	.116	.182	.070	.231	.299	.062	.117	.284	.161*
15. Alcohol problems	.311	.330	.018	.352	.425	.072	.149	.154	.007
16. Number of drinks/month	23.39	31.43	.080	9.42	15.58	.083	41.46	44.02	.017
17. Risk taking	.309	.200	.088	.245	.150	.096	.195	.074	.165*

* Significant at .05 or better.

who live alone—who almost by definition are unmarried—would be in poorer mental health than the married. However, we have shown that in the three unmarried statuses, those who live alone are in essentially the same mental health as those in the same marital status who live with others; because of this, one would suspect that the same relationship would be found if the married were included in the analysis. Nevertheless, an analysis of the entire sample is of value because so far we have been adjusting for age and sex only within a particular unmarried status, and the general pattern of the results might change if the adjustments were based on the characteristics of the sample as a whole. These data, with the adjustments for age and sex for the full sample, including the married, are presented in table 5. It should be noted that the significance tests are for mean differences across the entire variable and so do not necessarily indicate the existence of significant differences between those who live alone and those who do not.

As was expected, the married tend to be in better mental health and to engage in fewer problematic behaviors than those who are not. However, when we compare the effects of living alone with the effects of marriage, for the never married and the divorced/separated, on a majority of the scales those who live alone are either better off than the married or closer to the married than are those who do not live alone. For the widowed, those who live alone are closer to the married in half of the comparisons. As in the previous analysis, in the area of problematic or deviant behaviors, those who live alone do tend to be somewhat worse off, and this effect seems most pronounced in the area of drug and alcohol use. This analysis confirms the earlier analysis presented in table 3. Taken as a whole, the data presented in tables 3 and 5 indicate that, for the vast majority of persons who live alone, living arrangement is not a particularly problematic condition.

Interestingly, those who live alone are consistently better off than the married on the manifest irritation scale (there is no one to get angry with), in self-esteem among the widowed, in control over one's life among the never married, and in risk taking across all marital status categories.

Throughout the analyses presented we have adjusted only for age and sex. In analyzing the data dealing with mental health, analyses parallel to those in tables 3 and 5 were run, controlling for family income, education, and race as well as for age and sex. In this analysis the results were virtually the same as those presented above, although the pattern of those living alone being in slightly better mental health than those in the same marital status living with others was somewhat sharper. This is due primarily to the fact that, although persons who live alone are not deficient in income-earning ability (controlling for marital status), they do tend to have a low "family" income. We also ran all the analyses with an inter-

TABLE 4

SUMMARY OF FINDINGS IN TABLE 3

	MENTAL HEALTH AND WELL-BEING		PROBLEMATIC BEHAVIORS	
	Alone Worse	Alone Better	Alone Worse	Alone Better
Never married.....	Psychiatric symptoms Nervous breakdown Depression subscale Self-esteem Happiness Life satisfaction Bleak outlook	Positive affect Manifest irritation Belonginglessness* Control	Suicidal thoughts Sleeping pills and tranquilizers Alcohol problems Number of drinks	Risk taking
Divorced/separated...	Home satisfaction Bleak outlook	Psychiatric symptoms Positive affect Nervous breakdown Manifest irritation* Depression subscale Self-esteem Happiness Life satisfaction* Belonginglessness Control	Suicidal thoughts Pills Alcohol problems Number of drinks	Risk taking
Widowed.....	Positive affect Nervous breakdown Home satisfaction* Belonginglessness Bleak outlook	Psychiatric symptoms Manifest irritation Depression subscale Self-esteem* Happiness Life satisfaction Control	Sleeping pills and tranquilizers* Alcohol problems Number of drinks	Risk taking*

* Significant at .05 or better.

TABLE 5

MEAN SCORES ON MENTAL HEALTH AND PROBLEMATIC BEHAVIOR SCALES BY LIVING ARRANGEMENTS AND MARITAL STATUS, ADJUSTED FOR SEX AND AGE (Married Included)

		Never Married		Divorced/ Separated		Widowed		
Variable	Married	Not Alone	Alone	Not Alone	Alone	Not Alone	Alone	Beta
Mental well-being:								
1. Psychiatric symptoms	5.06	5.18	5.39	7.30	6.70	6.03	5.88	.165*
2. Positive affect	5.98	5.69	5.69	5.29	5.64	5.05	5.39	.141*
3. Nervous breakdown	.098	.088	.131	.235	.197	.130	.174	.140*
4. Manifest irritation	1.28	1.11	.860	1.50	1.15	1.20	1.10	.095
5. Depression subscale	.975	1.11	1.04	1.69	1.37	1.35	1.28	.173*
6. Self-esteem	6.72	6.45	6.43	6.23	6.44	6.35	6.75	.113*
7. Happiness	2.35	2.14	2.16	1.86	2.00	1.80	1.76	.293*
8. Life satisfaction	2.37	2.26	2.19	1.97	2.20	1.91	1.95	.245*
9. Home satisfaction	2.63	2.26	2.34	2.14	2.14	2.20	1.94	.357*
10. Belonginglessness	1.62	1.87	1.66	1.88	1.76	1.56	1.82	.109*
11. Bleak outlook	.290	.752	.886	1.31	1.36	1.27	1.58	.367*
12. Control	1.17	1.15	1.25	.990	1.12	1.05	1.02	.085
Problematic behaviors:								
13. Suicide thoughts	.019	.029	.044	.093	.063	.053	.041	.124*
14. Sleeping pills and tranquilizers	.133	.139	.193	.247	.264	.090	.273	.130*
15. Alcohol problems	.177	.242	.249	.341	.453	.233	.269	.177*
16. Number of drinks/month	15.54	20.52	25.88	37.13	48.69	18.02	23.07	.207*
17. Risk taking	.201	.221	.103	.236	.183	.284	.151	.071

* Significant at .05 or better.

action term for sex and found no evidence that interaction between sex and living alone predicts mental health characteristics or maladaptive behavior.

CONCLUSIONS

In contrast to what one would anticipate on the basis of the literature on social integration, living alone does not appear to be particularly problematic. Indeed, there is some evidence that it may be a somewhat better living arrangement for those who are not married than is living with others. This makes a great deal of sense if we assume that the benefit of families to members is primarily a benefit to the spouses and minor children and not necessarily to other adult persons in the household. In an analysis not reported here, using data collected in the United States (Chicago), we have found that the presence of "others" living in the household who are not part of the conventional nuclear family is related to an increase in the experience of excessive demands, and to a lack of privacy (Gove and Hughes, unpublished data). Both of these dimensions are related to poor mental health on the part of the respondent (Gove et al. 1979).

In more specific terms, unmarried persons who live with others, if they are widowed, often live with their children who are adults; if they are divorced, they often live with their children who are young; and if they have never married, they often live with parents who still retain a certain amount of control over them. Without describing particular situations, it is obvious that any of these living arrangements may be related to a wide variety of difficult relations among the participants, and these difficult relations are probably related to diminished mental well-being.

Although the factors which contribute to the variation in mental health of those who live alone are probably different from the factors which contribute to the mental health of those who live with others, each living arrangement, depending on the social and economic context, has both benefits and costs. For those who live alone, the benefits seem, from the standpoint of the analysis in this paper, to come very close to outweighing the costs.

The results of this study raise three issues that clearly warrant careful investigation. First, we do not know what factors lead some unmarried persons and not others to live alone. Second, we do not know whether or not persons who live alone compensate in some way for their relative isolation through other behaviors which they find particularly rewarding. For example, persons who live alone may develop supportive friendships which provide an alternative form of social integration to that experienced by most persons. Similarly, persons may find being self-sufficient especially rewarding, so that, for example, the performance of household tasks be-

comes more meaningful and rewarding (a factor that would seem particularly important among the widowed). Third, as it appears that mental health and well-being vary by living arrangement, this study suggests that it may be fruitful to investigate, in a systematic way, the microenvironmental factors responsible for the variation.

In connection with this last point, the most important finding is that these data raise very serious questions about our understanding of social integration. From the time of Durkheim to the present, social integration theorists have largely drawn on very rare events such as suicide, mortality due to cirrhosis of the liver, mental hospitalization, etc., as the prime source of evidence for both inducing and testing the theory. However, although living alone is associated clearly with high rates on these indices, the present analyses indicate that data on these rare events tell us little if anything about the vast majority of persons in ordinary situations. As noted above, social integration theory, both as it was developed and as it has been tested, rests on a tenuous empirical base. Specifically, our analysis clearly suggests that social integration theory may be flawed in two important ways.

First, many of the effects frequently attributed to social integration may not be a consequence of close, intimate, warm social relationships, but simply a consequence of social control. Persons writing about social integration have emphasized the importance of close primary relations for the emergence, stabilization, and production of meaning in the lives of individuals. It is argued that enhanced meaning is responsible for the relatively low rates of suicide and other maladaptive behaviors among persons who are socially integrated. As this study finds that living alone is essentially unrelated to mental health and well-being, but mildly related to maladaptive or problematic behaviors, it may be that many of the effects of social integration are due primarily to social control and not to socially supportive relationships which supposedly create a state of psychological well-being. For example, if persons who live alone and persons who live with others have precisely the same rates of attempting suicide (and/or thinking about suicide), the same probability of drinking too much, the same probability of experiencing psychological distress, then persons who live alone will have higher rates of mortality or institutionalization for these problems simply because there are no persons living with them to intervene before the problem reaches a critical point. Thus, the results of this study suggest that social integration may be linked to certain indicators of well-being through a purely mechanical process involving the simple presence of persons.

Second, perhaps the most basic flaw is that social integration theory focuses on the benefits of social integration and has ignored its costs. As Gove (1972) suggested, and Brocki (1979), Gove and Hughes (1979),

and Gove et al. (1979) have shown, one's family roles may have a negative impact on one's mental health and well-being if the roles themselves are stressful or if they involve excessive demands and obligations. Using the Cumming and Henry (1961) "role count" and "lifespace" indices (measures of social integration very different from ours), Ortega (1979) has found that social integration, while related to higher scores on Bradburn's (1969) positive affect scale ($P < .001$ for both indices), is also related to higher scores on the psychiatric symptom scale used in the present study (although the relationships to the latter scores are not significant). Thus, just as persons may gain substantial satisfaction and personal gratification from family relations, they may also suffer frustration, aggravation, hostility, and repressed anger from being constrained to conform to the obligations necessary to meet socially legitimated demands of others in the household. It is not surprising that the latter process is related to poor mental health.

Just as social integration has opposing tendencies, so does the lack of social constraint among the unmarried. Constraint may be the source of considerable frustration; at the same time, it tends to reduce the probability of problematic or maladaptive behaviors. This appears to be the process Durkheim observed and reported on in *Suicide* (1951). What he did not note is that there is evidence for the negative effects of social constraint in his own data. In an attempt to show that the suicide rate was not explainable by psychological variables, he presented data on both admissions to mental hospitals and suicides for the same countries for comparable years (Durkheim 1951, p. 74). He correctly noted that these rates were inversely related, but the only significance he attached to this fact was the indication that there was no evidence supporting the hypothesis that psychopathology produced suicide. However, viewed from our framework, if we treat the suicide rate as a proxy measure for social integration, then when social integration is strong enough to reduce the suicide rate, it should tend to produce elevated rates of psychological distress.[6]

If the arguments above are correct, social integration theory should focus on the costs and benefits of social integration, paying particular attention to the conditions under which an optimal balance may be reached. As noted above, marriage seems to entail social rewards which balance out the negative consequences of social constraint, commitments, and obligation. It is not clear how this happens or whether marriage is the only living

[6] As Pope notes (1976, p. 213), Durkheim (1951, p. 74) presents data on two groups of nations which partially overlap, although the years are different for the common countries. For "group A," $r = -.14$, significant at .358; for "group B," $r = -.39$, significant at .091; and for the two groups together, $r = -.24$, significant at .142. This finding may be replicated by correlating the rate of admissions to mental hospitals for neurosis for states in the United States in 1960 with the suicide rate in the same states. The correlation is $-.22$, significant at .065; with the age-adjusted suicide rate, $r = -.33$, significant at .01.

arrangement in which it can occur. Gove et al. (1980) have recently described the processes in marriage which are conducive to the emergence, reinforcement, and stabilization of adult identities. They suggest that these processes explain why married persons tend to be in better mental health than unmarried persons and why marital satisfaction and good marital relations are unambiguously related to good mental health. If they are correct, it follows that we should examine carefully the mechanisms in marriage to see how the interaction of identities between spouses leads to an effective balancing of rewards and constraint.

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Hierarchy and Ecological Control in Federal Budgetary Decision Making[1]

John F. Padgett
Harvard University

A central issue for fiscal sociology is the articulation between state and society. Operationally, what is required is the embedding of bureaucratic decision making in history. This article proposes the concepts of hierarchy and ecological control as one possible bridge between organization theorists and macro sociologists. A stochastic process model of federal budgetary decision making within the executive branch is developed and tested using HUD program allocation data from the Johnson administration. The model emphasizes that budgets emerge from the interaction of three levels of organizational decision making, each of which is embedded in a distinct cultural context. Historical application focuses on the impact of the Vietnam War on domestic antipoverty and housing programs.

INTRODUCTION

In 1918, Joseph Schumpeter observed,

> The fiscal history of a people is above all an essential part of its general history. An enormous influence on the fate of nations emanates from the economic bleeding which the needs of the state necessitate, and from the use to which its results are put. . . . Fiscal measures have created and destroyed industries, industrial forms and industrial regions even where this was not their intent, and have in this manner contributed directly to the construction (and distortion) of the edifice of the modern economy and through it of the modern spirit. But even greater than the *causal* is the *symptomatic* significance of fiscal history. The spirit of a people, its cultural level, its social structure, the deeds its policy may prepare—all this and more is written in its fiscal history, stripped of all phrases. He who knows how to listen to its message here discerns the thunder of world history more clearly than anywhere else. . . .
>
> The public finances [therefore] are one of the best starting points for an investigation of society, especially though not exclusively of its political life. . . . Notwithstanding all the qualifications which always have to be made in such a case, we may surely speak of a special set of facts, a special set of problems, and of a special approach—in short, of a special field: fiscal sociology, of which much may be expected. [1954, pp. 6–7]

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This statement has a history which goes back to Jean Bodin ("Financial means are the nerves of the state" [1583]) and to Rudolf Goldscheid ("The budget is the skeleton of the state stripped of all misleading ideologies" [1917]). More recently, the tradition is alive in writers as diverse as James O'Connor (1973) and Daniel Bell (1976). Despite this history, however, it seems safe to conclude, along with O'Connor, that the field of fiscal sociology has been largely stillborn: "The budget remains, in [Schumpeter's] words, a 'collection of hard, naked facts' not yet 'drawn into the realm of sociology' " (1973, p. 4). Most sociologists persist in seeing federal budgetary decision making as a dry, technocratic matter unrelated to their basic concerns.

Fiscal sociology, if ever it comes into existence, should above all be concerned with the articulation between state and society. Embedding organizational decision making in history is the operational objective. Three rather large tasks are involved: (*a*) an institutional analysis of the structure of governmental expenditure and/or taxation policymaking, along with a historical description of its operation and development; (*b*) a "symptomatic" analysis of mechanisms for the representation of external group and/or class interests within the system; and (*c*) a "causal" analysis of the impact of expenditure and taxation policies on a wide variety of social phenomena, such as economic growth, income distribution, the regulation of intergroup and economic relations, and the changing structure of the polity itself.

This article is concerned primarily with the first, secondarily with the second, and not at all with the third of these tasks. I present and test a stochastic process model of U.S. federal budgetary decision making within the executive branch. This model draws heavily on concepts from organization theory and from hierarchy theory. But it also identifies three "ecological control" mechanisms for the routine representation of external interests within the federal budgetary bureaucratic system. Empirical testing focuses on allocational decision making within HUD (the Department of Housing and Urban Development) and within OMB (the Office of Management and Budget) during the Johnson administration. Historical application is to the impact of the Vietnam War on domestic housing and antipoverty programs.

Theoretical Background

Contending theorists have posed the fundamental issue as the relative autonomy of the state. The broadest cleavage within the budgetary literature of political science and economics is between the two metaphors of state as manager and state as reactor. The guiding image of public finance economists is that of a central authority rationally allocating relatively pliant resources in accordance with macroeconomic policy, program policy priorities, and efficiency criteria (Musgrave and Musgrave 1976). Institutionally oriented political scientists, on the other hand, led by the incre-

mentalists, emphasize the structural rigidities induced by bureaucratic politics and standard operating procedures (Lindblom 1961; Wildavsky 1964; Davis, Dempster, and Wildavsky 1966, 1974; Crecine 1969). Despite radical disagreement about the significance of cognitive limitations in the face of budgetary complexity, both schools tacitly agree that the primary focus of analysis should be on dynamics located *within* the governmental system.

Writers in the pluralist and Marxist traditions, on the other hand, have emphasized the dependence of state expenditures on external interest group or class demands. The dominant image of the pluralists, of course, is that of state as broker among competing interests (Bentley 1949), whereas the dominant Marxist image is that of state as instrument for class domination (O'Connor 1973). Despite major disagreement about the locus and structure of political power, both traditions tacitly agree that the primary focus of analysis should be on dynamics located *outside* the governmental system. Each school, however, has struggled for a way to represent more autonomous actions of state decision makers within their formulations (Frohlich, Oppenheimer, and Young 1971; Skocpol 1979).

Within the budgetary literature, debates among these schools have been structured by the fact that quantitative empirical studies to date appear to support the institutionalists' position more strongly than any other. The statistical results of Davis et al. (1966, 1974), in which R^2s in excess of .9 are obtained simply by regressing federal allocation decisions on earlier decisions (the "base"), are well known. Their interpretation emphasized the importance of incrementalist decision rules or "aids to calculation" which induce temporal stability in a structured institutional context of advocate versus guardian roles. Crecine (1969, 1975) has improved on this interpretation by insisting upon the constraining role of macroeconomic fiscal policy, as implemented through a lexicographically ordered set of cutting rules. On the state governmental level, the complementary regression finding is the relative dominance of economic and demographic determinants over political determinants of state allocation outcomes (Dye 1966; Hofferbert 1966; Dawson 1967; Sharkansky 1969). Additional comparative support is alleged by the finding of broad organizational similarities in the budgetary systems of many different nation-states (Wildavsky 1964, 1975; Lord 1973; Campbell 1977).

The dominant image which emerges from this line of research is that of a very inertial and buffered institutional system which extrapolates deterministically from t to $t + 1$ because of organizational stability and bureaucratic standard operating procedures, with perhaps some modification due to aggregate fiscal policy. This overall image does not square well with Schumpeter's vision. At the risk of considerable oversimplification, one could conclude that the answer to the issue of the articulation between state and society is that there is none. The state, in the realm of budgeting at least,

is such a buffered and inertial bureaucratic system that external political demands are reflected within it at best only at the time of legislative birth of new programs and at the extreme margins of discretionary expenditure growth.

Current Research Issues

Part of the problem with this conclusion, I have argued elsewhere (Padgett 1980), is methodological. Regression analyses too often contain an implicit "black box" conception either of the governmental system as a whole or of various stages and actors within the system. Inputs are correlated with outputs, and inferences are sought about the internal structure of decision making within the system. In the area of budgeting, however, many alternative decision processes are at least plausibly consistent with the broad patterns revealed by traditional incrementalist regressions (Padgett 1980). This fact gives rise to a discrepancy between the rigid, bureaucratic image presented in quantitative analyses and the more contextual image presented by qualitative observers who stress the intense disputes and gaming, the ambiguity, and sometimes even the confusion and volatility afflicting the budgetary system (Natchez and Bupp 1973).

Earlier research (Padgett 1980), moreover, revealed the relative superiority of the "serial judgment" model over the incrementalist model of federal budgetary decision making, once the empirical focus shifted away from the time-series regression analysis of absolute expenditure levels into the cross-sectional stochastic process analysis of distributions of allocation change. This behavioral decision model, a member of the bounded rationality tradition (Simon 1957, 1972), emphasized the importance both of ordered search through a limited number of budgetary alternatives and of stochastic "informed judgment" selection of final budget choice. Serial judgment theory presumes contextual sensitivity and temporal flexibility, and it implies the occasional occurrence, on a routine basis, of radical and "catastrophic" change.

Apart from methodological problems, however, a more difficult conceptual issue confronts anyone who is interested in implementing Schumpeter's program. The problem is simply put: on the one hand, state expenditure decisions are made largely by and implemented through formal organizations. This point, so obvious once stated, is recognized but has been insufficiently appreciated by public finance economists, by pluralists, and by Marxists alike, who treat organizations as fairly pliable instruments. On the other hand, by adopting a more historical vantage point, we can perhaps also agree to take as a truism that state expenditures are embedded in the legal, economic, and political structures of society. This point, while recognized, has been insufficiently appreciated by the institutionalists, who emphasize the fairly buffered and self-enclosed character of federal budget-

ing. The problem, of course, is how to meld the organizational and the historical perspectives. Put another way, the problem is the classic sociological one of how to integrate conceptually distinct levels of analysis.

THEORY

To address this problem, first I consider the federal budgetary system from the internal point of view—that is, from the perspective of organization and hierarchy theory (Simon 1969, 1973). In the next subsection, I reexamine the same federal budgetary system from the external point of view—that is, from the perspective of ecological control.

Hierarchy

The necessary anatomical background is as follows. Within the executive branch, there are three major institutional clusters: (*a*) the President, along with his "troika" of economic advisers (the Chairman of the Council of Economic Advisers, the Secretary of the Department of the Treasury, and the Director of OMB), (*b*) the Office of Management and Budget, and (*c*) numerous major domestic[2] departments (e.g., HEW, HUD, Interior, etc.). For budgetary purposes, OMB and domestic departments are internally arrayed roughly in parallel. The OMB is composed of a Director and Deputy Director (along with supporting Office of Budget Review staff), a series of Division Chiefs in charge of broad functional areas (such as the Human Resources division and the Natural Resources division), and a large number (approximately 200) of budget examiners assigned to specific programs. Domestic departments are composed of a Secretary and Under Secretary (along with supporting budget office staff), Assistant Secretaries in charge of program clusters (such as the Renewal and Housing Assistance division and the Metropolitan Development division within HUD), and numerous program chiefs administratively in charge of operational programs (such as the Urban Renewal, Elderly Housing, Model Cities, and Water and Sewer programs within HUD).

Within this organizational complex, one can distinguish at least three functional levels of budgetary decision making. At the most aggregate presidential level, decision making centers on the macroeconomic determination of total federal spending.[3] Fiscal policy and defense or war-related

[2] The institutional budgetary system for the Department of Defense is somewhat different from that on the domestic side. In particular, OMB does not occupy as salient a position (Crecine 1975).

[3] The President himself gets involved in more detailed program allocation decisions only in two circumstances: a few "pet programs" or high saliency cases and annual end-of-year departmental appeals of OMB allowances. While Presidents do differ in their propensities to get involved in such detail (with Johnson being relatively active, and Nixon being exceedingly reluctant), the dollar amounts involved in such direct intervention are trifling in federal budget terms. Cognitive limitations in the face of the complexity of the U.S. federal budget provide one obvious reason for this state of affairs.

issues reign; the outcome is a total domestic spending target, which may change during the course of the planning year.[4] At the intermediate level of OMB and domestic department higher echelons, decision making centers on the distributional determination of relative spending priorities among programs. Political and tactical as well as substantive policy issues reign; the outcome is a set of budget policy guidelines, which structure the inevitable cuts necessary to reach total targets. At the most micro level of budget examiners and program chiefs, decision making centers on the administrative determination of "proper" allocations necessary to fund individual program "needs." Technical, legal, administrative, and efficiency issues reign; the outcome is a set of program funding requests or recommendations which program chiefs and budget examiners each make within their respective organizations.

Bureaucratic conflict exists at each level. Economic advisers differ over fiscal policy, and foreign policy advisers differ over the proper level of defense. The upper echelons of OMB and domestic departments struggle over relative priorities and over the proper interpretation of ambiguous "presidential commitments." The OMB and departmental budget staffs disagree over the proper definition and measurement of program "need." The overall image which emerges from this bureaucratic politics perspective is more that of many different institutional actors pulling simultaneously in many different directions than it is that of the budget as a rational instrument for central planning and control.

From a hierarchical point of view, however, the details of what occurs within each level across thousands of programs and issues are less revealing than the character of the interfaces between levels. Presidential decision making about fiscal policy and defense may be exceedingly complicated and convoluted. However, from the point of view of OMB, all that is produced from this within-level conflict is a single number—either of the form "your overall expenditure target is $500 billion" or of the form "cut $2 billion." The very top echelons of OMB may have some idea about where this number came from, but whether they do or do not is largely irrelevant to their task. Politics has been compressed into a single piece of information.

A similar phenomenon occurs at the very bottom of the hierarchy. Program "needs" are usually assessed and justified in terms of quantitative measures, such as "projected application rates." Applications, for example from mayors interested in initiating new Urban Renewal projects, may be the outcome of highly complicated community decision making. But from the point of view of the federal budgetary system itself, all such politics (and/or demographics and economics) have been aggregated and com-

[4] In addition to overall fiscal policy target decisions, defense/domestic "split" decisions are also made by the President, in conjunction with the National Security adviser and the Secretary of the Department of Defense, with some input from the State Department Secretary and the OMB Director (see Crecine 1975).

pressed into a single piece of information. Lower budgetary officials can adopt a bureaucratic approach precisely because they have no need to pierce beneath the veneer of these inputs.[5]

Hierarchical interfaces also have implications for the structure of alternatives perceived at each level. Starting at the micro program level, individual and discrete cutting alternatives, which lower-level budget officers perceive, are difficult to characterize in general, because they depend contextually on information inputs which are extremely diverse across programs. However, the *distribution* of alternatives that budget officials will perceive, once they start looking, will be heavily dependent on the legal and technical constraints on the program. The dollar sizes of cutting alternatives perceived will be much higher, on average, for the FNMA Special Assistance program, which allows one the option of selling off mortgage portfolio assets, than for the Public Housing program, which requires chipping away at housing units for which contracts have already been let. "Uncontrollability" does not mean that nothing can be done to inhibit expenditure growth;[6] however, such legal constraints on the discretion of the executive are no accident. What on one level appears to be a technical budgetary concept, on another represents the hidden hand of Congress at work. Legislative history and conflict are compressed into a legal structure which shapes, without predetermining, the feasible alternatives perceived by lower-level executive budgetary officials.

Moving now to interfaces within the executive complex proper, the alternatives perceived by upper echelons within departments and OMB are constrained by the decisions of lower levels. Upper echelons are capable of shaping the mix of priorities which are embedded in any department's set of program allocations, both by their promulgation of budget policy guidance to lower budget officials and by their judicious selection from among the cutting alternatives confronting them. However, cutting alternatives per se are generated by the judgment of lower budget levels about what is "feasible." Higher officials are capable of piercing beneath the detailed composition of such alternatives to only a limited degree.

At the presidential level as well, such organizational constraints are operative. Final selections of fiscal policy targets and defense/domestic splits, while based primarily on macroeconomic and foreign policy grounds, are not necessarily made in a complete budgetary vacuum. A limited number of overall federal expenditure "packages" are presented to the President by OMB, and he chooses from among this limited set. Presidential awareness

[5] This is not to argue that budgetary officials are completely blind. Outside knowledge may shape the biases with which such officials approach the evaluation of their information. However, budgetary decision makers' ability to understand exactly what they are dealing with is certainly no greater than that of sociologists.

[6] Most obviously, legal constraints can be altered at any time "simply" by a change in legislation. However, even the executive branch itself can and does cut uncontrollables when it wishes via the device of "reestimating projections" (Crecine et al. 1981).

of the programmatic consequences of this macro choice can be only exceedingly cursory, given the complexity of the federal budget as a whole.

The structural image I have been developing is that of a hierarchically organized, nearly decomposable system (Simon 1969, 1973; Ando, Fisher, and Simon 1963; Crecine 1977). Bureaucratic politics, of the direct interpersonal variety, takes place laterally on three different levels simultaneously. Levels interface within this organizational complex through aggregate targets going down and through alternative-set menus coming up. The deceptively simple reason that these three organizational levels, encompassing such different relevant issues and such different political dynamics, have to relate at all is numerical aggregation—program budget totals add to department budget totals, which add to the overall federal budget total. The meaning of the term "hierarchy" here is much closer to Simon's conception (1973) of a "Chinese boxes" ordering of aggregation levels than it is to Weber's conception of a linear tree of authority relations.

Ecological Control

This internal organizational decision structure gives us a clue about the articulation between state and society and about the possible simultaneous truth of the statements that "the state is a relatively autonomous bureaucratic system" and that "the state is embedded in the legal, economic and political structures of society." That clue is *ecological control*, which may be defined as indirect control over the underlying premises of choice (such as information, alternative sets, targets, and the definition of issues deemed "relevant") rather than direct control over the process of selection itself (Cartwright 1965; March and Simon 1958).[7]

Each aggregation level of organizational decision making is embedded within a distinct cultural context. In the short run, these contexts define the formats of compressed cybernetic inputs. In the long run, they are the historical residues of past political struggles and structural relationships. If we now reexamine the budgetary system from the perspective of the outside looking in, we find that ecological control of the premises of organizational decision making takes three forms. At the micro program level of aggregation, the perception of feasible budgetary alternatives is affected by a program's controllability, which is rooted in the legal structure. At the intermediate departmental level of aggregation, selection from among budgetary cutting alternatives is guided by central organizational authorities' relative program priorities, which are rooted in institutional "missions" and constituency relations. At the macro presidential level of aggregation, both the total size and the distribution of cutting effort demanded from

[7] The term "ecological" is meant to connote the "terrain" of policy choice rather than population biology.

various organizations are controlled by fiscal targets, which are rooted in macroeconomic and defense considerations.

Thus, "culture" at the micro program level is equivalent to the legal structure. In terms of short-term decision making, the legal system affects not the content of the compressed information which organizations perceive (e.g., the level of "projected application rates") but the format of such aggregated inputs (e.g., the fact that they are examining projected application rates in the first place). Speaking metaphorically, culture in this sense defines the window out of which one is looking rather than the detailed image that one sees. Selective perception or "uncertainty absorption" (March and Simon 1958) is one consequence. In the long run, of course, this form of ecological control is rooted in past political struggles within the Congress.

At the intermediate departmental level, "culture" means organizational ideologies or "missions" (Halperin 1974). In the short run, such ideologies affect the definition of what policy issues are deemed "relevant." Bureaucratic conflict between organizations is in large part a struggle over the ideological baseline of "what are the most important issues in the choice at hand?" In the long run, such organizational missions are rooted, at least in part, in constituency relations.

At the presidential level, "culture" in our era means Keynesian economics on the macroeconomic front and "balance of power" or Cold War ideology on the foreign policy front. Keynesian economics not only defines the set of cybernetic economic indicators to monitor but also provides econometric computer simulations to legitimate and to control presidential interpretation of these indicators. Some theorists have contended that an ideological emphasis on macroeconomic stabilization and growth is consistent with the class composition of dominant parties (Hibbs 1977). In this article, however, these three forms of ecological control are not assumed to work in mutually consistent directions.

The Implication of Time

To meld operationally this external ecological control perspective with the internal hierarchical perspective, one has to appreciate the implications of real time for framing conceptually distinct levels of analysis. Program budget decisions are made repeatedly in a time frame of weeks, or even days; fiscal policy targets are set annually in the best of circumstances or, more typically, a few times each year; substantive policy priorities within the executive change on the order of every few years (usually during changes in administration); legal controllability constraints change only after many years; and truly major institutional or ideological shifts occur in a time frame of decades. What in one time frame appears as an ecological premise in another appears as a discretionary choice. Within the context of any one

time frame, moreover, corresponding decisions change discontinuously, not smoothly.

Of course, the federal budgetary system is the residue of historical forces operating in many different dynamic frames. However, both correlational studies of budgetary allocations and qualitative case accounts of specific budget choices will tend to uncover only the most proximate of causes—usually the compacted information inputs which decision makers themselves perceive. The theoretical implications of this are to overstrengthen the image of the state as a highly self-enclosed bureaucratic system, governed exclusively by standard operating procedures and internal bureaucratic gaming, and to undercut the development of Schumpeter's call for a "fiscal sociology."

To make analytic progress, the researcher must self-consciously select a particular historical time frame (along with its implicit definition of the "dependent variable"), thereby consigning lower-frequency dynamics to the role of structural parameters (i.e., ecological premises of decision) and higher-frequency dynamics to the role of stochastic variation.[8] "We will, of course, want to select the [analytic] boundaries so as to make that [dynamic] separation as sharp as possible" (Simon 1973, p. 10).

MODELS AND PREDICTIONS

I now implement my hierarchy and ecological control approach by focusing on the middle level of organizational aggregation—decision making within the higher echelons of domestic departments and OMB. That is, fiscal policy decisions and total targets are taken as exogenously given, and the informational determinants of micro-level cutting alternatives will be subsumed within probability distributions. The issue, then, is the structure of OMB and departmental authorities' attempts to shape departmental budgets to reflect their own substantive or strategic priorities, primarily through the means of central cuts to lower-level requests for increases.

I hypothesize that this decision structure is sequential search. That is, once program priorities and total targets have been determined, central authorities cut the set of program budgets confronting them by focusing their attention first on one program and then on another, cutting each in turn until the aggregate target is achieved. There are two key ingredients. (1) The probabilities of focusing attention on different programs are not equal because of the "mix of priorities" embodied in budget policy guidances (reflected in θ_i below). (2) The expected dollar sizes of cuts to different

[8] The physical science terminology is intentional. "As Melvin Calvin has put it: 'This is one of the most fundamental things we have to teach freshmen: What is the difference between an atom and a molecule? An atom interacts at one energy level and molecules interact at the other, and that is how we tell the difference' " (Simon 1973, p. 9). Statistical mechanics, with its aggregation of molecular interactions through probability distributions and its reconstruction of gaseous dynamics through structural parameters such as temperature, provides the formal prototype.

programs, once attention has been focused on them, are not equal because of heterogeneous legal constraints (reflected in β_i below). Both of these elements—central attention focus and lower-level generation of feasible cutting alternatives—will be modeled as stochastic processes.

Following a brief sketch of the annual planning cycle, analysis proceeds in four parts: (*a*) a formal overview of the decision structure within each cutting stage; (*b*) mathematical specification of propositions regarding attention focus and alternative generation; (*c*) structurally oriented predictions about cross-sectional distributions, for individual budget stages, of both allocation change and numbers of cuts; and (*d*) historically oriented predictions about the time-series cumulation of program-budget means and variances across internal executive stages and about actual values of distributional parameters.

The Planning Cycle Context

Detailed institutional anatomies of the stages in the annual planning cycle for the executive budget are presented in many places elsewhere (Crecine 1977; LeLoup 1977; Padgett 1978), so only the most abbreviated of descriptions will be given here. In the preview stage (May–August), two things happen: (*a*) program chiefs and budget offices within domestic departments formulate a set of requested budget increases (over the previous year's "current estimate"), which are then sent to OMB, usually with only minor central departmental modification; and (*b*) OMB budget examiners, in consultation with their division chief superiors, formulate a set of total departmental budget "ceilings," which are transmitted back to the departments as guidance for the development of their regular fall submissions.[9] These ceiling totals are designed to be consistent with current fiscal policy. At the end of September, departments submit to OMB their formal agency requests. Two types of central cuts may be in evidence here: (*a*) regular, usually modest, cuts from the earlier preview submission to the current fall submission; and (*b*) a progressively more stringent series of "band" cuts, illustrating the manner by which departments would get their budget total below ceiling, if that should prove necessary.[10]

The OMB Director's Review stage[11] occupies October, November, and

[9] The program composition underlying these departmental ceiling totals is usually also transmitted, but on a much more informal basis between OMB examiners and departmental budget officers. Ceiling totals are sent more formally from the OMB Director to the Departmental Secretary.

[10] Departments, of course, frequently resist strenuously the actual implementation of these more severe "band" cuts. This addendum to the regular agency request was a reform of the Johnson administration and, in particular, of OMB Director Charles Schultze.

[11] The Director's Review derives its name from a quasi-judicial hearing in which examiners formally defend their allowance recommendations to the OMB Director and Deputy Director. These central authorities, however, typically make very few modifications.

early December. Examiners cut agency requests in order to derive OMB Director's Review allowances or marks. These program-by-program allowances are constrained to be consistent with the perhaps updated fiscal policy, departmental total, and budget policy guidance instructions which examiners receive from their superiors. After the Director's Review, departments are allowed to appeal some subset of their allowances, first to the OMB director and then, in a smaller number of cases, to the President himself. The proportion of the budget involved in such appeals, however, is typically very small. In addition, in late December OMB may also be confronted with the need to make fairly frantic, last-second "ratchet" cuts. These are triggered by major economic or war uncertainties at the presidential level. The cycle concludes with the submission to Congress of the "President's Budget" in mid or late January.

A sample overview of the history of one program's budgetary changes, over the course of an executive planning cycle, is presented in figure 1.

Focus for now on the two central cutting stages—the departmental fall submission and the OMB Director's Review. An excellent qualitative flavor of the decision problem in the first of these stages is contained in the following quotation from an internal HUD memorandum, entitled "HUD's Budget Priorities for 1969," written by Deputy Under Secretary W. R. Ross on August 8, 1967:

> For the 1969 budget, the general nature of the budget requests prepared by the program offices can best be characterized as the "demand" level. That is, with few exceptions, the totals suggested for each program are closer to the expected application level less an estimated "tolerable" backlog.
>
> The Problem:
>
> 1. To select, for current and anticipated conditions [e.g., Vietnam], the mix of priorities which will persuade the President that his 1969 budget for urban affairs can and must be larger than he will initially believe possible within total budget constraints as he perceives them.
>
> 2. To apply the priorities established to the budget proposals to illuminate the specific policy choices involved in successive reductions from the program manager-proposed levels. [Ross 1967]

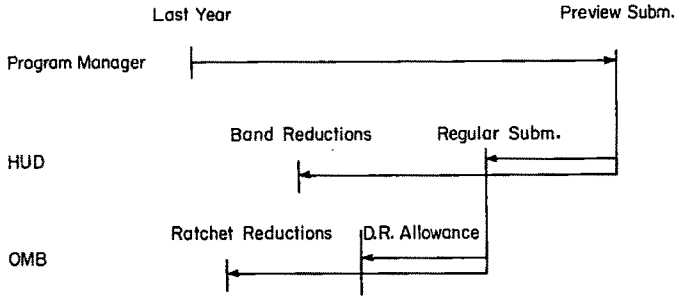


FIG. 1.—Sample structure of annual budgetary change (scale in $)

A Sequential Attention Model of Hierarchical Control

As hypothesized above, the choice structure within which both domestic department and OMB decision makers go about solving such cutting problems is sequential search. Consider the representation of the cutting process given in figure 2. The scheme can refer either to departmental cuts of program-manager requested budgets or to OMB examiner cuts in formal agency submissions. For illustrative purposes only, I shall write primarily in terms of the latter case. Notation is introduced in the order of the actual OMB cutting process. (1) Programs i refer to the constituent elements of a department, the total of which is n (e.g., for HUD, programs are such entities as Urban Renewal, Elderly Housing, Model Cities, Water and Sewer Grants, etc.). (2) The set $\{a_i\}$ is the agency budget submission received by OMB, the departmental total of which is A. (3) B is the total departmental target given to OMB examiners. (4) This departmental target defines the total size of the required OMB cut: $\Gamma = A - B$. (5) The set $\{\gamma_{ik}\}$ is the array of individual dollar cuts which examiners make in order to reach the cutting target, Γ. (6) There is a stochastic number, K, of such cuts necessary to achieve this target objective, c_i of which fall on the individual program i (hence, $K = \Sigma_{i=1}^{n} c_i$). Most important, (7) the existence of only one nonzero entry in each "cutting round" column, k, models sequential cutting, the dollar size of cut in each round being defined

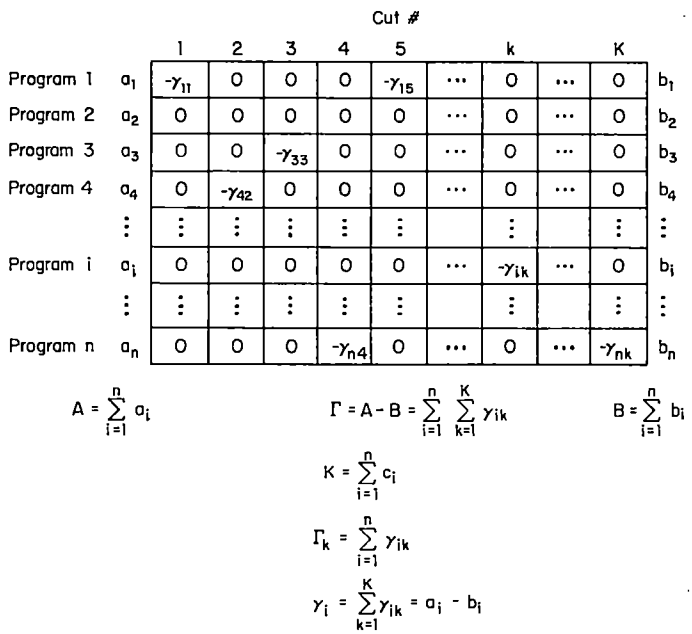


FIG. 2.—Hypothesized structure of cutting within stages

as Γ_k (hence, $\Gamma = \Sigma_{k=1}^{K}\Gamma_k$). (8) The final program budget change produced by this OMB cutting process is $\gamma_i = \Sigma_{k=1}^{K}\gamma_{ik} = a_i - b_i$.

In the next subsection, this framework is implemented first by hypothesizing a particular structure of attention focus across programs, in terms of a set of attention probabilities $\{\theta_i\}$, and then by hypothesizing a particular structure to the process of generating cutting alternatives $\{\gamma_{ik}\}$, in terms of program-distinctive parameters $\{\beta_i\}$ which define "relative controllability."

Before proceeding directly to this task, however, I should highlight that all three levels of budgetary decision making are embedded in the figure 2 formulation. The micro program level is embodied in the generation of cutting alternatives, $\{\gamma_{ik}\}$. View the matrix solely from the perspective of a given row. Descriptively, this perspective corresponds to the development of one program's budget over time. Theoretically, this perspective corresponds to behavioral decision theory. The process is identical to the serial judgment model (Padgett 1980). Discrete "salient" alternatives are encountered sequentially and accepted until an unacceptable state ensues, which is controlled by aggregate fiscal targets. Perceptions of the size of each individual γ_{ik} alternative depend on a variety of contextual detail (such as the dollar size of Boston's Urban Renewal application, unobligated balance carryovers from the previous year, etc.), and hence γ_{ik} can be represented as a random variable (Woodroofe 1975). The parameters governing these perceptual densities are distinctive to each program i because of size and controllability considerations. Such parameters $\{\beta_i\}$ reflect legal, technical, and administrative constraints on the budgetary system.

Now view the matrix solely from the perspective of its columns. Descriptively, this perspective corresponds to the implementation of relative program priorities, at the intermediate departmental level of decision making. Theoretically, organizational control theory is relevant; here this means sequential attention focus (Cyert and March 1963; March and Olsen 1976). Budgetary attention focus is governed, ambiguously and stochastically, by the policy guidance produced by upper echelons in the organization. Political and tactical considerations, as well as substantive preferences, may underlie the short-run determination of these policy guidances, represented by the attention probability vector $\{\theta_i\}$.[12]

Finally, consider only the overall number of columns in the matrix, which describes the number of cuts necessary to achieve the total departmental target. This is where the role of presidential macroeconomic fiscal policy comes in. Targets (B) are received from on high,[13] which in conjunc-

[12] Of course, higher cutting attention probability means lower substantive priority, and $\Sigma_{i=1}^{n}\theta_i = 1$.

[13] More specifically, as outlined above, the chain of causation runs from presidential selection of the total federal expenditure target, on macroeconomic and/or political

tion with the total agency request (A) define the aggregate cutting task (Γ). Given such a fixed "stopping point" (Γ), the number of cuts necessary (K) will be a random variable, since the micro level $\{\gamma_{ik}\}$ and intermediate level $\{\theta_i\}$ are also stochastic. The parameters of the distribution of K, however, will clearly be a function of aggregate totals (B, A, and thus Γ). The "tighter" the fiscal year, the more columns in the matrix, and conversely. Fiscal policy controls how long and hard the system is forced to work rather than the details of what it does.

Figure 2 describes the history of one particular budget stage (e.g., OMB Director's Review), but the concatenation of numerous budget stages within each planning cycle can be represented formally simply by "stacking" analogous matrices onto the ends of one another. Policy conflicts between OMB and HUD can be represented by discrepancies between their two attention probability sets $\{\theta_{Oi}\}$ and $\{\theta_{Hi}\}$.

In this model, fiscal policy affects housing policy not directly or self-consciously but indirectly as it is filtered through the concatenation of many such stages. Speaking colloquially, the relative sizes of the program-manager request total, the departmental request total (A), and the OMB allowance total (B) affect the distribution of "how often the ball is in whose court" and hence the relative degree to which different organizational actors' priorities are reflected in the final president's budget. This consequence of macro-level context for micro-level allocation is one example of ecological control in action.

Mathematical Specification

Only five propositions are required to specify the model completely. The first two of these hypotheses have received support in earlier research (Padgett 1980):

I. Cutting alternatives, γ_{ik}, are generated by a Poisson process (Feller 1968; Coleman 1964). Thus

$$p(\gamma_{ik}) = \text{Exponential}[\beta_i] = \beta_i e^{-\beta_i \gamma_{ik}}.$$

The expectation of this probability distribution is $E(\gamma_{ik}) = (1/\beta_i)$, and β_i is further hypothesized to be the same for both OMB and HUD.

Budgeteers' perception of how much a given program can be cut, once attention has been focused on it, is "unbiased" in the following sense: the probability of perceiving any fixed dollar cut as feasible is independent of how much has been cut from the program in the past.[14] Once departmental

grounds, through the presidential determination of the aggregate defense/domestic split, through the OMB Office of Budget Review decomposition of the resulting domestic total into departmental targets (Crecine 1975).

[14] More technically, for any very small allocation interval $d\gamma_{ik}$, the probability of perceiving one feasible cutting alternative is $p(d\gamma_{ik}) = \beta_i d\gamma_{ik} + o(d\gamma_{ik})$, where $o(d\gamma_{ik})$ means "negligible probability" (Karlin and Taylor 1975).

budget offices or OMB examiners determine that a given program should or has to be cut, they rely only on the contextual programmatic details of what they observe. These highly diverse (and compressed) informational inputs can be considered, from the point of view of the budgetary system itself, essentially as random bombardments from the outside world.

Different programs, however, will differ systematically in the average size ($= 1/\beta_i$) of the cuts so perceived, because each program is subject to different controllability constraints. Such controllability parameters, β_i, will be the same for both OMB and HUD decision makers because they are both confronted with the same legal structure.

II. Controllability parameters β_i are distributed heterogeneously across programs. In particular,

$$p(\beta_i) = \text{Gamma}[\alpha, \delta] = \frac{\delta^\alpha}{\Gamma(\alpha)} \beta_i^{\alpha-1} e^{-\delta\beta_i}.$$

This particular distributional form was selected because of its mathematical tractability and because it can approximate virtually any unimodal reality which exists. It is, therefore, a flexible technical device which allows for realistic legal heterogeneity and which permits more central structural analysis to go forward while postponing investigation into the determinants of the β_i's.

III. Attention focus is structured as a heterogeneous, stationary process. Thus,

$$p(c_i = 1 \mid k) = \theta_i, \quad \text{for all } k.$$

(Here, the event $(c_i = 1 \mid k)$ means "attention is focused on the ith program in cutting round k.")

This is the core hypothesis about attention focusing. It asserts that there exists a fixed set $\{\theta_i\}$ of attention probabilities which is applied over and over again across all the K cuts within any one budget stage. Central budget policy guidance, for each set of $\{\gamma_i\}$ cuts, is the reason. Because of the "mix of priorities" defined by organizational upper echelons, θ_i's are presumed heterogeneous across programs. As described above, however, the relative priorities of HUD and OMB may not be the same. (Hence HUD's set $\{\theta_{Hi}\}$ may differ from OMB's set $\{\theta_{Oi}\}$.)

Heterogeneity of attention probability can be represented by a sample vector drawn from the following multivariate distribution:

IV. $p(\theta_1, \ldots, \theta_i, \ldots, \theta_n) = \text{Dirichlet}[\nu_1, \ldots, \nu_i, \ldots, \nu_n]$.
If, further, $\nu_1 = \nu_2 = \ldots = \nu_i = \ldots = \nu_n = \nu$, then all marginal distributions are:

$$p(\theta_i) = \text{Beta}[\nu, (n-1)\nu] = \frac{\Gamma(n\nu)}{\Gamma(\nu)\Gamma((n-1)\nu)} (\theta_i)^{\nu-1}(1-\theta_i)^{(n-1)\nu-1}.$$

The details of this heterogeneity hypothesis need not detain us, for the reasons discussed under proposition II. The constant ν constraint is necessary in order to avoid the empirical proliferation of ν_i parameters.

The parameter ν has a particular substantive meaning—it represents a type of social norm. The $\nu = 1$ special case can be taken as a random baseline,[15] which can be interpreted thus: "higher-echelon decision makers in every time period reexamine their priorities from scratch, without cross-sectional preconceptions." In contrast, the $\nu > 1$ case represents a distinct normative bias toward equity. Variance of θ_i's goes down continuously with increasing ν, out to the extreme limit of $\nu = \infty$, at which complete homogeneity prevails (i.e., all θ_i's are identical and collapse to $1/n$). The opposite $\nu < 1$ case, on the other hand, represents an extremely selective approach to budgetary decision making, in which some programs are highly favored and others, as a result, are forced to absorb most of the aggregate shock.

Thus the abstract parameter ν can be said to reflect a social norm of selectivity versus equity or, alternatively, an aggressive versus a conservative approach to budgetary decision making. John Campbell (1977) has argued that such budgetary norms are rooted in broader cultural traditions. He found that the Japanese budgetary system was highly skewed in the "balance" or equity direction.

A final, empirically innocuous assumption will be useful in deriving the asymptotic limiting behavior of the system:

V. n and K are both large.

The foregoing sequential attention model of federal executive budgetary control applies (with different parameters) to any OMB cutting stage[16] and to any departmental cutting stage (see fig. 1). The stage ignored so far is the very first program-manager stage, in which (usually substantial) budgetary increases over the previous year are requested.[17] This stage, however, is structurally very simple, as indicated in the quotation above from HUD Deputy Under Secretary Ross. Both top-down fiscal constraints and cross-program considerations of relative priorities are completely absent. Program-manager preview requests are independent of one another and are based simply on what such lower managers feel they will need for the upcoming budget year.

I will, therefore, model this stage (using the notation γ_{Pi} = requested preview increase over the previous year's current estimate) very simply by a set of n independent Poisson processes. Preview-requested increases

[15] The $\nu = 1$ constraint transforms the Dirichlet distribution into the multivariate Uniform special case.

[16] Even last-second OMB "ratchets" can be modeled in this fashion simply by stacking yet another matrix onto the chain.

[17] I will ignore the last appeals stage, even though it is politically heated, since from a dollar point of view it is usually the most trivial of all stages.

represent a pure case of monitoring diverse, program-specific informational inputs from the outside world. There is no particular reason for expecting that preview-increase controllability parameters will be identical to HUD and OMB central-cutting controllability parameters. However, it will be seen empirically that, to a remarkably close approximation,

$$(1/\beta_i)_{\text{Preview increase}} \simeq 2(1/\beta_i)_{\text{HUD/OMB cut}}.$$

With this last preview-increase stage included, the entire federal executive planning cycle, at the intermediate level of aggregation, has been modeled in stochastic process form.

Stochastic Predictions

I now report the more structurally oriented predictions of the ecological control model—cross-sectional distributions for individual budget stages, both of dollar allocation change (theorems 1, 4a, 4b, 5a, and 5b) and of numbers of cuts (theorems 2, 3a, and 3b). All predictions are presented in abbreviated format as theorems. The mathematical derivations of theorems 1–6 from the five propositions above are confined to the Appendix.

Substantive discussion and interpretation of the theorems are postponed until the empirical and the historical sections of the article. To aid understanding in the meantime, the reader may find it helpful to recall how the three aggregation levels of budgetary decision making are reflected in notation: (1) The micro program level generation of cutting alternatives—$\{\gamma_{ik}\}$ or, cross-sectionally, $\{\Gamma_k\}$—is governed by controllability, $\{\beta_i\}$. The two higher-order parameters, α and δ, summarize legal variation in controllability, β_i, across programs. (2) Intermediate departmental level attention focus—reflected in numbers of cuts, $\{c_i\}$—is governed by budget policy guidance, $\{\theta_i\}$. Here, ν is the higher-order parameter (the equity/selectivity social norm) which summarizes "mix of priority" variation in attention focus, θ_i, across programs. (3) Macro presidential level control is reflected in the aggregate departmental cutting target, Γ. The final set of program budgetary changes generated at each stage, $\{\gamma_i\}$, emerges from the interaction of all three levels.

Theorem 1: The probability distribution of all individual cuts *across all programs* (Γ_k), for both OMB and domestic departments, is Pareto. In particular,

$$p(\Gamma_k) = (\alpha/\delta)[1 + \Gamma_k/\delta]^{-(\alpha+1)}.$$

As long as $\alpha > 1$, the average size of all cuts is $E(\Gamma_k) \equiv \bar{\delta} = \delta/(\alpha - 1)$.

Theorem 2: The probability distribution of total number of cuts required (K) approaches a Normal as Γ becomes large, for the case of $\alpha > 2$. In particular,

$$\lim_{\Gamma \to \infty} p(K \mid \Gamma) = \text{Normal}\left[(\Gamma/\bar{\delta}), \left(\frac{\alpha}{\alpha - 2}\right)(\Gamma/\bar{\delta})\right].$$

In the case of $1 < \alpha < 2$, the probability distribution of K approaches a Stable distribution:[18]

$$\lim_{\Gamma \to \infty} p(K \mid \Gamma) = \text{Stable}[\alpha] .$$

For all $\alpha > 1$, $E(K \mid \Gamma) = (\Gamma/\bar{\delta})$.

Theorem 3a: Given a large number of programs (n) and knowledge of relative priority (θ_i), the probability distribution of numbers of cuts (c_i) received by *any one individual program* is a Poisson. In particular,

$$\lim_{n \to \infty} p(c_i \mid \Gamma, \theta_i) = \frac{(\theta_i \Gamma/\bar{\delta})^{c_i}}{c_i!} e^{-(\theta_i \Gamma/\bar{\delta})} ,$$

where $E(c_i \mid \Gamma, \theta_i) = \text{Var}(c_i \mid \Gamma, \theta_i) = (\theta_i \Gamma/\bar{\delta})$.

Theorem 3b: Again for large n, the unconditional probability distribution of numbers of program cuts received (c_i), *across all programs*, is Negative Binomial. In particular,[19]

$$\lim_{n \to \infty} p(c_i \mid \Gamma) = \frac{\Gamma(c_i + \nu)}{c_i! \Gamma(\nu)} \phi^{\nu} (1 - \phi)^{c_i} .$$

The parameter ϕ is a notational simplification of the following: $\phi \equiv (n - 1)/[\Gamma/\nu\bar{\delta} + (n - 1)]$. The first moment is $E(c_i \mid \Gamma) = \nu(1 - \phi)/\phi$.

Theorem 4a: The probability distribution of *any one program's* allocational change during a cutting stage ($\gamma_i = \Sigma_{k=1}^{K} \gamma_{ik} = a_i - b_i$ in the example above), given knowledge of that program's controllability (β_i) and of its relative priority (θ_i), is the following intractable Compound Poisson, with singularity at the point of no change:

$$p(\gamma_i \mid \Gamma, \beta_i, \theta_i) = \begin{cases} e^{-(\theta_i \Gamma/\bar{\delta})} & , \quad \gamma_i = 0 \\ \sum_{c_i=1}^{\infty} \frac{\beta_i^{c_i}}{\Gamma(c_i)} \gamma_i^{c_i - 1} e^{-\beta_i \gamma_i} \frac{(\theta_i \Gamma/\bar{\delta})^{c_i}}{c_i!} e^{-(\theta_i \Gamma/\bar{\delta})} & , \quad \gamma_i > 0 . \end{cases}$$

The moments of this distribution, however, are simple:

$$E(\gamma_i \mid \Gamma, \beta_i, \theta_i) = (\theta_i/\beta_i)(\Gamma/\bar{\delta}) ;$$

$$\text{Var}(\gamma_i \mid \Gamma, \beta_i, \theta_i) = (2\theta_i/\beta_i^2)(\Gamma/\bar{\delta}) .$$

Theorem 4b: The probability distribution of allocational change during a cutting stage, *across all programs*, is exactly a Pareto distribution with singularity at the origin, for the baseline case of $\nu = 1$:

$$p(\gamma_i \mid \Gamma) = \begin{cases} \phi & , \quad \gamma_i = 0 \\ (1 - \phi)(\alpha/\delta)\phi[1 + (\phi/\delta)\gamma_i]^{-(\alpha+1)} & , \quad \gamma_i > 0 . \end{cases}$$

[18] The Stable distribution has no closed-form representation, except in a few special cases, so that it cannot be written out here. In the case of $\alpha = 2$, the Stable is the Normal; in the case of $\alpha = 1$, the Stable is the Cauchy (see Feller 1971).

[19] Unfortunately, notation here is potentially confusing: $\Gamma(\nu)$ means the gamma function, which is a continuous form of the factorial (Taylor and Mann 1972), whereas Γ is our aggregate fiscal target variable.

The expectation of this distribution is $[\Gamma/(n-1)]$, and the variance is infinite whenever $\alpha < 2$.

For more general ν, a closed-form solution does not exist. However, an excellent Pareto approximation, which preserves the correct expectation and singularity, is as follows:

$$p(\gamma_i|\Gamma) = \begin{cases} \phi^\nu & , \quad \gamma_i = 0 \\ (1-\phi^\nu)(\alpha/\delta)\Lambda(\phi/\nu)[1+\Lambda(\phi/\nu\delta)\gamma_i]^{-(\alpha+1)} & , \quad \gamma_i > 0 . \end{cases}$$

The parameter Λ is a notational simplification of the following: $\Lambda \equiv (1-\phi^\nu)/(1-\phi)$.

Theorem 5a: In the far simpler preview case of program-manager requested increases over the previous year (γ_{Pi}), the probability distribution for *any one program* is the following Exponential:[20]

$$p(\gamma_{Pi}|\beta_i) = (\beta_i/2)e^{-(\beta_i/2)\gamma_{Pi}} .$$

Theorem 5b: The *cross-sectional* preview-increase result is a Pareto:

$$p(\gamma_{Pi}) = (\alpha/2\delta)[1+\gamma_{Pi}/2\delta]^{-(\alpha+1)} .$$

Moment and Parameter Predictions

The foregoing predictions about cross-sectional distributions of allocation change and of number of cuts will be most useful in assessing the structural validity of the model. It remains to be shown, however, how the various budget stages fit together over time. There are two sides: (*a*) On the micro program level, means and variances of individual program budget changes over the entire annual planning cycle are presented (theorem 6). (*b*) On the intermediate departmental level, the actual parameter values implied in the allocation change distributions are highlighted (theorem 7). Theorem 6, in other words, embeds annual program budgets in the higher departmental and presidential levels of decision making. Theorem 7 embeds departmental budgets in the higher presidential level of decision making.

Theorem 6: The entire executive planning cycle, including preview increases, departmental cuts, and OMB cuts (see fig. 1), can be summarized for individual programs i in terms of moments. The following notation is required: $\gamma_{Fi} \equiv$ final President's Budget change for program i relative to the previous year; $\theta_{Hi} \equiv$ departmental (e.g., HUD) priority for program i; $\theta_{Oi} \equiv$ OMB's priority for program i; $\Gamma_H \equiv$ departmental aggregate cut of preview increases; and $\Gamma_O \equiv$ OMB aggregate cut of departmental requests.

The moment results, presented in terms of aggregate changes,[21] are as

[20] Here I anticipate the empirical result cited above.

[21] The same results, presented in terms of OMB totals (B), departmental totals (A), and previous year totals (CE), are as follows:

$$E(\gamma_{Fi}) = [2(1-n\theta_{Hi}) + \theta_{Hi}(A-CE)/\bar{\delta} - \theta_{Oi}(A-B)/\bar{\delta}](1/\beta_i)$$

$$\mathrm{Var}(\gamma_{Fi}) = 2[2(1+n\theta_{Hi}) - \theta_{Hi}(A-CE)/\bar{\delta} + \theta_{Oi}(A-B)/\bar{\delta}](1/\beta_i)^2 .$$

follows:

$$E(\gamma_{Fi}) = [2 - \theta_{Hi}(\Gamma_H/\bar{\delta}) - \theta_{Oi}(\Gamma_O/\bar{\delta})](1/\beta_i)\,,$$

$$\mathrm{Var}(\gamma_{Fi}) = 2[2 + \theta_{Hi}(\Gamma_H/\bar{\delta}) + \theta_{Oi}(\Gamma_O/\bar{\delta})](1/\beta_i)^2\,.$$

Theorem 7: The Pareto distribution implied by theorem 4b is most simply expressed in its empirically estimable form:

$$p(\gamma_i|\Gamma) = \begin{cases} S & , \quad \gamma_i = 0 \\ (1 - S)\alpha D[1 + D\gamma_i]^{-(\alpha+1)} & , \quad \gamma_i > 0\,. \end{cases}$$

However, the following parameter predictions are also embedded in the ecological control model:

$$a)\ S = \phi^{\nu} = \left[\frac{(n-1)}{\Gamma/\nu\bar{\delta} + (n-1)}\right]^{\nu};$$

$$b)\ D = \Lambda(\phi/\nu\delta) = \left\{1 - \left[\frac{(n-1)}{(\Gamma/\nu\bar{\delta}) + (n-1)}\right]^{\nu}\right\}\left[\frac{(n-1)\bar{\delta}}{\Gamma\delta}\right];$$

and

c) the α's estimated from data on distributions of allocation choice by budget stage, $\{\gamma_i\}$, are constant across stages and equal to the α estimated from micro-level data on the distribution of individual cuts, $\{\gamma_{ik}\}$.

The reader interested solely in the historical and policy implications of these results is encouraged to skip directly to the last section of this article. Empirical analysis is next.

TESTING AND INSTITUTIONAL INTERPRETATION

After a brief description of the data, empirical testing of the ecological control model proceeds in four parts. First, predictions about the overall cross-sectional pattern of dollar allocation change by stage (theorems 4b and 5b) are assessed.[22] The subsequent two parts focus on underlying structural components of the model: probability predictions about the micro-level generation of cutting alternatives (theorem 1) and about the intermediate level of attention focus (theorem 3b). Finally, historical data on fiscal targets are used to generate point predictions about the parameters governing the distributions of allocation change (theorem 7).

Data Description

The model is tested using data on the allocation decisions for HUD programs, made within OMB and HUD during the Johnson administration

[22] A methodological justification for the empirical analysis of allocation change, rather than of absolute budget level, is presented in Padgett (1980).

(i.e., fiscal years 1967–70, the executive planning for which occurred in calendar years 1965–68). Since this information is not available in the public record, data were coded from internal governmental planning documents, contained in the Crecine OMB archive[23] and in the Padgett HUD archive,[24] and compiled under the legal auspices of the Freedom of Information Act.

The full list of budget stages analyzed in this article is presented in table 1. Each item on the list corresponds to a vector of from 22 to 47 HUD program allocations. Allocations were measured in terms of new obligational authority (NOA), which is the executive-branch equivalent of congressional appropriations. Cross-sectional predictions about fiscal allocation change (theorems 4b and 5b) are assessed in terms of the "change from base" format illustrated in figure 1. More disaggregate theorems 1 and 3b predictions rely on the entire set of table 1 allocation vectors. The level of disaggregation in the full data set, from which the table 1 items were drawn, is unusual in that virtually a week-by-week reconstruction of the evolution of internal executive-branch decision making regarding HUD over a 14-year period (FY 1957–70) is feasible.[25]

One preliminary note about the definition of "programs": All theorems are evaluated using data in their pristine, disaggregate format—that is, "programs" are operationally equivalent to all line items in which the "base" is nonzero, exactly as they appear in formal planning documents. Even within one year, the number of HUD programs facing OMB may be smaller than the number of HUD programs facing HUD, since HUD may have already eliminated some line items. Across years, the number of HUD programs, so defined, fluctuates considerably owing to birth and death both of proposals and of actually operative programs. For purposes of $\{\beta_i\}$ and $\{\theta_i\}$ parameter estimation only, therefore, a more consistently aggregated

[23] The Crecine archive is a rich collection (approximately 120,000 documents) of executive-branch fiscal policy and budgetary memoranda, spanning the Truman through the Nixon administrations. The archive focuses primarily on the Office of Budget Review within OMB but is supplemented with material from the presidential libraries and from the OMB program divisions. The research team of John P. Crecine, George Galloway, Mark Kamlet, David Mowery, Douglas Neal, John F. Padgett, and Chandler Stolp collected these documents. The National Science Foundation (SOC72-05488, SOC76-01052) funded the project. The assistance of OMB archivists Donald Street and Melvin Margerum is gratefully acknowledged.

[24] The HUD archive, which I assembled under the financial sponsorship of the Department of Housing and Urban Development (H-2368G), is a collection (approximately 10,000 documents) of budgetary memoranda drawn from the HUD budget office historical files. These documents span the Eisenhower through the Johnson administrations. The assistance of Roger Henderson within HUD is gratefully acknowledged.

[25] In the full data set, 1,300 allocation vectors have been coded, which span the various budgetary formats (NOA, expenditures, program levels, and operating expenses). In addition to major items, such as those listed in table 1, numerous interim revisions, addendums, rejected alternatives, and the like have been coded.

TABLE 1

BUDGET DATA ANALYZED

Budget Stage	FY 1967	FY 1968	FY 1969	FY 1970
HUD preview.....	Preview current estimate HUD preview submission	Preview current estimate HUD preview submission	Preview current estimate Assistant Secretary "B" recommendation	Preview current estimate Proposed preview estimate
HUD regular submission..	HUD regular submission	HUD regular submission	High BRC alternative HUD regular submission (middle BRC alternative)	Revised BRC estimate HUD regular submission
HUD band reduction.....	Lower priority band Second priority band Third priority band Fourth priority band	Band I Band II Band III Band IV/V	First approximation to OMB planning figure Adjusted to planning figure (low BRC alternative)	Possible additional reductions to reach planning figure
OMB Director's Review...	OMB division recommendation OMB allowance ("A" mark)	OMB division recommendation OMB DR allowance	OMB division recommendation OMB DR allowance	OMB DR allowance
OMB ratchets.....	OMB "B" mark	Possible OMB reductions below allowance	Possible major reductions	

NOTE.—DR = Director's Review, BRC = budget review committee.

set of 25 "standard" programs was constructed to enable meaningful time-series pooling across the four years studied.[26]

One modification of the disaggregate program data proved necessary to remove "spillovers" from the preview-increase stage to the HUD regular submission stage. Occasionally, events occur within this two-month period which necessitate an upward revision in the program-manager requests. These last-minute upward revisions are folded into the very beginning of the HUD cutting process. Such revisions were treated for purposes of analysis as having occurred during the immediately preceding preview stage. Therefore, "HUD central cuts" data sets consist only of the actual budgetary reductions observed.

Cross-sectional Patterns of Allocation Change

The most basic prediction of the ecological control model is theorem 4b, which aggregates into allocation choices the interaction of all three levels of federal budgetary decision making. Theorem 4b posits that the cross-sectional distribution of HUD or of OMB cuts will be Pareto, with singularity at the point of no change. Mathematically and graphically, this probability distribution prediction can be expressed either in density or in cumulative format, as is illustrated in figure 3. In these graphs the more familiar Exponential distribution is also presented for visual calibration.

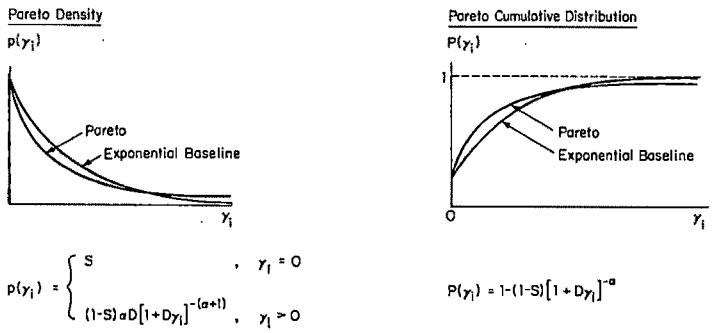


FIG. 3.—Theorem 4b hypothesis about the cross-sectional distribution of HUD or OMB cuts; S and D are defined in theorem 7.

[26] The procedures I employed in constructing these 25 aggregated programs were as follows. (*a*) Proposed legislation supplements to existing programs were folded into the programs themselves (e.g., the aggregated "Urban Renewal" program consists of the Urban Renewal regular budget estimate plus any proposed Urban Renewal legislation). (*b*) Small line items clearly derivative of larger existing programs were aggregated (e.g., Urban Fellowships was clustered with the Community Development Training program). (*c*) All Administrative Expense line items were grouped together. (*d*) All unaccounted for items of proposed legislation were aggregated into a residual Minor/Miscellaneous legislation category. An effort was made to maintain as many programs as was possible, consistent with the constraint of having nonzero data across all years. The final set of aggregated programs is presented in tables 5, 7, and 8.

The Pareto distribution has embedded within it two qualitative observations. (*a*) Most programs most of the time receive fairly small cuts. As noted in Padgett (1980), this prevalence of marginal change is consistent with the regression results of the incrementalists (Davis et al. 1966, 1974). (*b*) The equally characteristic "fat tail" of the Pareto, however, describes the less frequent, but nonetheless routine, generation of radical and even "catastrophic" change. This more volatile pattern is the structural consequence of controllability and policy priority heterogeneity, which induces differential sensitivity among programs to aggregate fiscal and foreign policy events. The graphical effects of more restrictive or tighter fiscal targets are two: the program-level Pareto distribution is spread out to the right, and the size of the "no cut" singularity is decreased.

Goodness of fit was measured with the Kolmogorov-Smirnov statistic, which is the maximum absolute deviation or error of the empirical cumulative distribution from the theoretical cumulative distribution (Bickel and Doksum 1977). Hence, the lower the statistic, the better the goodness of fit. (The alternative χ^2 statistic was not employed since this approach loses information through grouping.) The parameters α, D, and S were estimated using maximum-likelihood procedures, the algorithms of which are available upon request.

The results are reported in table 2. At least three different approaches can be taken to assess the accuracy of the fits.

a) An orthodox but not very powerful approach (Simon 1968) is to rely on significance tests. Significance levels were calculated, for the case of distributions with estimated parameters, using the Monte Carlo results reported in Bickel and Doksum (1977, p. 381).[27] The Pareto hypothesis could never be rejected, at the .05 level, for any stage either in the case of HUD cuts or in the case of OMB cuts.

b) A "critical test" approach is to compare the model explicitly against a competing null alternative. The competing incrementalist model has already been rejected (Padgett 1980). In the present analysis, therefore, the competing null alternative selected was the Exponential distribution, with singularity at the origin. This null distribution is a strenuous one, in the light of the strong graphical similarity of the Exponential and the Pareto. The two-parameter Exponential was also estimated using maximum-likelihood techniques. Table 2 reveals that the Pareto prediction is superior in all cases.

c) A final contextual method is simple visual inspection. The budgetary stage possessing the largest number of program observations is presented in figure 4. The data are graphed in cumulative step-function form, and

[27] These significance values are exact for the case of the Normal distribution, but are approximations otherwise. Unfortunately, no Kolmogorov-Smirnov Monte Carlo simulation results for the case of the Pareto could be located in the literature.

TABLE 2

TEST OF THEOREM 4b: KOLMOGOROV-SMIRNOV STATISTICS

	Pareto Prediction	Exponential Null
	HUD Cuts	
1967:		
Regular (25)	.07117*	.15782*
Bands (25)	.08937*	.18261
1968:		
Regular (42)	.04071*	.09148*
Bands (42)	.06864*	.16190
1969:		
Regular (47)	.04236*	.27655
Bands (47)	.05999*	.24709
1970:		
Regular (41)	.06995*	.19710
Bands (41)	.04965*	.18631
	OMB Cuts	
1967:		
Director's Review (22)	.06459*	.17742*
Ratchet (22)	.08244*	.32316
1968:		
Director's Review (41)	.08738*	.20483
Ratchet (41)	.08259*	.26521
1969:		
Director's Review (38)	.07173*	.16731
Ratchet (38)	.06791*	.19500
1970:		
Director's Review (38)	.05369*	.29019

NOTE.—N's in parentheses.

* Hypothesis cannot be rejected at .05 level of significance.

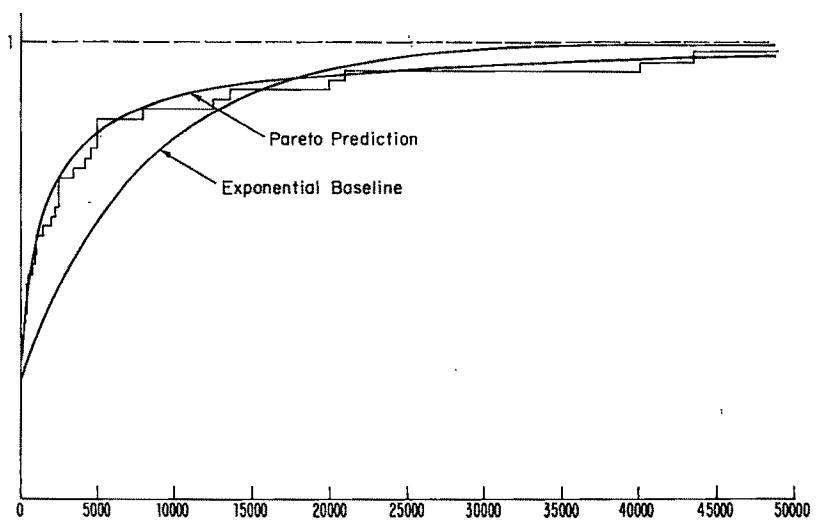


FIG. 4.—Sample data set: 1969 HUD band reductions ($N = 47$; scale in units of $10,000).

continuous theoretical predictions are indicated. Visually, the accuracy is clear.

Therefore, even though density parameters vary considerably over time, (as will be shown in another section), the Pareto structural form predicted by the ecological control model is remarkably consistent across both years and organizations. Elsewhere (Padgett 1980) I present results which indicate that these HUD program findings generalize both to all domestic departments and to the Eisenhower and Kennedy administrations.[28]

The allocation change prediction for the first preview stage (program-manager requested increases over the previous year) is also a Pareto but without singularity (theorem 5b). Program managers independently develop projections of program "need," unconstrained by aggregate fiscal policy or relative priority considerations. The empirical implication is that Pareto parameters, for this stage only, should be constant across years.

Kolmogorov-Smirnov statistics and maximum-likelihood parameters for the preview stage are presented in table 3. There was a slight but persistent tendency for no increase to be requested; therefore, goodness-of-fit statistics are presented for both the singularity and the no-singularity cases. Except for this observed but unpredicted singularity,[29] the hypothesis is confirmed. Nonzero preview increases are distributed as Pareto, and preview parameters appear to be roughly constant, within sample fluctuation, across the four years studied.

The ecological control model is, therefore, confirmed at the level of allocation choice. I now look beneath these decision outcome results at underlying structural components of the model.

Cross-sectional Patterns of Cutting Alternatives

Cutting alternatives perceived by central organizational authorities are generated by lower budgetary personnel (examiners in OMB and budget officers in HUD). The decision structure operative at this lower level is hypothesized to be serial judgment (Padgett 1980). The consequence of this hypothesis (theorem 1) is that the probability distribution of all individual cuts across all programs is Pareto, for both HUD and OMB. The HUD and OMB parameters, moreover, should be identical because the legal structure confronting both organizations is the same.

Kolmogorov-Smirnov statistics and maximum-likelihood parameters are presented in table 4. The data underlying this analysis are all the individual program cuts (γ_{ik}) observed over the entire time period, disaggregated by

[28] In Padgett (1980), empirical analysis was performed on program data in percentage change format, rather than in the dollar change format employed here. Consistency of results across these formats is to be expected, since all that is involved is a rescaling of the β_i parameters.

[29] One possible explanation of this error is that program managers are behaving like unconstrained serial judgment decision makers (Padgett 1980).

TABLE 3

TEST OF THEOREM 5b: KOLMOGOROV-SMIRNOV (K-S) STATISTICS AND PARAMETER ESTIMATES

PREVIEW INCREASES	K-S STATISTICS			PARAMETER ESTIMATES			
	Pareto without Singularity	Pareto with Singularity	Exponential Null	Fraction in Singularity	$\hat{\alpha}$	$\hat{1/2\delta}$	Sample Mean (in Units of $10,000)
1967 (25).....	.12000*	.12566*	.34052	.12000	.56483	.0014603	9,857.719
1968 (42).....	.14286	.08556*	.32811	.14286	.72843	.0011948	6,417.617
1969 (47).....	.13873	.08244*	.31825	.12766	.63404	.0015435	6,980.211
1970 (41).....	.10768*	.09425*	.28583	.09756	.80216	.0007782	7,617.559

NOTE.—*N*'s in parentheses.

* Hypothesis cannot be rejected at .05 level.

TABLE 4

TEST OF THEOREM 1: KOLMOGOROV-SMIRNOV (K-S) STATISTICS AND PARAMETER ESTIMATES

DISAGGREGATED CUTS	K-S STATISTICS	PARAMETER ESTIMATES		
		$\hat{\alpha}$	$1/\hat{\delta}$	$\hat{\delta}$
HUD cuts: FY 1967–70 (162).....	.05928*	1.13734	.0005960	4,803.207
OMB cuts: FY 1967–69 (101).....	.06786*	1.15124	.0005647	4,896.344
Pooled HUD and OMB cuts (263)..	.06189	1.14271	.0005837	4,838.977

NOTE.—N's in parentheses.
* Hypothesis cannot be rejected at .05 level.

the cutting rounds listed in table 1. (The FY 1970 OMB Director's Review data could not be used in this analysis, since my documentary sources did not decompose these final choices into a series of successive iterations.)

The alternative generation prediction of theorem 1 is confirmed. Disaggregated cutting alternatives are distributed as Pareto, and HUD parameters are identical to OMB parameters. Because of this last identity, the HUD and OMB data sets were merged in order to generate a set of final parameter estimates, which will figure in the aggregate parameter predictions to come.

These micro-level findings about cutting alternatives help to explain the cross-sectional patterns of allocation change observed in the previous subsection. Final budgetary decisions made by higher organizational authorities are distributed as Pareto because the alternatives being presented to them by lower budgetary officials are distributed as Pareto, because of the serial judgment structure of alternative generation. The ecological terrain of one organizational level's decision problem is shaped in part by the choices of the next level down.

To probe further into the structure of cutting alternatives, information about the controllability parameters (β_i) governing each program is required. Given the Exponential proposition I and given micro-level cuts disaggregated by cutting round, maximum-likelihood estimation (MLE) of such parameters is straightforward: MLE $(1/\hat{\beta}_i)$ is simply the sample average of program i's cuts over the entire four-year period (assuming that the legal structure for program i has not changed during this period).

The final set of $(1/\hat{\beta}_i)$ parameters, based on the aggregated program definitions described above (n. 26) and on pooled OMB and HUD cuts, is presented in table 5. Programs in the table are rank ordered according to their relative degree of controllability: the larger the average size cut, the more controllable the program, measured in raw dollar terms. Put another way, programs are rank ordered according to the relative sensitivity of their budget estimates to aggregate macroeconomic or foreign policy events

TABLE 5

CONTROLLABILITY PARAMETERS

Programs	$(1/\hat{\beta}_i)$ = Average Size Cut (in Units of $10,000)	Observed Cuts (N)
Urban Renewal (including Urban Renewal legislation)	35,000	4
Federal National Mortgage Association	28,689	9
Model Cities Supplemental Grants	28,333	6
College Housing	25,000	6
Model Cities Urban Renewal Addon	22,500	4
Water and Sewer Grants	10,095	21
Open Space	5,363	11
Urban Transportation	4,454	13
Elderly Housing	3,960	20
Minor/Miscellaneous Legislation	3,107	7
Metropolitan Development Incentives	2,632	9
Neighborhood Facilities	2,216	16
Rehabilitation Loans	1,856	16
Participation Sales Insufficiencies	1,565	1
Urban Planning	1,507	18
Rent Supplements	1,350	8
Model Cities Planning	1,300	4
Public Works Planning	1,142	12
Public Housing Annual Contributions	1,100	5
Research	997	11
Community Development Training	683	9
Advance Acquisition	671	7
Urban Information and Technical Assistance	379	7
Administrative Expenses	359	12
Alaska Housing	200	5

(in the absence of any explicit policy intervention to the contrary). Further interpretation is provided in the historical section.

One empirical regularity is worthy of note. The controllability parameters reported in table 5, which are based on HUD and OMB central cuts, relate in a very simple way to the analogous parameters governing preview increases. When the average size of program-manager requested increases is regressed on the average size of central authority cuts for all 25 programs (with zero intercept constraint), the result is as follows:

$$(1/\hat{\beta}_i)_{\text{increase}} = 1.8215 \cdot (1/\hat{\beta}_i)_{\text{cut}}\,; \quad R^2 = .9299\,.$$

When the one outlier, Urban Renewal, is excluded,[30] the result is:

$$(1/\hat{\beta}_i)_{\text{increase}} = 1.9841 \cdot (1/\hat{\beta}_i)_{\text{cut}}\,; \quad R^2 = .9481\,.$$

I therefore simplify the model by $(1/\hat{\beta}_i)_{\text{increase}} = 2 \cdot (1/\hat{\beta}_i)_{\text{cut}}$.

[30] Historically, the fact that Urban Renewal is an outlier is not surprising. Although there was not as strong a push for preview increases as would be expected from the regression alone, most of Urban Renewal's increase during this period was obtained "backdoor" through the Model Cities Urban Renewal Addon program. Such large quantities of funds were being received indirectly that less was required in the older mainline program.

Cross-sectional Patterns of Attention Focus

At the intermediate departmental level of aggregation, the model hypothesizes that hierarchical control is structured by a process of sequential search. Cutting attention shifts successively from one program to another until fixed fiscal targets are attained. Attention focus is structured, but only stochastically, by a set of budgetary policy guidelines, which are operationalized as a heterogeneous set of attention probabilities $\{\theta_i\}$.

The relevant probabilistic prediction is theorem 3b, which posits that the distribution of observed numbers of cuts across programs will be a Negative Binomial. As indicated above, ν is the equity/selectivity social norm which governs the degree of heterogeneity in policy discrimination pursued by decision makers. When $\nu = \infty$, all θ_i variation collapses into homogeneity (i.e., all $\theta_i = 1/n$), and the Negative Binomial heterogeneous prediction is transformed into the following Poisson:

$$p(c_i|\Gamma) = \frac{(\Gamma/n\bar{\delta})^{c_i}}{c_i!} e^{-(\Gamma/n\bar{\delta})} .$$

Data are presented in the form of histogram arrays, one for each year by organization, of numbers of programs receiving c_i cuts. These arrays are feasible because, as predicted, most of the time most program budget estimates do not change within organizations from one cutting round vector to another. The maximum number of program cuts observable is the number of cutting round vectors listed in table 1. Since ν was presumed constant within administrations, because of institutional and personnel stability, maximum-likelihood estimation of ν parameters (one each for OMB and HUD) was based on cross-year pooled data sets. That is, for ν estimation only, HUD histogram arrays across FY 1967–70 and OMB histogram arrays across available FY 1967–69 were collapsed. The results are: HUD $\hat{\nu} = 5.9$, OMB $\hat{\nu} = \infty$. Thus, from the aggregate perspective of numbers-of-cuts data alone, OMB appears to adopt such an extreme equity stance that policy priorities are hard to perceive. In its approach to central cutting, HUD is more clearly selective.

Table 6 reports the observed histogram arrays along with Negative Binomial (HUD) and Poisson (OMB) fits. These fits are based on only the one year-by-year free parameter: $\mu \equiv E(c_i|\Gamma)$. The goodness-of-fit statistic employed is χ^2, which was calculated only over the truncated space of feasible observations.

On the basis of χ^2 significance tests, the theorem 3b prediction can be rejected in only one of the seven cases—OMB cuts during FY 1968. Virtually all of the OMB cuts observed in this year were clustered in one cutting round (division recommendations), which created a severely peaked distribution. This anomaly frustrated my observational capacity to perceive 1968 OMB cuts in their most pristine disaggregate form.

The puzzle about why OMB appears so much more egalitarian ($\hat{\nu} = \infty$)

TABLE 6

TEST OF THEOREM 3b: NEGATIVE BINOMIAL (HUD) AND POISSON (OMB) HISTOGRAMS OF CUTS

A. HUD: $p(c_i|\Gamma)$ IS NEGATIVE BINOMIAL[a]

	1967		1968		1969		1970	
CUTS (c_i)	Actual	Predicted	Actual	Predicted	Actual	Predicted	Actual	Predicted
0.....	10	9.9256	18	19.6696	13	12.8867	18	16.9738
1.....	10	8.4871	17	14.0018	13	14.9729	11	13.9042
2.....	2	4.2436	4	5.8283	11	10.1727	8	6.6600
3.....	1	1.6195	3	1.8518	7	5.2754	4	2.4350
4.....	2	.5222	0	.4971	3	2.3115	...	.7522
5.....	0	.1499	0	.1188	...	.9013	...	.2068
6.....	0	.0395	...	.0260	...	.3225	...	.0522
	$\hat{\mu}=1.0000$	$\chi^2=6.0649$*	$\hat{\mu}=.8095$	$\chi^2=2.6851$*	$\hat{\mu}=1.4468$	$\chi^2=1.0971$*	$\hat{\mu}=.9512$	$\chi^2=1.9441$*

B. OMB: $p(c_i|\Gamma)$ IS POISSON[b]

	1967		1968		1969	
CUTS (c_i)	Actual	Predicted	Actual	Predicted	Actual	Predicted
0.....	5	5.8877	9	16.2281	16	16.8070
1.....	6	7.7610	27	15.0407	16	13.7110
2.....	10	5.1152	4	6.9701	3	5.5926
3.....	1	2.2476	1	2.1534	3	1.5208
4.....	...	.7407	...	.4989	...	.3102
5.....	...	.1953	...	.0925	...	.0506
	$\hat{\mu}=1.1318$	$\chi^2=5.5658$*	$\hat{\mu}=.9268$	$\chi^2=14.6120$	$\hat{\mu}=.8158$	$\chi^2=3.0615$*

[a] With fixed $\nu = 5.9$, and $\hat{\phi} = \nu/(\nu + \mu)$.

[b] With fixed $\nu = \infty$, and $\mu = (\Gamma/n\delta)$.

* Hypothesis cannot be rejected at the .05 level.

in its cutting approach than the more selective HUD can be explained with the aid of θ_i parameter estimates. Maximum-likelihood estimation of $\{\theta_i\}$, based on proposition III and on the set of 25 "standard" programs (n. 26 above), is simple: MLE$(\hat{\theta}_i)$ equals the number of cuts received by program i, over the entire four-year (HUD) or three-year (OMB) period, divided by the total number of HUD or OMB cuts observed over these periods. The results for both HUD and OMB, rank ordered from highest to lowest priority, are presented in the first columns of tables 7 and 8, respectively.

One null explanation for these organizational priorities can be dismissed immediately: budgetary attention focus is not guided by controllability. For HUD, the correlation between $(1/\hat{\beta}_i)$ and $\hat{\theta}_{Hi}$ is $r^2 = .0540$. For OMB, the correlation between $(1/\hat{\beta}_i)$ and $\hat{\theta}_{Oi}$ is $r^2 = .1117$.

A more fruitful clue about the structure underlying these organizational priorities can be found through a qualitative examination of budgetary guidance memoranda and planning documents. The HUD documents cluster programs in terms of functional areas, which are highly correlated with breakdowns by organizational division within HUD. These program clusters are: (1) housing programs, located primarily in the Renewal and Housing Assistance division; (2) nonhousing programs in the Metropolitan Development division; (3) research, training, and administrative programs; and (4) market support programs in the Mortgage Credit division. On the other hand, OMB documents organize programs on a more technocratic basis, but with one important exception for presidential initiatives: (1) Administration Commitments, (2) Uncontrollable or Mandatory Expenditure programs, (3) Financial Asset programs, and (4) a rather broad and amorphous "Other" category. The Administration Commitments category is OMB's bureaucratic device for protecting the President's "pet programs" (e.g., Model Cities under LBJ) from more routine budgetary scrutiny.

Once HUD and OMB θ_i parameter estimates are organized into these qualitative categories, the lexicographic order apparent in tables 7 and 8 emerges. With the exception of Rehabilitation Loans and Elderly Housing,[31]

[31] These two exceptions to the dominant HUD pattern are due to the following: (1) Robert Weaver, Secretary of HUD under Kennedy and Johnson, viewed the Elderly Housing program partially as a gimmick which local communities used to circumvent the provision of low-income housing for blacks. Local governments generally prefer elderly housing to public housing, and it was Weaver's opinion that municipalities used elderly housing units to inflate their "low-income" housing statistics. (2) Deemphasis of the Rehabilitation Loan program by HUD under LBJ stemmed from a number of causes. Most obvious was the fact that this was a congressionally imposed, rather than LBJ initiated, program which aided moderate-income, owner-occupied units in borderline "tipping" neighborhoods, rather than in ghettos. In detailed program memoranda, however, the most recurrent justification for Rehabilitation Loan cuts was the low "need," as measured by application rates. Lower-level HUD program personnel speculated that this poor application rate performance could be attributed to two possible causes: (*a*) HUD regional offices were administratively set up to deal with municipal governments instead of individual personal loans; (*b*) members of the banking community sat on the boards of local housing authorities (an alternative administrative outlet) since flotation of bonds is crucial to Public Housing construction. Rehabilitation Loans were a direct competitor to the ongoing operations of Savings and Loan Associations.

TABLE 7

HUD PRIORITY STRUCTURE, 1967–70

$\hat{\theta}_i$	Renewal and Housing Assistance, plus Model Cities	Research, Training, and Administration	Mortgage Credit	Metropolitan Development
.00000	Participation Sales Insufficiencies			
.01449	Urban Renewal			
.01449	Public Housing			
.01449	College Housing			
.01724	Model Cities Planning			
.02586	Alaska Housing			
.02586	(Minor/Miscellaneous Legislation)[a]			
.02899	Rent Supplements			
.02899		Community Development Training		
.03448	Model Cities Supplemental Grants			
.03448	Model Cities Urban Renewal Addon			
.03623			FNMA	
.03623				Advance Acquisition
.04310				Metropolitan Development Incentives
.04310		Urban Information and Technical Assistance		
.04348		Administrative Expenses		
.05072		Research		
.05072				Public Works Planning
.05072				Urban Transportation
.06034				Open Space
.06522				Neighborhood Facilities
.07246				Urban Planning
.07971	Rehabilitation Loans			
.08696	Elderly Housing			
.08696				Water and Sewer Grants

NOTE.—The sum of $\hat{\theta}_i$'s does not equal one exactly, since not all programs existed all four years.

[a] Minor/Miscellaneous Legislation is listed in parentheses because it is actually an amalgam from all divisions.

TABLE 8

OMB PRIORITY STRUCTURE, 1967–69

$\hat{\theta}_i$	Administration Commitments	Uncontrollable Programs	Financial	Other
.00000.....	Model Cities Urban Renewal Addon			
.01176.....				Urban Renewal
.01724.....	Model Cities Supplemental Grants			
.01724.....		Participation Sales Insufficiencies		
.01724.....		Alaska Housing		
.01724.....				Urban Information and Technical Assistance
.03449.....	Model Cities Planning			
.03529.....		Public Housing		
.03529		Rent Supplements		
.03529.....				Research
.04706.....			FNMA	
.04706.....			College Housing	
.04706.....				Rehabilitation Loans
.04706.....				Public Works Planning
.04706.....				Community Development Training
.05172.....				Open Space
.05172.....				Metropolitan Development Incentives
.05172.....				Minor/Miscellaneous Legislation
.05882.....				Urban Transportation
.05882.....				Administrative Expenses
.07059.....				Neighborhood Facilities
.07407.....				Advance Acquisition
.08235.....				Urban Planning
.08235.....				Elderly Housing
.09412.....				Water and Sewer Grants

NOTE.—The sum of $\hat{\theta}_i$'s does not equal one exactly, since not all programs existed all three years.

HUD, at least during the Johnson administration, clearly preferred Renewal and Housing Assistance programs to Metropolitan Development programs. In the terminology of Halperin (1974), low-income housing programs are HUD's "essence," and nonhousing and suburban programs are more peripheral to HUD's primary concerns. Research, training, and mortgage support programs are intermediate in priority. This lexicographic preference structure is arguably rooted in HUD's primary bases of constituency support—minority groups, and mayors and other politicians from large urban areas.

In the case of OMB, the buffered position of the Administration Commitments category clearly reflects OMB's constituency, which is the President himself. Programs in the Uncontrollable category can be and are cut by the simple expedient of "reestimating projections," but the data reveal that OMB resorts to this gimmick fairly rarely. The Financial Asset category includes highly controllable programs, which either are not cut at all by OMB or else are cut very severely, frequently at the tail end of the year. With the exception of Urban Renewal and two research programs,[32] the large bulk of OMB cutting effort centers on the undifferentiated "Other" category. Thus, "Other" cuts are OMB's bread and butter; and Financial Asset cuts (i.e., selling off mortgage portfolios at a discount) are nonsubstantive "shock absorbers," which OMB very self-consciously holds in reserve to cover unanticipated contingencies. The OMB's aggregate emphasis on equity (high ν) is attributable, in large part, to the scope of its undifferentiated "Other" schema.

The images which emerge from these statistical analyses are consistent with the ideologies or "missions" of the two organizations. On the one hand, HUD considers itself an activist organization, with a fairly clear vision of the desegregated and orderly urban society it would like to see (though not so clear a vision of how to achieve this). Consequently HUD organizational leaders tend to think in substantive policy schema rather than in technocratic terms. On the other hand, OMB envisions itself as an unbiased presidential assistant, charged specifically with the task of improving efficiency and economic rationality. Unless explicit presidential initiatives are at stake, OMB submits programs more equally to its scrutiny and justifies its cuts more typically on administrative, technical, and efficiency grounds.

Short-run organizational lexicographic preference structures, I argue, are rooted in longer-term institutional roles and constituency relations.

[32] Again, Urban Renewal is an outlier because of its intimate association with the Model Cities Urban Renewal Addon effort. Also (as mentioned in n. 30), preview increases for the mainline Urban Renewal program were not as large as expected statistically. Research programs ranked surprisingly high in OMB's priorities, but this may be due to the fact that OMB hoped such applied research would lead to new methods of administration and of cutting other programs' costs.

Parameter Predictions

To operationalize the parameter predictions of theorem 7, the structural components ν, α, δ, and $\bar{\delta}$ already presented in tables 4 and 6 will be used. Thus, the only exogenous piece of historical information required is Γ, the aggregate fiscal cutting target. With only this one historical input, ultimately rooted in macroeconomic and/or defense issues, the ecological control model can predict how this aggregate change will be distributed among domestic programs. Historically observed Γ's and resulting parameter predictions are reported in table 9 for HUD and in table 10 for OMB. Only two systematic patterns of deviation are observed:

1. Observed α's for allocation choices are slightly below the disaggregate prediction of $\hat{\alpha} = 1.14271$. The actual average α, across all stages, is $\hat{\alpha} = 1.03705$. I suspect that the structural parameter is in fact $\alpha = 1$. This result implies that the model can be simplified in a parsimonious direction: (*a*) the very general proposition II (i.e., $p(\beta_i) = \text{Gamma}[\alpha, \delta]$) can be replaced by $p(\beta_i) = \text{Exponential}[\delta]$; (*b*) the general theorem 2 conclusion that $p(K \mid \Gamma) = \text{Stable}[\alpha]$ can be replaced by $p(K \mid \Gamma) = \text{Cauchy}$.

2. In the OMB table 10, both $(1 - S)$, the percentage of nonzero cases, and D, a parameter governing the expected severity of nonzero cuts, are underpredicted. These two errors are related, since they imply that OMB tends to cut more programs more gently than the ecological control model predicts. The source of the error is traceable to $\hat{\nu} = \infty$: OMB, due to its "unbiased" and technocratic institutional role, is more equitable in its cutting strategy than would be the case under the more heterogeneous policy guidance presumed by the model.

In table 11, the expectations underlying "numbers of cuts" distributions in theorem 2 and theorem 3b are predicted. No systematic errors are observed.

The foregoing empirical analysis can be summarized in the following fashion. First, it was demonstrated that all three levels of decision making in the model could be aggregated to yield correct predictions about outcomes (i.e., the distribution of program allocation choices). Then the two main structural components of the model (alternative generation and attention focus) were confirmed as being accurate in essentials. Institutional differences between OMB and HUD were highlighted. The final prediction of parameters showed that the model, in addition to accounting for structural regularities, is robust across historical variation.

HISTORICAL AND POLICY INTERPRETATION

These statistical analyses, designed to test the structural validity of the ecological control model, have a clear historical context. From the point of view of HUD, two large-scale but contradictory political developments

TABLE 9

HUD PARETO PARAMETER PREDICTIONS

	Fiscal Target	Actually Estimated			Theoretically Predicted*		
	Γ_H	$(1-\hat{S})$	$\hat{\alpha}$	$\hat{D}$†	$(1-\phi')$	α	$\Lambda\phi/\nu\delta$
1967 (25):							
Regular submission.....	40,581	.44000	1.22447	.0006393	.28785	1.14271	.0004809
Bands.....	112,781	.76000	1.68068	.0003415	.59303	1.14271	.0003565
1968 (42):							
Regular submission.....	56,300	.19048	.77780	.0004729	.24207	1.14271	.0004979
Bands.....	200,750	.57143	.91034	.0003099	.60698	1.14271	.0003502
1969 (47):							
Regular submission.....	195,290	.61702	.62220	.0007694	.55866	1.14271	.0003717
Bands.....	280,230	.74468	.79743	.0004556	.68054	1.14271	.0003155
1970 (41):							
Regular submission.....	168,220	.53659	.99295	.0003850	.55547	1.14271	.0003731
Bands.....	240,920	.63415	.82640	.0003800	.67676	1.14271	.0003174

NOTE.—N's in parentheses.

* Predictions employ the following fixed parameter estimates derived from earlier analyses: (a) from micro-level alternative generation (table 4)—$\alpha = 1.14271$, $\bar{\delta} = 4838.977$, and $\delta = 1713.156$; and (b) from intermediate-level attention focus (table 6)—$\nu = 5.9$.

† Reported maximum-likelihood estimates of D are based on fixed $\alpha = 1.14271$. Since under simultaneous estimation, MLE $\hat{D}$ is not independent of MLE $\hat{\alpha}$, this procedure permits error in predicted D's to be assessed independently of error in predicted α's.

TABLE 10

OMB PARETO PARAMETER PREDICTIONS

	Fiscal Target	Actually Estimated			Theoretically Predicted*		
	Γ_O	$(1-\hat{S})$	$\hat{\alpha}$	$\hat{D}$†	$(1-\phi^{\nu})$	α	$\Lambda\phi/\nu\delta$
1967 (22):							
Director's Review	47,778	.59091	1.25470	.0006509	.37510	1.14271	.0004657
Ratchets	136,823	.77273	.67423	.0004933	.73983	1.14271	.0003207
1968 (41):							
Director's Review	83,915	.70732	1.38383	.0008660	.35179	1.14271	.0004736
Ratchets	168,215	.78049	.92137	.0006792	.58065	1.14271	.0003900
1969 (38):							
Director's Review	83,080	.52632	1.49022	.0005409	.37125	1.14271	.0004670
Ratchets	150,780	.57895	1.11963	.0003680	.56921	1.14271	.0003945
1970 (38):							
Director's Review	146,660	.73684	.87945	.0007489	.55919	1.14271	.0003985

NOTE.—N's in parentheses.

* Predictions employ the following fixed parameter estimates derived from earlier analyses: (*a*) from micro-level alternative generation (table 4)—$\alpha = 1.14271$, $\bar{\delta} = 4838.977$, and $\delta = 1713.156$; and (*b*) from intermediate-level attention focus (table 6)—$\nu = 100{,}000 \cong \infty$.

† Reported maximum-likelihood estimates of D are based on fixed $\alpha = 1.14271$. Since under simultaneous estimation, MLE $\hat{D}$ is not independent of MLE $\hat{\alpha}$, this procedure permits error in predicted D's to be assessed independently of error in predicted α's.

TABLE 11

THEOREMS 2 AND 3b EXPECTATION PREDICTIONS

A. HUD

	ACTUAL		PREDICTED	
	K	$E(\epsilon_t)$	$\Gamma/\bar{\delta}$	$\nu(1-\phi)/\phi$
1967 (25).....	25	1.0000	23.31	.9771
1968 (42).....	34	.8095	37.35	.9111
1969 (47).....	68	1.4468	57.91	1.2589
1970 (41).....	39	.9512	49.79	1.2447

B. OMB

	ACTUAL		PREDICTED	
	K	$E(\epsilon_t)$	$\Gamma/\bar{\delta}$	$\Gamma/n\bar{\delta}$
1967 (22).....	29	1.1318	28.28	1.2852
1968 (41).....	38	.9268	34.76	.8479
1969 (38).....	31	.8158	31.16	.8200

NOTE.—N's in parentheses.

characterized the 1965 through 1968 period. On the one hand, this was the era of Johnson's Great Society and of black urban riots. The direct effect was the creation of new HUD programs, such as Model Cities and Rent Supplements. The indirect effect was a high executive-branch priority on inner city housing and urban programs in general, and hence upward budgetary pressure on HUD. A contradictory historical development was the massive escalation of the Vietnam War. Defense expenditures rose precipitously, but in a volatile and uncertain manner. The net effect was downward budgetary pressure on the domestic side, as efforts were made to offset partially the rising costs of Vietnam.

The central historical question to be addressed with the aid of the ecological control model, therefore, is, What were the unintended domestic budgetary and policy consequences of the Vietnam War?

Domestic policy consequences are unintended because resource allocation in the executive branch emerges from the interaction of decision making at three levels of budgetary aggregation rather than from the domination of any one. The Weberian image of a unified central command system, in which policy is set on high and is then implemented below, is highly misleading. In the relatively short annual time frame of budgetary decision making, the state appears to be a relatively autonomous bureaucratic system with its own internal dynamic—the analysis of which is crucial for understanding the discretionary translation of social inputs into resource outputs.

In a longer historical time frame, however, the determination and interaction of parameter premises are of central concern. In the model above,

ecological control of the premises of budgetary decision making has three forms. At the micro program level of aggregation, the perception of feasible budgetary alternatives (γ_{ik}) is affected by a program's relative controllability (β_i), which is rooted in the legal structure. At the intermediate departmental level of aggregation, selection from among budgetary cutting alternatives is guided by central organizational authorities' policy priorities (θ_i), which are rooted in institutional roles and constituency relations. At the macro presidential level of aggregation, both the amount and the distribution of cutting effort demanded from the system are controlled by fiscal targets (Γ_H and Γ_O), which are grounded in macroeconomic and defense considerations.

To analyze the historical question of this section, therefore, one must "unpack" the separate implications of each ecological level and then reassemble these parts into the interactive whole. The technical tool for this task is theorem 6, which cumulates the results of all budget stages into predictions about the expectation and variance of program i's budgetary growth or decline (γ_{Fi}), as reflected in the final President's Budget, relative to the previous year. This theorem is operationalized with the $\{\beta_i\}$, $\{\theta_{Hi}\}$, $\{\theta_{Oi}\}$, Γ_H, and Γ_O parameters reported in tables 5, 7, 8, 9, and 10.

Controllability and Fiscal Targets

Consider now the interaction of controllability and fiscal targets alone. All effects from the intermediate policy level of aggregation are temporarily suppressed by $\theta_{Hi} = \theta_{Oi} = 1/n$. The expectation half of theorem 6 collapses into the simple form: $E(\gamma_{Fi}) = 2(1/\beta_i) - (\Gamma/n\bar{\delta})(1/\beta_i)$, where $\Gamma \equiv \Gamma_H + \Gamma_O$. This expression is graphed in figure 5 using the empirical $\{\beta_i\}$ parameters in table 5. The image is that of a purely mechanical budgetary system, guided by no policy priorities whatsoever. This image is useful as a baseline from which to evaluate the impact of organizational preferences.

On a general level, it is clear that controllable programs (high $1/\beta_i$) shrink more drastically than uncontrollable programs (low $1/\beta_i$) as a function of stringent fiscal constraint (high Γ). Controllability (and thus the legal structure) controls the differential sensitivity of programs to external macroeconomic or defense events. It is as one's intuition might expect: uncontrollable programs are highly buffered from influence by upper executive echelons, and conversely.

The variance result in theorem 6—in particular, the $(1/\beta_i)^2$ term—implies in addition that the volatility of growth around expected values may be quite extreme for highly controllable programs. Uncontrollable programs, on the other hand, exhibit a more glacial and apparently deterministic pattern. The incrementalist models of Davis et al. (1966, 1974), therefore, fit better the more uncontrollable the program.[33]

[33] This conclusion is supported by the empirical findings of Gist (1974).

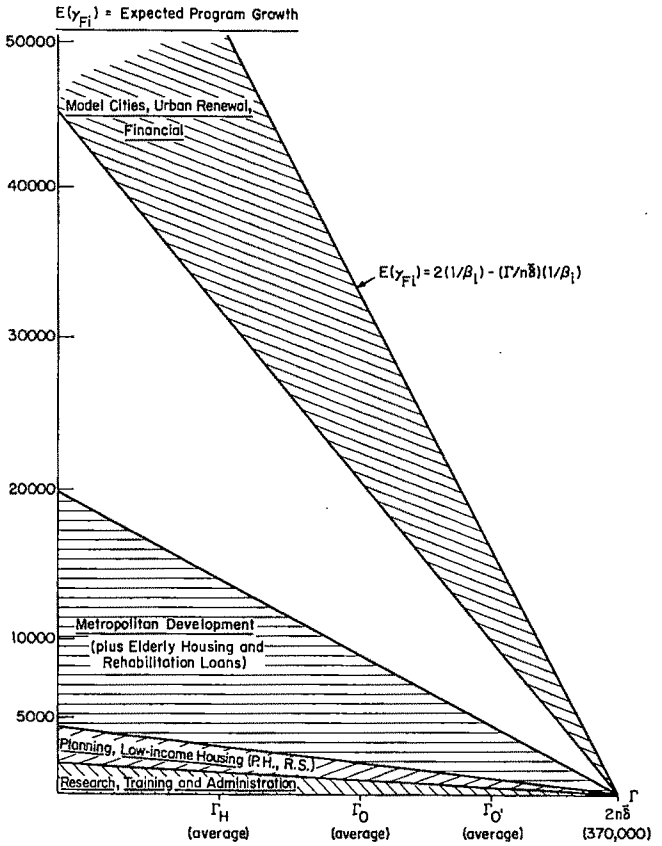


FIG. 5.—Interaction of fiscal policy and controllability alone

Historically, it is clear that as the Vietnam War drives aggregate cutting targets (Γ) up, it is the Model Cities, the Urban Renewal, and the financial programs which are structurally the most vulnerable to large dollar cuts, in the absence of explicit policy intervention to the contrary. Metropolitan Development programs are the second most vulnerable; and low-income housing, planning, and various administrative support programs are least vulnerable, from the controllability point of view. The structural contradiction between Johnson's Vietnam and antipoverty efforts is immediately apparent.[34]

[34] This analysis suggests that, in the absence of explicit policy intervention to the contrary, the routine operation of the budgetary system in response to a shock such as the Vietnam War would tend to cripple the Great Society Model Cities program, which was just beginning to get off the ground. Under Johnson, this explicit policy intervention was very much forthcoming. The model, however, does shed some light on the bureaucratic aspects of Model Cities' demise under Nixon. While Nixon sought to dismantle the Great Society for reasons having little to do with budgetary decision making, the model suggests that by this explicit negative policy Nixon may have only accelerated a tendency which was

Visual calibration of the relative magnitudes of various fiscal effects is presented on the Γ-axis in figure 5. The Γ_H presented is the average aggregate HUD regular submission cut observed over this four-year period; Γ_O adds the average aggregate OMB Director's Review cut observed; and $\Gamma_{O'}$ adds the average aggregate OMB ratchet cut observed. The magnitude ($\Gamma_{O'} - \Gamma_O$) is directly traceable to the war in Vietnam: as annual defense expenditures become more clear at the end of each year, Johnson demanded final, last-second rounds of domestic cutbacks (Padgett 1979*a*, 1979*b*). At least a portion of the more traditional Γ_H and ($\Gamma_O - \Gamma_H$) cutbacks, moreover, indirectly reflected the impact of the Vietnam War, as both HUD and OMB tried to anticipate likely but uncertain war demands.[35]

Organizational Priorities and Fiscal Targets

Consider now the interaction of organizational policy priorities and fiscal targets alone, holding constant the impact of controllability. Figure 6 illustrates this interaction, using the empirical parameters from tables 7 and 8, within the "priority space" of $\theta_{Hi} \times \theta_{Oi}$. Interpretation is as follows. (*a*) Programs closer to the origin are of higher priority; programs farther from the origin are of lower priority. (*b*) Programs closer to the 45° line are programs where OMB and HUD agree on relative priority; programs farthest from the 45° line are programs of greatest priority disagreement.

With controllability suppressed, it is impossible to speak of absolute dollar levels of budget growth or decline, but it is possible to demarcate "winners" (programs for which budget growth is expected) from "losers" (programs whose expected budgets decline) as a function of aggregate fiscal targets (Γ_H and Γ_O). Technically, this is achieved by setting $E(\gamma_{Fi}) = 0$ in theorem 6, which produces the "break-even" or demarcation line (i.e., $\theta_{Hi} = 2(\bar{\delta}/\Gamma_H) - (\Gamma_O/\Gamma_H)\theta_{Oi}$) illustrated in figure 6. As overall fiscal "tightness" ($\Gamma \equiv \Gamma_H + \Gamma_O$) is increased, holding constant distribution of cutting effort (i.e., $\Gamma_H = \Gamma_O$), the break-even line is shifted in parallel out from the origin. As distribution of aggregate cutting effort changes, holding constant fiscal tightness, the break-even line rotates in accordance with the slope (Γ_O/Γ_H).

On a general level, the substantive implications are straightforward. Not surprisingly, a tighter fiscal year makes for fewer "winners" among programs than a looser fiscal year. When we remember the quantitative sizes of expected gains and losses, however, an additional implication becomes

already embedded in the routine operation of the budgetary system. Given the generally conservative fiscal and domestic stance of the Nixon administration, coupled with the continued pressure of the war in Vietnam, a policy of omission as well as commission toward Model Cities would probably have been sufficient over a slightly longer time to cripple the program.

[35] Through no fault of its own, however, OMB chronically underestimated Vietnam War expenditure projections through this period, necessitating numerous supplementals.

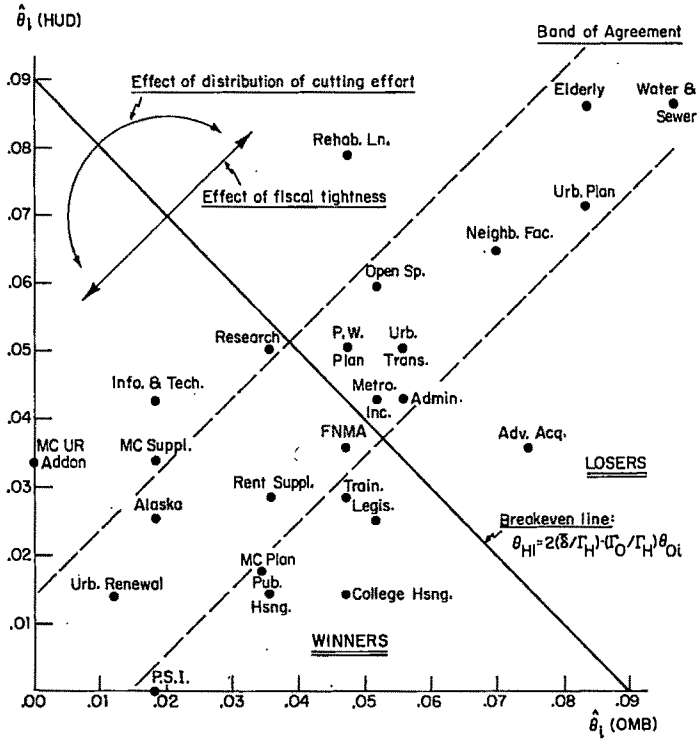


FIG. 6.—Interaction of fiscal policy and organizational priorities alone

apparent: "Winner" programs of high priority to both OMB and HUD are not greatly affected by whether the fiscal year is, in aggregate, tight or loose. Implicit "protective barriers" are thrown up around such programs by central organizational authorities. "Loser" programs of low priority to both OMB and HUD, on the other hand, are affected drastically by restrictive fiscal policy shifts. In addition to reasons of legal controllability, therefore, the interaction of presidential fiscal policy and organizational priorities causes most of the aggregate "shock" to be absorbed by such programs.

The ecological distribution of cutting effort between OMB and HUD, reflected graphically in rotations of the demarcation line, affects most radically high-conflict programs involving greatest priority disagreement. Whichever organization holds the cutting initiative "controls" the allocation outcome for such programs.

Empirically during the Johnson administration, OMB and HUD both placed a low priority on the suburban Metropolitan Development programs. Model Cities, Urban Renewal, and low-income housing enjoyed high priority. Various financial, research, and administrative support programs were intermediate in joint preference. In terms of policy conflict, OMB

placed a higher priority on Model Cities, Rehabilitation Loans, and research programs than did HUD; HUD preferred College and Public Housing, administration and training, and new legislation more than did OMB.

Therefore, the fiscal "shock" of the Vietnam War was absorbed in HUD largely by the Metropolitan Development sector. These programs were structurally vulnerable because of controllability and were also of low organizational priority. The highly controllable and intermediate preference financial programs (FNMA and College Housing) experienced wild fluctuations in budgetary allocation. Low-income housing, research, and administrative support programs, being of intermediate priority and of fairly low controllability, were on average not greatly affected by the Vietnam War. However, since OMB tended to have an edge in cutting initiative during this period, research and Model Cities programs fared better, while training, College Housing, and proposed legislation fared worse than they otherwise would have.

Of particular interest are the Model Cities and Urban Renewal programs, because they experienced contradictory pressures during this era. On the one hand, they were very vulnerable structurally. On the other hand, they were of very high priority. The net effect was that, while expansion was definitely inhibited, the executive branch under Johnson succeeded in sheltering these programs from some of the potentially more catastrophic consequences of the Vietnam War, insofar as doing so was within the executive's power.

Historically, of course, the antipoverty effort did experience serious financial difficulties, even during the Johnson era. But, on the direct choice level, the largest Model Cities cuts came from the Congress, not from the executive. On the more indirect ecological level, however, the distribution of cutting effort between the Congress and the executive as a whole shifted markedly in the direction of the Congress, once Johnson submitted his 10% tax surcharge proposal to fund the war in 1967 (Padgett 1979*a*, 1979*b*). In other words, while Johnson, OMB, and HUD in fact struggled strenuously to save the highly vulnerable Model Cities program from the impact of the Vietnam War, the executive's traditionally strong political control over the federal budget deteriorated rapidly once new taxes became an issue—with obvious negative consequences for the antipoverty effort.

Three-way Interaction: Controllability, Organizational Priorities, and Fiscal Targets

The interaction of all three ecological premises—controllability, organizational priorities, and fiscal targets—can now be reconstructed with the aid of figure 7, which illustrates theorem 6 in graphical form. Interpretation is as follows. The vertical axis measures expected allocation change, from one year to the next, for program i in the final President's Budget. This change

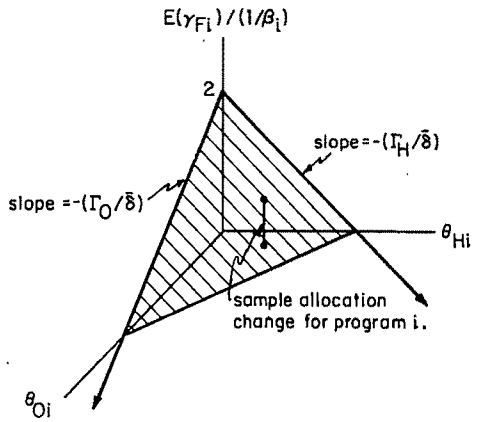


FIG. 7.—Interaction of fiscal policy, controllability, and organizational priorities

is standardized in terms of the program's controllability—$(1/\beta_i)$, which is the average size of program cut. The two horizontal axes define the same $\theta_{Hi} \times \theta_{Oi}$ "priority space" as was presented in figure 6. The tilted "fiscal plane" is controlled, as illustrated, by the aggregate fiscal targets, Γ_H and Γ_O, which are standardized here in terms of average cross-sectional cut $(\bar{\delta})$. The intersection of this "fiscal plane" with the horizontal "priority space" is the break-even line presented in figure 6.

This figure can be used, even without knowledge of underlying equations, in order to generate the expected value or mean allocation change for any HUD program i. First locate program i in the horizontal priority space of $\theta_{Hi} \times \theta_{Oi}$ (see fig. 6). Then the vertical distance between this point and the corresponding point in the fiscal plane is the predicted standardized mean allocation change for program i. Predicted dollar allocation change is generated simply by multiplying this vertical distance by $(1/\beta_i)$. The set of all n such vertical projections defines the expected budget changes for all programs within HUD.

The fiscal plane compactly summarizes final allocation changes, from one year to the next, across all HUD programs simultaneously. The impact of various macro fiscal shifts on domestic housing policy is thereby highlighted. The effect of "tightness" of fiscal year is simply to expand or to contract the fiscal plane outward or inward from the origin (holding constant the fixed point $\{E(\gamma_{Fi})/(1/\beta_i), \theta_{Hi}, \theta_{Oi}\} = \{2, 0, 0\}$). The effect of distribution of cutting effort, on the other hand, is to rotate or swing the fiscal plane around from left to right. Figure 7 presents the case of equal distribution of cutting effort.

Substantively, these macro fiscal shifts (be they due to the war in Vietnam or to some other source) ecologically control the distribution of which institutional actor's preferences are more saliently represented in the final

President's Budget. Fiscal tightness controls the *vertical* distribution of bureaucratic "power," so defined. In looser fiscal years, the fiscal plane's slopes are more gentle. That is, programs receive, in standardized terms, more egalitarian budget growth, largely independent of central policy priorities. Unstandardized program growth is driven primarily by program-manager requests (which are affected by controllability alone), and central authorities simply do not kick into action much under such circumstances.

On the *horizontal* dimension of OMB versus HUD, it is the distribution of aggregate cutting effort which is crucial. Take the extreme cases first: If HUD central authorities do not cut their program-manager requests at all, thus shifting the entire burden to OMB, then OMB's priorities will be reflected and HUD's central priorities will not. On the other hand, if HUD central authorities take the initiative and cut all that is required (either by choice or as a result of OMB preview ceiling pressure), then OMB's program priorities will be irrelevant and HUD will control the relative allocation result. The more typical in-between cases have already been discussed. The ecological distribution of cutting effort between OMB and HUD affects high-conflict programs of greatest priority disagreement the most.

Thus macro fiscal targets, derived on grounds having little to do with detailed programmatic priorities, tip the lower-level bureaucratic politics "balance of power" between organizations and among institutional levels. Once again, this is the theme of ecological control in action: outcomes from one level of aggregation shape without predetermining decision processes operating at other levels of aggregation. In an autonomous bureaucratic system, like the state, social control of expenditures operates not directly or self-consciously but indirectly through structural parameters or underlying premises of organizational decision making.

CONCLUSION

Schumpeter's challenge for a fiscal sociology, in which the central issue is the articulation between state and society, has not been accepted. Institutional political scientists, public finance economists, pluralists, and Marxists remain split into different camps with very little cross-fertilization. I propose one route of integration through the concepts of hierarchy and of ecological control. From an internal perspective, federal executive budgets emerge from the interaction of three levels of organizational and expenditure aggregation. The structure of decision making within this bureaucratic complex must be taken seriously for any understanding of the translation of political, economic, and social inputs into state expenditure outputs. From an external perspective, however, each level of organizational aggregation is embedded in a distinctive cultural context of "ecological control" premises which reflect, in highly compressed form, the historical residues of past political struggles—program controllability, whose roots lie in the

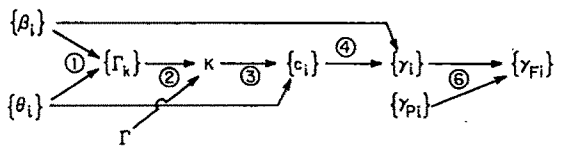
legal structure; organizational priorities, whose roots lie in institutional roles and constituency relations; and presidential fiscal targets, whose roots lie in macroeconomic and defense issues. Such ecological control premises change on different historical time frames. This hierarchy and ecological control approach is one possible bridge between organization theorists and macro sociologists.

A stochastic process model of budgetary decision making within the federal executive branch has been developed to make the arguments more precise. Seven probabilistic theorems were derived and tested. Empirical analyses, using HUD and OMB allocation data from the Johnson administration, strongly supported the hierarchy and ecological control model. On a more applied level, the model provides a systematic framework for exploration into historical issues, such as the impact of the Vietnam War on domestic housing and antipoverty policy.

MATHEMATICAL APPENDIX

Theorem Proofs

Overall, theorems 1–6 have the schematic structure (with theorem numbers encircled) shown in the unnumbered figure below.



Theorem 1: $p(\Gamma_k) = (\alpha/\delta)[1 + \Gamma_k/\delta]^{-(\alpha+1)}$, where Γ_k is the column sum: $\Gamma_k \equiv \Sigma_{i=1}^{n}\gamma_{ik}$.

Proof: From propositions I and III and the definition of Γ_k, it follows that $p(\Gamma_k|\theta_i, \beta_i)$ is the following mixture: $p(\Gamma_k|\theta_i, \beta_i) = \Sigma_{i=1}^{n}\theta_i\beta_i e^{-\beta_i\Gamma_k}$. To find unconditional $p(\Gamma_k)$, we utilize the law of total probability:

$$p(\Gamma_k) = \int_0^\infty \int_0^1 p(\Gamma_k|\theta_i, \beta_i)p(\theta_i)p(\beta_i)d\theta_i d\beta_i ,$$

where $p(\theta_i)$ and $p(\beta_i)$ are given by heterogeneity propositions II and IV, respectively.

Thus, integrating out θ_i first:

$$p(\Gamma_k|\beta_i) = \int_0^1 \sum_{i=1}^{n} \theta_i\beta_i e^{-\beta_i\Gamma_k}\left\{\frac{\Gamma(n\nu)}{\Gamma(\nu)\Gamma((n-1)\nu)}(\theta_i)^{\nu-1}(1-\theta_i)^{(n-1)\nu-1}\right\}d\theta_i$$

$$= \sum_{i=1}^{n} \beta_i e^{-\beta_i\Gamma_k}\left[\frac{\nu}{(\nu) + (n-1)\nu}\right]$$

$$= \frac{1}{n}\sum_{i=1}^{n}\beta_i e^{-\beta_i\Gamma_k} .$$

And, integrating out β_i next:

$$p(\Gamma_k) = \int_0^\infty \frac{1}{n} \sum_{i=1}^{n} \beta_i e^{-\beta_i \Gamma_k} \left\{ \frac{\delta^\alpha}{\Gamma(\alpha)} \beta_i^{\alpha-1} e^{-\delta\beta_i} \right\} d\beta_i$$

$$= \frac{1}{n} \frac{\delta^\alpha}{(\delta + \Gamma_k)^\alpha} \sum_{i=1}^{n} \int_0^\infty \beta_i \frac{(\delta + \Gamma_k)^\alpha}{\Gamma(\alpha)} \beta_i^{\alpha-1} e^{-(\delta+\Gamma_k)\beta_i} d\beta_i$$

$$= (\alpha/\delta)[1 + \Gamma_k/\delta]^{-(\alpha+1)} .$$

Theorem 2:

$$\text{For } \alpha > 2, \lim_{\Gamma \to \infty} p(K|\Gamma) = \text{Normal}\left[(\Gamma/\bar{\delta}), \left(\frac{\alpha}{\alpha - 2}\right)(\Gamma/\bar{\delta})\right] .$$

$$\text{For } 1 < \alpha < 2, \lim_{\Gamma \to \infty} p(K|\Gamma) = \text{Stable}[\alpha] .$$

Proof: At the aggregate level of analysis, the superposed sequence Γ_k forms a Renewal Process (Feller 1971), with Γ_k playing the role of a "waiting time." Thus, for the case of $\alpha > 2$, where finite mean and variance exist, the Renewal Central Limit theorem (Feller 1971, p. 372) implies that

$$\lim_{\Gamma \to \infty} p(K|\Gamma) = \text{Normal}\left[(\Gamma/\bar{\delta}), \left(\frac{\alpha}{\alpha - 2}\right)(\Gamma/\bar{\delta})\right] .$$

For the case of $1 < \alpha < 2$, finite variance of Γ_k does not exist, and the Renewal Central Limit theorem does not hold. However, Feller (1949) proves through generating functions, for conditions which are satisfied by the Pareto, that

$$\lim_{\Gamma \to \infty} p(K|\Gamma) = \text{Stable}[\alpha] .$$

For finite expectation ($\alpha > 1$), the basic Renewal theorem itself (Feller 1971, p. 360) implies that

$$\lim_{\Gamma \to \infty} E(K|\Gamma) = (\Gamma/\bar{\delta}) .$$

Theorem 3a:

$$\lim_{n \to \infty} p(c_i|\Gamma, \theta_i) = \frac{(\theta_i \Gamma/\bar{\delta})^{c_i}}{c_i!} e^{-(\theta_i \Gamma/\bar{\delta})} .$$

Proof: The characteristic property of the Poisson is that $E(c_i)$ equals $\text{Var}(c_i)$. It is the only commonly recognized discrete distribution which has this property. Furthermore, for *fixed* K, micro-level cutting of each individual program is a Bernoulli Process (Feller 1968, p. 196). Therefore (see Karlin and Taylor 1975, p. 8),

$$E(c_i) = E_K[E(c_i|K)] = E_K[\theta_i K] = \theta_i E(K)$$

$$= \theta_i(\Gamma/\bar{\delta}) .$$

Also,

$$\text{Var}(c_i) = E_K[\text{Var}(c_i|K)] + \text{Var}_K[E(c_i|K)]$$

$$= E_K[\theta_i(1 - \theta_i)K] + \mathrm{Var}_K[\theta_i K]$$
$$= \theta_i E(K) + \theta_i^2[\mathrm{Var}(K) - E(K)]$$
$$= \theta_i(\Gamma/\bar{\delta}) + \theta_i^2[\mathrm{Var}(K) - (\Gamma/\bar{\delta})]\,,$$

where if $\alpha > 2$, $\mathrm{Var}(K)$ is given above, and if $1 < \alpha < 2$, finite $\mathrm{Var}(K)$ is given in Feller (1949). For large n, $\theta_i^2 \ll \theta_i$, so that

$$\lim_{n\to\infty} \mathrm{Var}(c_i) = E(c_i)\,.$$

Theorem 3b:

$$\lim_{n\to\infty} p(c_i|\Gamma) = \frac{\Gamma(c_i + \nu)}{c_i!\Gamma(\nu)}\phi^{\nu}(1 - \phi)^{c_i}\,, \qquad \text{where } \phi \equiv \left[\frac{(n-1)}{\Gamma/\nu\bar{\delta} + (n-1)}\right].$$

Proof: Once again, we need to rely on the law of total probability and on heterogeneity proposition IV:

$$p(c_i|\Gamma) = \int_0^1 p(c_i|\Gamma, \theta_i)p(\theta_i)d\theta_i$$
$$= \int_0^1 \left\{\frac{(\theta_i\Gamma/\bar{\delta})^{c_i}}{c_i!}e^{-(\theta_i\Gamma/\bar{\delta})}\right\} \left\{\frac{\Gamma(n\nu)}{\Gamma(\nu)\Gamma((n-1)\nu)}(\theta_i)^{\nu-1}(1 - \theta_i)^{(n-1)\nu-1}\right\} d\theta_i\,.$$

As it stands, this expression is not tractable. However, for large n, we can rely on the following

Lemma:

$$\lim_{\substack{n\to\infty \\ \theta_i\to 0}} \mathrm{Beta}[\nu, (n-1)\nu] = \mathrm{Gamma}[\nu, (n-1)\nu]\,.$$

Proof: Let $n \to \infty$ and $\theta_i \to 0$, so that $\nu\theta_i(n-1) \to t_i$. Then

$$p(t_i) = \frac{\Gamma(n\nu)}{\Gamma(\nu)\Gamma((n-1)\nu)}\left[\frac{t_i}{\nu(n-1)}\right]^{\nu-1}\left[1 - \frac{t_i}{\nu(n-1)}\right]^{(n-1)\nu-1},$$

$$\lim_{n\to\infty} p(t_i) = \frac{1}{\Gamma(\nu)}[(n-1)\nu]^{\nu}[\nu(n-1)]^{-(\nu-1)}t_i^{\nu-1}e^{-t_i}$$
$$= \frac{\nu(n-1)}{\Gamma(\nu)}t_i^{\nu-1}e^{-t_i}\,.$$

Therefore,

$$\lim_{n\to\infty} p(\theta_i) = \frac{[\nu(n-1)]^{\nu}}{\Gamma(\nu)}\theta_i^{\nu-1}e^{-\nu(n-1)\theta_i}\,.$$

Given this lemma, the theorem above follows directly:

$$p(c_i|\Gamma) = \int_0^{\infty} \left\{\frac{(\theta_i\Gamma/\bar{\delta})^{c_i}}{c_i!}e^{-(\theta_i\Gamma/\bar{\delta})}\right\} \left\{\frac{[\nu(n-1)]^{\nu}}{\Gamma(\nu)}\theta_i^{\nu-1}e^{-\nu(n-1)\theta_i}\right\} d\theta_i$$

$$= \frac{(\Gamma/\bar{\delta})^{c_i}}{c_i!} \frac{[\nu(n-1)]^{\nu}}{\Gamma(\nu)} \int_0^{\infty} \theta_i^{c_i+\nu-1} e^{-[\Gamma/\bar{\delta}+\nu(n-1)]\theta_i} d\theta_i$$

$$= \frac{\Gamma(c_i+\nu)}{c_i!\Gamma(\nu)} \left[\frac{\nu(n-1)}{\Gamma/\bar{\delta}+\nu(n-1)}\right]^{\nu} \left[\frac{\Gamma/\bar{\delta}}{\Gamma/\bar{\delta}+\nu(n-1)}\right]^{c_i}$$

$$= \frac{\Gamma(c_i+\nu)}{c_i!\Gamma(\nu)} \phi^{\nu}(1-\phi)^{c_i} .$$

Theorem 4a:

$$p(\gamma_i|\Gamma, \theta_i, \beta_i) = \begin{cases} e^{-(\theta_i\Gamma/\bar{\delta})} & , \quad \gamma_i = 0 \\ \sum_{c_i=1}^{\infty} \frac{\beta_i}{\Gamma(c_i)} \gamma_i^{c_i-1} e^{-\beta_i\gamma_i} \frac{(\theta_i\Gamma/\bar{\delta})^{c_i}}{c_i!} e^{-(\theta_i\Gamma/\bar{\delta})} & , \quad \gamma_i > 0 . \end{cases}$$

Proof: Again, because of the law of total probability

$$p(\gamma_i|\Gamma, \theta_i, \beta_i) = \sum_{c_i=0}^{\infty} p(\gamma_i|\beta_i, c_i) p(c_i|\Gamma, \theta_i)$$

$$= p(\gamma_i|\beta_i, c_i = 0) p(c_i = 0|\Gamma, \theta_i) + \sum_{c_i=1}^{\infty} p(\gamma_i|\beta_i, c_i) p(c_i|\Gamma, \theta_i)$$

$$= \begin{Bmatrix} 1, & \gamma_i = 0 \\ 0, & \gamma_i > 0 \end{Bmatrix} e^{-(\theta_i\Gamma/\bar{\delta})} + \sum_{c_i=1}^{\infty} \begin{Bmatrix} 0 & , \quad \gamma_i = 0 \\ \frac{\beta_i^{c_i}}{\Gamma(c_i)} \gamma_i^{c_i-1} e^{-\beta_i\gamma_i} & , \quad \gamma_i > 0 \end{Bmatrix} \cdot \frac{(\theta_i\Gamma/\bar{\delta})^{c_i}}{c_i!} e^{-(\theta_i\Gamma/\bar{\delta})}$$

$$= \begin{cases} e^{-(\theta_i\Gamma/\bar{\delta})} & , \quad \gamma_i = 0 \\ \sum_{c_i=1}^{\infty} \frac{\beta_i}{\Gamma(c_i)} \gamma_i^{c_i-1} e^{-\theta_i\beta_i} \frac{(\theta_i\Gamma/\bar{\delta})^{c_i}}{c_i!} e^{-(\theta_i\Gamma/\bar{\delta})} & , \quad \gamma_i > 0 . \end{cases}$$

This follows both from proposition I and from the fact that c_i-fold convolutions of identically distributed Exponentials(β_i) are Gamma(c_i, β_i) (see Feller 1971, p. 11).

Theorem 4b(1): For the case of $\nu = 1$,

$$p(\gamma_i|\Gamma) = \begin{cases} \phi & , \quad \gamma_i = 0 \\ (1-\phi)(\alpha/\delta)\phi[1+(\phi/\delta)\gamma_i]^{-(\alpha+1)} & , \quad \gamma_i > 0 . \end{cases}$$

Proof: Here, we begin with

$$p(\gamma_i|\Gamma, \beta_i) = \sum_{c_i=0}^{\infty} p(\gamma_i|\beta_i, c_i) p(c_i|\Gamma)$$

$$= p(\gamma_i|\beta_i, c_i = 0)p(c_i = 0|\Gamma) + \sum_{c_i=1}^{\infty} p(\Gamma_i|\beta_i, c_i)p(c_i|\Gamma)$$

$$= \begin{Bmatrix} 1, & \gamma_i = 0 \\ 0, & \gamma_i > 0 \end{Bmatrix} \phi$$

$$+ \sum_{c_i=1}^{\infty} \begin{Bmatrix} 0 & , & \gamma_i = 0 \\ \dfrac{\beta_i{}^{c_i}}{\Gamma(c_i)} \gamma_i{}^{c_i-1} e^{-\beta_i\gamma_i}, & & \gamma_i > 0 \end{Bmatrix} \phi(1 - \phi)^{c_i}.$$

This last expression simplifies, with the aid of the power-series expansion of an exponential. Therefore,

$$p(\gamma_i|\Gamma, \beta_i) = \begin{cases} \phi & , \quad \gamma_i = 0 \\ (1 - \phi)\beta_i\phi e^{-\beta_i\phi\gamma_i}, & \gamma_i > 0. \end{cases}$$

Finally, we integrate out β_i, using proposition II, to get the unconditional distribution:

$$p(\gamma_i > 0|\Gamma) = \int_0^{\infty} p(\gamma_i > 0|\Gamma, \beta_i)p(\beta_i)d\beta_i$$

$$= \int_0^{\infty} (1 - \phi)\beta_i\phi e^{-\beta_i\phi\gamma_i} \left\{ \frac{\delta^{\alpha}}{\Gamma(\alpha)} \beta_i{}^{\alpha-1} e^{-\delta\beta_i} \right\} d\beta_i$$

$$= (1 - \phi)\phi \frac{\delta^{\alpha}}{(\delta + \phi\gamma_i)^{\alpha}} \int_0^{\infty} \beta_i \frac{(\delta + \phi\gamma_i)^{\alpha}}{\Gamma(\alpha)} \beta_i{}^{\alpha-1} e^{-(\delta+\phi\gamma_i)\beta_i} d\beta_i$$

$$= (1 - \phi)\phi(\alpha/\delta)[1 + (\phi/\delta)\gamma_i]^{-(\alpha+1)}.$$

Theorem 4b(2): For the more general case of $\nu \neq 1$, the following Pareto approximation has the correct expectation and singularity:

$$p(\gamma_i|\Gamma) = \begin{cases} \phi^{\nu} & , \quad \gamma_i = 0 \\ (1 - \phi^{\nu})(\alpha/\delta)\Lambda(\phi/\nu)[1 + \Lambda(\phi/\nu\delta)\gamma_i]^{-(\alpha+1)}, & \gamma_i > 0, \end{cases}$$

where $\Lambda \equiv (1 - \phi^{\nu})/(1 - \phi)$.

Proof: For the general case of $\nu \neq 1$, the simplification mentioned in the proof of theorem 4b(1) does not work. However, the exact expectation can be derived by the method of Laplace transforms (Feller 1971, p. 429), even without knowledge of the probability distribution in closed form. Laplace transforms are defined as follows:

$$\psi(\lambda) = \int_0^{\infty} e^{-\lambda x} p(x) dx.$$

In our case, $p(x)$ is the following:

$$p(\gamma_i|\Gamma, \beta_i) = \begin{cases} \phi^{\nu} & , \quad \gamma_i = 0 \\ \displaystyle\sum_{c_i=1}^{\infty} \frac{\beta_i{}^{c_i}}{\Gamma(c_i)} \gamma_i{}^{c_i-1} e^{-\beta_i\gamma_i} \frac{\Gamma(c_i + \nu)}{c_i!\Gamma(\nu)} \phi^{\nu}(1 - \phi)^{c_i}, & \gamma_i > 0. \end{cases}$$

Therefore,

$$\psi(\lambda) = \int_0^\infty e^{-\lambda\gamma_i}\phi^\nu d\gamma_i + \int_0^\infty e^{-\lambda\gamma_i}\left\{\sum_{c_i=1}^\infty \frac{\beta_i^{c_i}}{\Gamma(c_i)}\gamma_i^{c_i-1}e^{-\beta_i\gamma_i}\right.$$

$$\left. \times \frac{\Gamma(c_i+\nu)}{c_i!\Gamma(\nu)}\phi^\nu(1-\phi)^{c_i}\right\} d\gamma_i$$

$$= \phi^\nu\left(\frac{1}{\lambda}\right) + \sum_{c_i=1}^\infty\left[\frac{\Gamma(c_i+\nu)}{c_i!\Gamma(\nu)}\right]\phi^\nu(1-\phi)^{c_i}\left(\frac{\beta_i}{\beta_i+\lambda}\right)^{c_i}$$

$$= \phi^\nu\left(\frac{1}{\lambda}\right) + \phi^\nu\left\{\left(\frac{\beta_i+\lambda}{\beta_i\phi+\lambda}\right)^\nu - 1\right\}.$$

The binomial expansion was employed in the above.

Expectations are calculated from Laplace transforms as follows:

$$E(x) = -\frac{d}{dx}\psi(\lambda = 0).$$

In our case, this yields $E(\gamma_i|\Gamma,\ \beta_i) = \nu(1-\phi)/\beta_i\phi$. The unconditional expectation, therefore, is

$$E(\gamma_i|\Gamma) = \int_0^\infty E(\gamma_i|\Gamma,\beta_i)p(\beta_i)d\beta_i$$

$$= \int_0^\infty \nu(1-\phi)\left(\frac{1}{\beta_i\phi}\right)\left\{\frac{\delta^\alpha}{\Gamma(\alpha)}\beta_i^{\alpha-1}e^{-\delta\beta_i}\right\}d\beta_i$$

$$= \nu\left(\frac{1-\phi}{\phi}\right)\bar{\delta} = \Gamma/(n-1).$$

Direct calculation of the expectation of the theorem's Pareto approximation also yields this answer. The functional form was chosen to be identical to the $\nu = 1$ case in theorem 4b(1), which was derived exactly.

Theorem 5: For the case of program-manager requested increases (γ_{Pi}),

$$p(\gamma_{Pi}|\beta_i) = (\beta_i/2)e^{-(\beta_i/2)\gamma_{Pi}},$$

and

$$p(\gamma_{Pi}) = (\alpha/2\delta)[1+\gamma_{Pi}/2\delta]^{-(\alpha+1)}.$$

Proof: Proof follows along the lines of theorem 1. Here, as stated in the text, I anticipate the simplifying empirical result that $(1/\beta_i)_{\text{increase}} \doteq 2(1/\beta_i)_{\text{cuts}}$.

Theorem 6: For the final allocational change (γ_{Fi}) of the President's Budget, relative to the previous year,

$$E(\gamma_{Fi}) = [2 - \theta_{Hi}(\Gamma_H/\bar{\delta}) - \theta_{Oi}(\Gamma_O/\bar{\delta})](1/\beta_i)$$

$$\text{Var}(\gamma_{Fi}) = 2[2 + \theta_{Hi}(\Gamma_H/\bar{\delta}) + \theta_{Oi}(\Gamma_O/\bar{\delta})](1/\beta_i)^2.$$

Proof: Here we simply cumulate the moments which are contained in the probability distributions of theorems 4a and 5.

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Analyzing Recent Trends in Prejudice toward Blacks: Insights from Latent Class Models[1]

Steven A. Tuch
Pennsylvania State University

This research note applies latent structure methods to the analysis of a set of survey items designed to measure prejudice toward blacks. Data from 1972 and 1977 national samples are employed to demonstrate the utility of latent structure techniques in analyzing trends over time in prejudiced attitudes. Various latent class models are presented and evaluated, and a four-class quasi-latent structure model is found to best describe the data in both years. This model posits the existence of two classes of "intrinsic ideologues" who exhibit extreme response patterns with certainty, in addition to a single latent dichotomous variable that accounts for the association among the indicators. The fit of the model is very good, and changes in prejudice over time are succinctly described with reference to its estimated parameters.

This research note analyzes a set of survey items designed to measure prejudice toward blacks. Previous research has documented a consistent decline in overt expressions of antiblack prejudice over the past several decades (see, e.g., Campbell 1971; Greeley and Sheatsley 1971; Taylor, Sheatsley, and Greeley 1978), but in examining this trend most studies have relied on univariate distributions of responses to prejudice measures. A different approach is adopted here. The prejudice items examined in this paper are discrete (either dichotomous or dichotomized), and latent structure methods are well suited for characterizing the underlying variable the items are presumed to measure. Several models of possible use in describing the data are suggested, their statistical adequacy is evaluated, and procedures for assigning individuals to classes of a latent variable are illustrated[2]

[1] The data sets utilized in this paper were made available by the Inter-University Consortium for Political and Social Research. The data for the spring 1972 and 1977 General Social Surveys, National Data Program for the Social Sciences, were originally collected by James A. Davis of the National Opinion Research Center, University of Chicago, and were distributed by Roper Public Opinion Research Center, Williams College. Neither the original collector of the data nor the consortium bears any responsibility for the analyses or interpretations presented here. For helpful comments on a draft of the manuscript I am indebted to Clifford C. Clogg, Marylee C. Taylor, Pamela Oliver, and three anonymous reviewers. A previous version of this paper was presented at the 1980 annual meeting of the Southern Sociological Society, March 26–29, in Knoxville, Tennessee. Requests for reprints should be sent to Steven A. Tuch, Department of Sociology, Pennsylvania State University, University Park, Pennsylvania 16802.

[2] All of the models examined in this paper were estimated with a general computer program for maximum-likelihood latent structure analysis, MLLSA (see Clogg 1977).

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THE PREJUDICE ITEMS

The data utilized in this analysis are from the 1972 and 1977 General Social Surveys administered by the National Opinion Research Center. For a full discussion of sampling methods, see Davis (1972, 1977). The actual questions used to measure prejudice toward blacks, and mnemonics for each, are as follows:

A: "Do you think there should be laws against marriages between blacks and whites?" (MARR)

B: "How strongly would you object if a member of your family wanted to bring a black friend home to dinner? Would you object strongly, mildly, or not at all?" (DINN)

Here are some opinions other people have expressed in connection with black-white relations. Which statement on the card comes closest to how you, yourself, feel?

C: "Blacks shouldn't push themselves where they're not wanted." (PUSH)

D: "White people have a right to keep blacks out of their neighborhoods if they want to, and blacks should respect that right." (SEG)

(Range of possible responses: agree strongly to disagree strongly)

E: "Do you think white students and black students should go to the same schools or to separate schools?" (SCHOOL)

F: "If your party nominated a black for President, would you vote for him if he were qualified for the job?" (PRES)

Variables *A*, *E*, and *F* are dichotomous, with responses of "yes" on variable *A*, "separate schools" on variable *E*, and "no" on variable *F* considered the prejudiced responses. Variables *C* and *D* are Likert-type items, but they are dichotomized here so that responses of "agree strongly" or "agree slightly" are taken as the prejudiced response. Variable *B* has also been dichotomized, with "strongly" or "mildly" considered the prejudiced response. Although polytomous variables can easily be incorporated into the latent structure framework (see Clogg 1979), for simplicity of presentation only dichotomous items are considered here.

Table 1 presents the distribution of responses for the six items by year. To save space, the distribution of respondents in the observed 64-fold cross-classification of the items is not presented. After eliminating black respondents and cases with missing data on any of the variables, 1,061 cases for the 1972 data and 1,199 cases for the 1977 data remain for analysis. Inspection of the item marginals in table 1 reveals the anticipated pattern: on five of the six items, the incidence of prejudiced responses decreased over the five-year period from 1972 to 1977, a finding consistent with previous research. An examination of the single item marginals yields only a coarse description of this trend, however. Note the differentials in the reduction of prejudiced responses on items *A*, *B*, *C*, *E*, and *F:* on item *A* (MARR),

TABLE 1

DISTRIBUTION OF RESPONSES ON SIX PREJUDICE ITEMS, 1972 AND 1977

ITEM	PERCENTAGE GIVING PREJUDICED RESPONSE	
	1972*	1977†
A (MARR).....	38.5	27.1
B (DINN).....	29.1	27.9
C (PUSH).....	74.1	71.5
D (SEG).....	39.6	41.5
E (SCHOOL)....	14.2	13.8
F (PRES).....	26.1	21.9

* Sample size = 1,061.
† Sample size = 1,199.

for instance, the reduction between 1972 and 1977 is 11.4%, while on item *E* (SCHOOL) the reduction is only 0.4%. Latent structure methods can provide a parsimonious interpretation of these over-time trends by taking account of all the items simultaneously, associations among them, and their status as indicators of an underlying variable. The question of dimensionality can also be examined (e.g., do the observed variables measure one type of attitude or two or more associated attitudes?).

THE LATENT STRUCTURE MODEL

Since detailed treatments of latent structure analysis are available elsewhere (see, e.g., Lazarsfeld and Henry 1968; Goodman 1974*a*, 1974*b*; Clogg 1981), few introductory comments concerning the method will be presented here. The reader is referred to these sources for a systematic account of latent structure methodology.

Latent structure notation for a six-way cross-classification of the observed variables *A*, *B*, *C*, *D*, *E*, and *F* is presented below. Let π_{ijklmn} represent the probability that an individual will be in response pattern (i,j,k,l,m,n) of the cross-classification of *A*, *B*, *C*, *D*, *E*, and *F* for $i = 1,2; j = 1,2; k = 1,2; l = 1,2; m = 1,2; n = 1,2$. It is hypothesized that the six items are indicators of a latent variable *X* that can "explain" the relationship among the manifest variables. If *X* has *T* classes, then

$$\pi_{ijklmn} = \sum_{t=1}^{T} \pi^{ABCDEFX}_{ijklmnt}, \tag{1}$$

where

$$\pi^{ABCDEFX}_{ijklmnt} = \pi^{X}_{t}\pi^{\bar{A}X}_{it}\pi^{\bar{B}X}_{jt}\pi^{\bar{C}X}_{kt}\pi^{\bar{D}X}_{lt}\pi^{\bar{E}X}_{mt}\pi^{\bar{F}X}_{nt}. \tag{2}$$

Equation (1) implies that each respondent is located in only one of the *T* latent classes of *X*, and equation (2) implies that, within the *t*th latent

class, responses on the six observed variables are mutually independent, that is, that X "explains" the association among the observed variables. The parameters π_t^X represent the distribution of respondents among classes of the latent variable, while the conditional probabilities $\pi_{it}^{\bar{A}X}, \ldots, \pi_{nt}^{\bar{F}X}$ measure the association between the observed and latent variables. The latter quantities will be utilized below to estimate certain effects in structural equation models of the kind described by Goodman (1974*b*).

Some Possible Latent Structures

A "baseline" model useful for comparative purposes is designated H_0, the model of mutual independence in which $T = 1$ in equation (1). The likelihood-ratio χ^2 statistics for the independence model are $L^2 = 1278.20$ and $L^2 = 1334.44$ on 57 df for the 1972 and 1977 data, respectively.[3] These results indicate that the hypothesis of mutual independence is not congruent with the data in either year.[4]

Consider next the two-class latent structure model H_1 in which $T = 2$ in equation (1). Table 2 provides summary information obtained by fitting this model to the 1972 data. With $L^2 = 162.64$ on 50 df, we would be led to reject this model on statistical grounds; yet it is clear that, by introducing a two-class latent variable, χ^2 has been substantially reduced.[5] As the parameters in table 2 indicate, the proportion of respondents in latent classes 1 and 2 ($\hat{\pi}_1^X$ and $\hat{\pi}_2^X$) is estimated to be .30 and .70, respectively. Given the coding scheme for the observed variables (1 = prejudiced response, 2 = nonprejudiced response), inspection of the conditional probabilities reveals that latent class 1 tends to exhibit prejudiced responses to the items, while latent class 2 (the modal class) tends to exhibit nonprejudiced responses. Since model H_1 is not congruent with the data, a detailed examination of the conditional probabilities is not carried out.

Although the fit of model H_1 represents a substantial improvement over the baseline independence model, it is nevertheless untenable on statistical grounds. Another latent structure model, the four-class model H_2, was also fitted to each data set. The four-class model would be appropriate if two

[3] The Pearson χ^2 statistic, X^2, is also frequently examined in analyses of the kind reported here. Both L^2 and X^2 are justified by (different) large sample theory and can be used to evaluate whether or not the sample is sufficiently large to warrant statistical conclusions about a particular model. If the two statistics lead to the same conclusion, partial assurance is provided that the large sample approximations are adequate. Although the results are not reported, for every model examined in this paper L^2 and X^2 lead to the same conclusion regarding model adequacy.

[4] The code book for the General Social Survey states that the samples have approximately the same efficiency as simple random samples of about two-thirds the size of the actual samples. If the χ^2 statistics were adjusted to take this into account, all of the models examined in this note would fit the data more adequately. The adjusted values of L^2 for model H_0 are 852.13 and 889.63 in 1972 and 1977, respectively.

[5] The adjusted value of L^2 for model H_1 in 1972 is 108.43.

TABLE 2

PARAMETER ESTIMATES FOR LATENT STRUCTURE MODEL H_1, 1972 DATA

	LATENT CLASS PROPORTIONS
	$\hat{\pi}^X_1 = .30$
	$\hat{\pi}^X_2 = .70$

	Conditional Probabilities	
	Latent Class 1	Latent Class 2
$\hat{\pi}^{\bar{A}X}_{1t}$	.70	.08
$\hat{\pi}^{\bar{A}X}_{2t}$	.30	.92
$\hat{\pi}^{\bar{B}X}_{1t}$	.46	.01
$\hat{\pi}^{\bar{B}X}_{2t}$	.54	.99
$\hat{\pi}^{\bar{C}X}_{1t}$	.84	.21
$\hat{\pi}^{\bar{C}X}_{2t}$	.16	.79
$\hat{\pi}^{\bar{D}X}_{1t}$	.97	.64
$\hat{\pi}^{\bar{D}X}_{2t}$	.03	.36
$\hat{\pi}^{\bar{E}X}_{1t}$	.75	.10
$\hat{\pi}^{\bar{E}X}_{2t}$	.25	.90
$\hat{\pi}^{\bar{F}X}_{1t}$	.81	.21
$\hat{\pi}^{\bar{F}X}_{2t}$	.19	.79

associated latent dichotomies were thought to underlie the six indicators; it can be viewed as an analogue to a two-factor factor analytic model (see Goodman 1974*b*). Although χ^2 for model H_2 was reduced to acceptable magnitudes in both years, three-factor interactions between the indicator variables and the two latent dichotomies were present. In such a case the model becomes complex and not particularly instructive, and so an alternative to model H_2 will be considered.[6] The alternative, a four-class restricted latent structure model, was arrived at by a mixture of statistical and substantive considerations: in statistical terms, the standardized residuals for the two extreme response patterns (1,1,1,1,1,1) and (2,2,2,2,2,2) were exceptionally large under H_1 (4.16 and 4.74 in 1972; 4.52 and 4.55 in 1977, respectively), an indication that these two cells were not fitted well under

[6] Sometimes the interactions can be eliminated by fitting certain restricted models of the kind suggested by Goodman (1974*b*). Some of these restricted models were fitted to these data, but the results were not compelling.

the model;[7] in substantive terms, the model presented below takes explicit account of "ideological" response patterns (see Duncan 1979; Duncan, Sloane, and Brody 1979).

Model H_3 is a four-class model with the conditional probabilities of the third and fourth latent classes restricted as follows:

$$\pi_{13}^{\bar{A}X} = \pi_{13}^{\bar{B}X} = \pi_{13}^{\bar{C}X} = \pi_{13}^{\bar{D}X} = \pi_{13}^{\bar{E}X} = \pi_{13}^{\bar{F}X} = 1\,, \tag{3a}$$

$$\pi_{14}^{\bar{A}X} = \pi_{14}^{\bar{B}X} = \pi_{14}^{\bar{C}X} = \pi_{14}^{\bar{D}X} = \pi_{14}^{\bar{E}X} = \pi_{14}^{\bar{F}X} = 0\,. \tag{3b}$$

Restriction (3a) implies that an individual in latent class 3 will exhibit response pattern (1,1,1,1,1,1) with certainty. Restriction (3b) implies that an individual in latent class 4 will exhibit response pattern (2,2,2,2,2,2) with certainty. If these two cells of the 64-fold cross-table are singled out for special consideration, model H_3 becomes a quasi-latent structure with respect to the remaining 62 cells in the table (see Goodman [1947*b*] for a detailed discussion of quasi-latent structure models of this kind). Model H_3 states that there exists a dichotomous latent variable X' which accounts for the association among the observed variables when cells (1,1,1,1,1,1) and (2,2,2,2,2,2) are restricted according to (3a) and (3b), respectively. The restrictions in (3a) state that a proportion of respondents (π_3^X) exhibit prejudiced responses with certainty, implying that this class is composed of ideologues who consistently exhibit the prejudiced response regardless of the item involved. Similarly, the restrictions in (3b) state that a proportion of respondents (π_4^X) exhibit nonprejudiced responses with certainty, implying that this class is composed of ideologues who consistently exhibit the nonprejudiced response.

Table 3 displays the parameter estimates for the quasi-latent structure model H_3. With $L^2 = 53.51$ and $L^2 = 49.29$ on 48 df in 1972 and 1977, respectively, model H_3 provides a very compelling fit to the data.[8] The parameter estimates reported in table 3 provide for an unambiguous interpretation of the latent classes. An examination of the conditional probabilities reveals that individuals who exhibit the prejudiced response on the indicators tend to be in latent class 1, while those who exhibit the nonprejudiced response tend to be in latent class 2. To illustrate, the conditional probability that an individual is at level 1 on variable C (PUSH), given that he is in latent class 1, is $\hat{\pi}_{11}^{\bar{C}X} = .79$ and .84 in 1972 and 1977,

[7] Observed and expected values for these cells are as follows:

	1972		1977	
	Observed	Expected	Observed	Expected
1,1,1,1,1,1....	79	49.67	63	35.91
2,2,2,2,2,2....	193	137.47	256	192.81

[8] The adjusted L^2 values are 35.67 and 32.86 in 1972 and 1977, respectively.

TABLE 3

PARAMETER ESTIMATES FOR QUASI-LATENT STRUCTURE MODEL H_3, 1972 AND 1977 DATA

Latent Class t	$\hat{\pi}_t^X$	$\hat{\pi}_{1t}^{\bar{A}X}$	$\hat{\pi}_{1t}^{\bar{B}X}$	$\hat{\pi}_{1t}^{\bar{C}X}$	$\hat{\pi}_{1t}^{\bar{D}X}$	$\hat{\pi}_{1t}^{\bar{E}X}$	$\hat{\pi}_{1t}^{\bar{F}X}$	L_2
1972:								
1.....	.24	.62	.33	.79	.95	.68	.75	53.51
2.....	.57	.10	.01	.26	.80	.12	.26	...
3.....	.06	1.00*	1.00*	1.00*	1.00*	1.00*	1.00*	...
4.....	.13	.00*	.00*	.00*	.00*	.00*	.00*	...
1977:								
1.....	.24	.54	.41	.84	.93	.66	.75	49.29
2.....	.56	.10	.02	.33	.83	.16	.11	...
3.....	.03	1.00*	1.00*	1.00*	1.00*	1.00*	1.00*	...
4.....	.17	.00*	.00*	.00*	.00*	.00*	.00*	...

* Model restrictions using eqq. (3a) and (3b).

respectively. Similarly, the conditional probability that an individual is at level 1 on variable E (SCHOOL), given that he is in latent class 1, is $\hat{\pi}_{11}^{\bar{E}X} = .68$ and .66 in 1972 and 1977, respectively. The pattern is identical for variable A (MARR), with $\hat{\pi}_{11}^{\bar{A}X} = .62$ and .54 in 1972 and 1977, respectively, and for variable F (PRES), with $\hat{\pi}_{11}^{\bar{F}X} = .75$ in both years. Thus, based on this pattern of conditional probabilities, latent class 1 may be designated a "latent prejudiced" class and latent class 2 a "latent nonprejudiced" class. Latent class 3, as discussed above, consists of individuals who exhibit the prejudiced response on all six indicators and are therefore designated as intrinsically prejudiced. Similarly, latent class 4 consists of individuals who exhibit the nonprejudiced response on all the indicators, an intrinsically nonprejudiced class. Note the ordinality of the predicted latent variable: it consists of an intrinsically prejudiced class, latent prejudiced and latent nonprejudiced classes, and an intrinsically nonprejudiced class. This ordinality will be exploited below.

The conditional probabilities associated with variable B (DINN) and variable D (SEG) require some elaboration. The parameter estimates reported in table 3 indicate that $\hat{\pi}_{11}^{\bar{B}X} = .33$ and .41 in 1972 and 1977, respectively. Consequently, since $\Sigma_{j=1}^{2} \hat{\pi}_{j1}^{\bar{B}X} = 1$, it is seen that $\hat{\pi}_{21}^{\bar{B}X} = .67$ and .59, respectively, in the two years. Thus the conditional probability that an individual is at level 2 (the nonprejudiced response) on variable B, given that he is in latent class 1, is higher than the probability that he is at level 1 (the prejudiced response). The reason for this apparent anomaly is that variable B is not often responded to in a prejudiced way: even for individuals in latent class 1, the probability is high that the variable will elicit a nonprejudiced response. Note, however, that this probability is *extremely* high for individuals in latent class 2 ($\hat{\pi}_{22}^{\bar{B}X} = .99$ and .98 in 1972 and 1977, respectively), a point which will be returned to below. Variable D (SEG) represents a slightly different situation. Since $\hat{\pi}_{11}^{\bar{D}X} = .95$ and .93

and $\hat{\pi}^{\bar{D}X}_{12} = .80$ and .83 in 1972 and 1977, respectively, the variable barely discriminates at all between latent classes. The probability is very high for members of *both* latent classes that variable D will be responded to in a prejudiced way.

The latent class probabilities (the $\hat{\pi}^{X}_{t}$ in table 3) reveal an interesting pattern. The proportion of respondents estimated to be in latent class 1, the latent prejudiced class, remained constant at .24 over the five-year period from 1972 to 1977. Similarly, the proportion in latent class 2, the latent nonprejudiced class, decreased only slightly, from .57 in 1972 to .56 in 1977. Thus, taking account of all six indicators simultaneously, there is little change in the magnitude of the latent prejudiced and nonprejudiced classes over time, a conclusion that appears to contradict the earlier analysis of the six item marginals. More marked, although still not dramatic, changes are evident in the last two latent classes, that is, among the ideologues. The proportion of respondents estimated to be in latent class 3 decreased from .06 in 1972 to .03 in 1977, while the proportion in latent class 4 increased from .13 to .17 over this five-year period.[9] These results therefore indicate that the incidence of individuals exhibiting consistently prejudiced response patterns decreased from 1972 to 1977, and the incidence of those exhibiting consistently nonprejudiced response patterns increased. Such findings are generally consistent with the results of previous research, yet they more clearly specify the *pattern* of the reduction of prejudice in the data: finding a decrease in the occurrence of consistently prejudiced responses and an increase in the occurrence of consistently nonprejudiced responses lends important detail to the analysis of over-time trends in prejudiced attitudes.[10]

Table 4 displays the assignment of respondents into the four latent classes under model H_3 for the 1977 data and indicates how the latent variable can be constructed on the basis of the conditional probabilities. This is accomplished by predicting latent class membership from observed response patterns (this procedure is discussed more fully in Lazarsfeld and Henry 1968; Goodman 1974*b*; and Clogg 1981). If $\pi^{ABCDEF\bar{X}}_{ijklmnt}$ denotes the conditional probability that an individual with response pattern (i,j,k,l,m,n) will be in latent class t, then:

$$\pi^{ABCDEF\bar{X}}_{ijklmnt} = \pi^{ABCDEFX}_{ijklmnt} / \pi_{ijklmn}. \tag{4}$$

If all individuals with response pattern (i,j,k,l,m,n) are assigned to class t' of X, where t' is the level of X where $\pi^{ABCDEF\bar{X}}_{ijklmnt}$ is at a maximum, then the

[9] A caveat should be mentioned here. As one reviewer noted, to the extent that respondents have become increasingly unlikely over the past few decades to express prejudiced attitudes openly even when they exist, some misallocation to classes may result.

[10] This is not to say, however, that the increase in consistently nonprejudiced responses is accounted for by the decrease in consistently prejudiced responses. In fact, were the data longitudinal, we would expect to discover shifts over the five-year period from class 3 into class 1, from class 1 into class 2, and from class 2 into class 4.

TABLE 4

ASSIGNMENT OF RESPONDENTS INTO LATENT CLASSES FOR MODEL H_3, 1977 DATA

OBSERVED RESPONSE					LATENT CLASS* $F = 1$	LATENT CLASS* $F = 2$
A	*B*	*C*	*D*	*E*		
1	1	1	1	1	3	1
1	1	1	1	2	1	1
1	1	1	2	1	1	1
1	1	1	2	2	1	1
1	1	2	1	1	1	1
1	1	2	1	2	1	1
1	1	2	2	1	1	1
1	1	2	2	2	1	2
1	2	1	1	1	1	1
1	2	1	1	2	1	1
1	2	1	2	1	1	1
1	2	1	2	2	1	2
1	2	2	1	1	1	1
1	2	2	1	2	1	2
1	2	2	2	1	1	1
1	2	2	2	2	2	2
2	1	1	1	1	1	1
2	1	1	1	2	1	2
2	1	1	2	1	1	1
2	1	1	2	2	2	2
2	1	2	1	1	1	1
2	1	2	1	2	1	2
2	1	2	2	1	1	2
2	1	2	2	2	2	2
2	2	1	1	1	1	1
2	2	1	1	2	2	2
2	2	1	2	1	1	2
2	2	1	2	2	2	2
2	2	2	1	1	1	2
2	2	2	1	2	2	2
2	2	2	2	1	2	2
2	2	2	2	2	2	4

* The characterization of the latent classes is as follows: classes 1 and 2 refer to "latent prejudiced" and "latent nonprejudiced," respectively, while classes 3 and 4 refer to "intrinsically prejudiced" and "intrinsically nonprejudiced," respectively. Classes 3 and 4 denote ideologues whose response patterns are extreme with certainty.

assignments reported in table 4 result. Note that the two extreme response patterns, (1,1,1,1,1,1) and (2,2,2,2,2,2), constitute the third and fourth latent classes, respectively, while individuals with the remaining 62 response patterns are assigned to latent class 1 or 2. With the latent variable so characterized, it can be meaningfully employed in subsequent analysis. The data reduction here is substantial: the six original indicators have been collapsed to form a single four-class ordinal latent variable.[11]

An example will illustrate the applicability of this procedure in substantive analysis. Much past research has examined the relationship between antiblack prejudice and region of residence and concluded that prejudice is

[11] For an application of latent structure methods to the *scaling* of response patterns, see Clogg and Sawyer (1981).

significantly higher in the South than in the non-South (see, e.g., Hyman and Sheatsley 1956, 1964; Sheatsley 1966; Schwartz 1967; Greeley and Sheatsley 1971; Campbell 1971; Middleton 1976). It will be instructive to examine the distribution of the predicted latent variable by region to determine whether this pattern is discernible in these data. Table 5 presents this cross-classification for the 1977 data, with region dichotomized into South and non-South.[12] Examination of the regional variations displayed in the table reveals a distinct pattern. It is seen that 16.9% of southern respondents, compared with 8.0% of nonsouthern respondents, are located in the latent prejudiced class, and 61.1% of the former, compared with 63.5% of the latter, are located in the latent nonprejudiced class. Similarly, 11.1% of southern respondents, compared with only 2.8% of nonsouthern respondents, are in the intrinsically prejudiced class, and 10.9% of the former, compared with fully 25.7% of the latter, are located in the intrinsically nonprejudiced class. Thus the regional variations in prejudice highlighted by previous research remain in evidence. The South in 1977 continued to lag behind the non-South in racial tolerance as measured by the predicted latent variable.

As noted above, the conditional probabilities in table 3 can be utilized to estimate certain effects in structural equation models of the kind described by Goodman (1974*b*). The path coefficients in table 6 are calculated from the conditional probabilities of model H_3 (see Goodman [1974*b*] for a detailed discussion of this procedure). The coefficients indicate that the observed variables A, B, C, D, E, and F are all influenced by the latent variable X', which consists of the first two classes of the latent variable X in model H_3. The path coefficients indicate the effect of the latent variable on the odds that an individual will be at level 1 rather than level 2 on the

TABLE 5

CROSS-CLASSIFICATION OF THE PREDICTED LATENT VARIABLE AND REGION OF RESIDENCE, 1977 DATA

Class of Predicted Latent Variable	South*	Non-South*	*N*
1. Latent prejudiced	59 (16.9)	68 (8.0)	127
2. Latent nonprejudiced	214 (61.1)	539 (63.5)	753
3. Intrinsically prejudiced	39 (11.1)	24 (2.8)	63
4. Intrinsically nonprejudiced	38 (10.9)	218 (25.7)	256
Total	350 (100.0)	849 (100.0)	1,199

* Column percentages are in parentheses.

[12] The classification of South and non-South follows the census definitions for the major regions of the country.

TABLE 6

PATH COEFFICIENTS FOR QUASI-LATENT STRUCTURE MODEL H_3, 1972 AND 1977 DATA

	1972	1977
$X' \rightarrow A$ (MARR)	1.36	1.18
$X' \rightarrow B$ (DINN)	1.80	1.86
$X' \rightarrow C$ (PUSH)	1.20	1.18
$X' \rightarrow D$ (SEG)	.78	.49
$X' \rightarrow E$ (SCHOOL)	1.37	1.16
$X' \rightarrow F$ (PRES)	1.08	1.61

indicator variables $A, \ldots, F$, given that he is at level (i,j,k,l,m,n,l) on the remaining variables. The magnitudes of the $X' \rightarrow B$ paths, 1.80 in 1972 and 1.86 in 1977, are the largest, suggesting that the B variable (DINN) is the best indicator of the latent variable X' in both sets of data. Similarly, the $X' \rightarrow D$ paths, .78 and .49 in 1972 and 1977, respectively, are the smallest in magnitude, an indication that the D variable (SEG) is the least accurate indicator of the latent variable X'. Given the pattern of conditional probabilities displayed in table 3, these findings are not counterintuitive. The B variable (DINN) turns out to be the most accurate indicator of the latent variable, despite the fact that $\hat{\pi}^{\bar{B}X}_{11}$ is only .33 and .41 in 1972 and 1977, respectively, because the probability that an individual is at level 2 on variable B, given that he is in latent class 2 ($\hat{\pi}^{\bar{B}X}_{22}$), is *extremely* high, as noted above, and this offsets the fact that $\hat{\pi}^{\bar{B}X}_{11}$ is low in both years. Variable D (SEG), on the other hand, is the least accurate indicator of the latent variable because the probability is high, irrespective of latent class membership, that it will be responded to in a prejudiced way. Thus in both 1972 and 1977 respondents who are characterized as *either* latent prejudiced or latent nonprejudiced tended to adhere to the notion that they have a right to exclude blacks from their neighborhoods if they so desire.

The applicability of this path analytic approach to subsequent analysis should be emphasized. Since the method provides a means of assessing the relative importance of an indicator variable, additional data reduction is possible. In the present case, elimination of the D variable would quite possibly be justified on the basis of its status as a poor indicator of the underlying latent variable.

SUMMARY

This note has analyzed six survey items designed to measure prejudiced attitudes toward blacks. The applicability of latent structure methods in the analysis of these discrete variables has been illustrated, and several latent class models have been presented and evaluated. The quasi-latent

structure model H_3 is well suited to the task of analyzing the pattern of responses in these data. This model, which posits the existence of a four-class ordinal latent variable that can account for the association among the six observed variables, provides a very compelling fit to the data. By taking account of ideological response patterns in addition to latent prejudiced and nonprejudiced classes, model H_3 provides for a parsimonious interpretation of these six indicator variables. The model indicates that the relative magnitudes of the latent prejudiced and nonprejudiced classes remained constant over the period 1972 to 1977, a finding somewhat at odds with previous investigations that have not examined a set of indicators simultaneously. The incidence of individuals exhibiting *consistently* prejudiced responses, however, did decrease, and this may partially account for the overall decline in prejudiced attitudes typically reported in previous research. The assignment of individuals to classes of the latent variable was illustrated, and use of the resulting ordinal variable suggested. The utility of latent structure methods in assessing the relative importance of an indicator variable has been demonstrated. It has been argued that, by taking account of all the items simultaneously, latent structure methods are particularly applicable to analyzing trends over time.

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Estimating the Effects of Marriage Timing on Educational Attainment: Some Procedural Issues and Substantive Clarifications[1]

Karl L. Alexander and Thomas W. Reilly
Johns Hopkins University

The importance of marriage timing for educational attainment is examined for a national sample of young men and women. In attempting to deal with technical deficiencies in earlier research on this issue, we distinguish schooling obtained prior to marrying from that obtained afterward and adjust both measures for the age-grading of school enrollment patterns. We focus on the implications of marriage timing for schooling obtained after one marries. We conclude that marrying early is detrimental to the educational attainments of women but of little or no consequence for men. We also demonstrate that formulations of the age at marriage–educational attainment relationship similar to those employed in other studies would have led to quite different conclusions.

FAMILY FORMATION AND EDUCATIONAL ATTAINMENT

Recent advances in both the sociology and psychology of life-course development (Atchley 1975; Clausen 1972; Jordaan 1974) have stimulated interest in the timing and sequencing of critical life events and in the "spillover" from experiences in particular institutional settings into other spheres of activity. In the educational literature, this interest is reflected in a renewed concern with the effects of family role transitions (i.e., marrying and childbearing) on school progression. Age at marriage, in particular, has received much attention as a career contingency in research on levels of educational attainment.

There is, of course, a substantial association between timing of marriage and levels of education (Glick and Carter 1958; Rele 1965). Both theory

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(Elder 1975) and research (Bacon 1974; Burchinel 1959; Elder and Rockwell 1976) indicate that early entry into marriage and parenthood often incurs numerous disadvantages, including educational sacrifice: "Early marriage identifies a life course of relative deprivation—low socioeconomic origins, restricted formal education, marriage into the lower strata, heavy childcare burdens and inadequate material resources" (Elder and Rockwell 1976, p. 51).

The strain between marriage and schooling certainly is suggested in much of the literature on school attrition. Marriage and parenthood are among the strongest predictors of dropping out of college (Bayer 1968; Folger, Astin, and Bayer 1970) and are offered frequently as reasons for either not beginning or not completing college by both men and women, though much more often by the latter (Astin and Panos 1969; Trent and Medsker 1968).

Recent attempts to model formally the consequences of marriage timing for educational attainment have incorporated quite contrasting assumptions regarding the character of the relationship or relationships between these variables, with two basic distinctions appearing recurrently. Several studies have accorded priority to family events, employing a simple recursive specification in which age at marriage intervenes between various family background indicators and eventual levels of educational attainment (Call and Otto 1977; Kerckhoff and Parrow 1979; Waite and Moore 1978). Kerckhoff and Parrow and Waite and Moore concluded from their analyses that early marriage had a substantial negative effect on the educational attainments of the men and women in their respective surveys. Call and Otto, in contrast, found little influence of marriage timing in their sample of males. In addition to these somewhat contradictory findings being a cause for concern, all three studies acknowledge uneasiness over the simplifying assumption that school enrollments exercise no influence on the age at which one marries.[2]

The recognized need to attend to the reciprocal effects of marriage timing and school attainment in order to draw accurate conclusions regarding the role of either in life-course development has motivated a second class of models in the literature. These all employ nonrecursive structural equations procedures to estimate simultaneously the influence of age at first marriage on level of educational attainment and the influence of educational attainment on age at first marriage (Marini 1978; Moore and Hofferth 1978; Voss 1975). These studies too are at odds with one another in the general patterning of their conclusions. Voss, for example, found early marriage to

[2] Waite and Moore (1978) acknowledge sidestepping the matter of causal priorities in these relationships, while Call and Otto (1977) disavow responsibility for their model specification, evaluating uncritically the framework developed by Nye and Berardo (1973).

be an educational *asset* for both men and women, especially for the former. Marini, in contrast, found that early marriage reduced subsequent educational attainment—but significantly only among women.[3] Beyond these contradictory conclusions, however, we believe that the conceptualization of the age at marriage–educational attainment relationship is deficient in this literature as well, and that the seeming refinement of the nonrecursive specification may actually be little better suited to the issues than the recursive alternative on which it supposedly improves. Our reservations along these lines warrant brief elaboration. They apply specifically to the three studies at hand but may have broader relevance as well.

At the most general level, all three studies recognize that occurrences in one institutional setting may have implications for what occurs in the other; in particular, that school progression is likely to be affected by family events, and, in turn, that family events may be influenced by schooling considerations. The three studies then apply this general imagery in straightforward fashion to the specific measures of family and educational experience at issue. They assume that age at marriage and educational attainment are mutually influential.[4]

There is, of course, a tremendous gap between the general sense that family and school factors may have relevance for one another and the formal assumption that two specific measures originating in the family and school are reciprocally related. To make this move from the general to the specific, substantive arguments justifying the use of these research variables and the assumption that they stand in reciprocal relationship to one another must be offered. In this instance, however, no such justifying rationale is advanced. In fact, consideration of the nature of the variables themselves seems to make a prima facie case against their being reciprocally related.

To begin with, the timing of marriage and the age at which schooling is concluded differ widely in relation to one another across persons. Among the men and women of the present sample (see sample description below), for example, 27% of the men and 16% of the women report two or more years of additional academic enrollment after marriage. Similarly, many men and women do not marry for several years after completing their educations. Overall, only 13% of the men and 29% of the women who obtained no additional schooling after marrying concluded their educations in

[3] Moore and Hofferth (1978) use a more complex model specification, which makes direct comparisons with Voss (1975) and Marini (1978) difficult. Nevertheless, the general patterning of their results, which apply only to women, is more in accord with Marini's than with Voss's.

[4] The paper by Moore and Hofferth (1978) considers both age at marriage and age at the birth of one's first child as factors affecting educational attainment. The other two consider only marriage timing, as does the present study. Our comments regarding the modeling of life-course events apply to both aspects of family formation, marriage and fertility.

the first year of their marriages.[5] Although it is not necessary that events be in close temporal proximity to one another for them to be reciprocally related, it is unlikely that any simple model of mutual influence could account for this diverse pattern of timing and sequencing.

More generally, variables which index the occurrence of life-course events are of such a nature that they probably cannot conform to the assumptions that underlie statistical estimation models of the sort employed in this research. Simultaneous-equations procedures for estimating reciprocal influence, such as two-stage least squares, employ the logic of comparative statics. Inferences about how variables have adjusted to changes in one another's values *within cases* are drawn from comparisons *across cases* in the currently stable situation (i.e., the present equilibrium point). Age at first marriage and level of educational attainment are not, however, the sort of variables that *can* adjust to one another successively within cases. That is, they both "happen" only once over the life course, and once they have occurred, they are invariant thereafter. Hence, the imagery of variables mutually adjusting to shifts in one another's values until an equilibrium point is reached does not seem especially applicable to events which are unique and invariant over the life course.

It may be, then, that such statistical models are not appropriate to variables of this sort, and, conversely, that such variables cannot stand in relation to one another in the way assumed by these models. Therefore it is difficult to know what interpretation should be lent to the coefficients of reciprocal influence presented in these studies. It is conceivable, of course, that they do in fact quantify what they are intended to quantify, but this is hardly self-evident. Until it is demonstrated explicitly, caution and skepticism are warranted.

There is, fortunately, an alternative research strategy which avoids these uncertainties and still allows us to address the issues most central to the studies we have just examined: whether early marriage is a deterrent to educational attainment, and, if it is, whether it is a more serious one for women than for men. We can treat these issues by distinguishing education accumulated prior to marriage from that obtained after marriage and by focusing on the second component.[6] The distinction enables us to ask how the likelihood of continuing in school after marrying differs as a function of the age at which marriage occurs. If early marriage is indeed a liability, as both theory and conventional wisdom suggest, this fact should be revealed

[5] "Years of enrollment" in this context refers to the number of calendar years during which the respondents received some formal academic preparation, rather than completed academic years. See the discussion of age-adjusted years of school enrollment prior to and after marriage in the Methodology section for further detail.

[6] Kerckhoff and Parrow (1979) recognize the desirability of this distinction and approximate it in their own analysis.

in differences between the postmarriage school experiences of early and late marriers.

This approach is parallel to that employed in several recent studies of earnings attainments (Griffin 1978; Griliches and Mason 1973; Mason 1974), in which valuable resources, such as work experience and schooling, are segmented into the quantities obtained before and after some other salient life experience (e.g., military service). With components expressed in this way, the separate consequences of each can be estimated. Theory often suggests these effects should differ. In the present instance, distinguishing schooling obtained after marrying from that obtained previously allows us to isolate more clearly the educational consequences of marriage timing. In fact, Bielby, Hawley, and Bills (1977, p. 59) recently have made a similar observation regarding research on the educational and career consequences of family events. In a related context, Cramer (1980) too has argued recently the merits of this sort of dynamic approach over the customary static one for studying the interdependencies among life-course events.

There remains, however, one further complication in making substantive sense out of the relationship between age at marriage and educational attainment. Age at marriage subsumes two major components: age and the occurrence of a socially significant role transition. It is important, therefore, that these two components be distinguished. Otherwise we run the very real risk of mistaking age effects for those of marriage timing. Since school enrollments themselves tend to be age patterned, a negative association is "built into" the relationship between age at marriage and years of postmarriage schooling. That is, we would expect older marriers to have fewer years of later enrollment simply because older persons generally will already have completed more of their education.

What we want to do, then, is isolate the way in which enrollment patterns following marriage at a particular age depart from some appropriate reference group's educational experience beyond that age. Thus, marriage effects would show up as deviations from the reference baseline, with beneficial consequences of marriage appearing as enrollment beyond that which would have been expected otherwise and liabilities of marriage being revealed as shortfall from the comparison group.[7]

To take account of the age-grading or patterning of school enrollments, we have constructed two control variables, one for postmarital schooling and one for premarital schooling. Each is based on the enrollment histories of those in our sample who remain unmarried at various benchmark ages.

[7] We considered working with the unadjusted enrollment measures in the analysis and using the adjustment variables as statistical controls, but this resulted in excessive collinearity in elements of the data matrix. We therefore decided to remove age effects first and use the residualized enrollment measures in the analysis.

The first indexes, for every age at marriage represented in the data, the average years of enrollment after that age obtained by same-sex respondents still unmarried as of that age. The second does the same for years of enrollment prior to particular ages. To partial out the age component from age at marriage, we simply express each individual's pre- and postmarital schooling as deviations from the sex-specific control values for the age at which that respondent married. For example, if a woman in our sample who married at age 20 received 13 years of premarital schooling and half a year of postmarital schooling, and the still unmarried female sample as a whole (regardless of marriage timing) received, on the average, 13.4 years of schooling prior to age 20 and 1.3 years of schooling after age 20, the respondent's age-adjusted scores would be —.4 for premarital enrollment and —.8 for postmarital enrollment. This example employs, incidentally, the actual sample averages for our female sample and the age-adjustment values for age 20. These deviations from the sample's age-normed averages are the variables actually employed in the analysis.

These age-adjusted deviation scores are used to index pre- and postmarital educational attainment in a simple recursive model of the effects of age at first marriage on postmarital schooling.[8] Our analysis also controls for several indicators of SES background and tested academic aptitude, which are known from previous literature to be important for both age at marriage and school attainments. The premarital enrollment measure also is used as a control variable in the equation predicting postmarital attainments. The analysis is conducted separately for men and women. We are especially interested in whether family constraints weigh more heavily on the educational progress of women, thereby contributing to their educational shortfall relative to men.[9] Finally, in order to assess the substantive implications of our procedural refinements, we will present three other estimations of the effects of age at marriage on school attainment: those obtained (*a*) when pre- and postmarital enrollments are not adjusted for the characteristic age-grading of such enrollments; (*b*) when pre- and postmarital enrollment are not distinguished and age at marriage is used as a determinant of overall level of educational attainment, as in the studies by Call and Otto, Kerckhoff and Parrow, and Waite and Moore; and (*c*) when a non-

[8] It should be noted that we do not give explicit consideration to certification levels in the analysis. Since certification, especially when scaled to years as is common practice, follows definitionally from enrollment patterns, to consider both would be redundant. The fact that this is not entirely true of our data is due to the crudeness of our measures, not to substantively interesting considerations. Our pre- and postmarriage enrollment measures together account for about 75% of the variance in men's levels of educational certification and 66% in women's.

[9] Nonrecursive estimations have attempted to tap the reciprocal relation between age at marriage and educational attainment. An anonymous reviewer correctly noted that we have not isolated nonmarriage after education. For this paper we have chosen to focus instead on the effect of marriage timing on educational attainment.

recursive specification linking age at marriage and level of educational attainment is used, as in the studies by Marini, Moore and Hofferth, and Voss. This final step should indicate how sensitive our conclusions are to the assumptions of the analysis regarding the character of the focal age at marriage–educational attainment linkage.

METHODOLOGY

The data for this analysis are from a national sample of youth, first studied in 1955 as high school sophomores and followed up in 1970. The original 1955 survey, conducted by the Educational Testing Service (ETS), included all sophomores in 97 schools. Unless otherwise noted, the analyses will be based on the subsamples of ever-married men and women for whom data are available for all variables included in the models, numbering 720 and 892, respectively.

The final sample targeted for the 1970 follow-up survey consisted of 42 schools with 4,151 sophomores. Usable follow-up data were obtained for 2,077 former students, 1,130 females and 947 males. The 50% overall nonresponse consists of 32% refusals, 16% lost cases (persons for whom current addresses could not be located), and 2% known deceased. The major sample and response biases involved an underrepresentation of large urban schools and low-aptitude students. These sample biases, of course, temper the generalizability of our results, especially since the project design underrepresents youth likely to marry extremely early. Nevertheless, careful assessment of the biases rising from various sources of sample attrition has found them in general to be surprisingly modest (see Alexander and Eckland 1973, pp. 57–69).

Variable Measurement

1. Four background status variables are employed in this analysis; except for some missing data estimates, all were obtained from the 1970 schedule: (1) mother's education and (2) father's education, originally measured as levels of educational certification and rescaled to years of schooling completed; (3) father's occupation while the respondent was in high school, coded in the Duncan SEI scale; and (4) a 13-item factor-weighted "acquisition" index of possessions in the respondent's high school household.

2. Academic aptitude was measured with a 20-item test administered by ETS during the 1955 survey. The test gives equal weight to vocabulary and to arithmetic reasoning and has an estimated reliability of .80.

3. Age at first marriage, obtained from retrospective annual reports of significant life events included in the 1970 survey, was calculated by finding the first year in which the respondent indicated he or she was married.

This calendar year was then converted to a value reflecting the number of years since 1955 and was added to the respondent's age in 1955.

4. Academic enrollments prior to and after marriage were also obtained from the retrospective annual reports. The data distinguished among types of schools attended, so that enrollment measures could be limited to academic training. Unfortunately, however, we have no information on the quantity of schooling within calendar years; therefore only crude in-out distinctions can be made. Thus, premarital enrollment for most cases (see the qualification below) is the number of calendar years in which some academic schooling had been obtained through the first year of the respondent's first marriage. Postmarital enrollment is for most cases the number of calendar years through 1970 in which some academic schooling had been obtained following the first year of marriage. These operationalizations were used for all respondents except those who were enrolled both in the year of marriage and in the following year. For these few cases, schooling during the marriage year was credited to postmarital enrollment. Our decisions regarding the treatment of education during the first year of marriage were guided by inspection of uncoded life-history data for a subsample of our respondents. For the vast majority of respondents for whom exact timing could be determined, schooling in the year of marriage was completed prior to the actual date of marriage.

To adjust for the age patterning of enrollments, the mean number of years of academic enrollment up to and including a given age and a second mean for enrollment beyond that age were obtained separately for both males and females who eventually married but had not yet married at each age. The age-adjusted measure of academic enrollment prior to marriage was defined as the respondent's number of years of enrollment prior to marriage minus the mean number of years of enrollment realized by the same-sex subsample still unmarried at the respondent's age at marriage. The age-adjusted measure of academic enrollment after marriage was similarly defined but in years of enrollment after the respondent's age at marriage.

RESULTS AND CONCLUSIONS

The determinants of postmarriage enrollments are presented separately for men and women in table 1. Also included are the coefficients predicting age at first marriage and schooling prior to marriage. The latter results are quite unexceptional and require little discussion. Our findings regarding postmarriage schooling, on the other hand, are of considerable interest, and we turn directly to them.

Even with premarriage enrollment levels controlled, high tested ability still emerges as a valuable educational asset among both men and women.

TABLE 1

METRIC AND STANDARDIZED COEFFICIENTS PREDICTING EDUCATION AFTER MARRIAGE FROM BACKGROUND VARIABLES, EDUCATION PRIOR TO MARRIAGE, AND AGE AT MARRIAGE, SEPARATELY BY SEX

Dependent Variable	Moth Ed	Fath Ed	Acquis	Fath Oc	Apt	Educ Pre	Age Marr	R^2	$\bar{X}$	SD
				Males (N = 720)						
Educ pre.....	.019	.032	.393*	.016*	.181*	...	...	.275	−.350	2.141
	(.025)	(.054)	(.161)	(.161)	(.332)					
Age marr.....	−.054	.034	.554*	−.000	.096*	...	...	.054	22.117	3.001
	(−.052)	(.041)	(.162)	(−.002)	(.125)					
Educ post.....	−.001	.033	.096	.005	.076*	.162*	.065*	.109	−.416	2.259
	(−.001)	(.053)	(.038)	(.044)	(.132)	(.154)	(.086)			
				Females (N = 892)						
Educ pre.....	.057*	.048*	.187*	.006*	.093*	...	...	.206	−.281	1.631
	(.110)	(.115)	(.109)	(.076)	(.233)					
Age marr.....	−.015	.050	.501*	.005	.047*	...	...	.068	19.767	2.768
	(−.017)	(.072)	(.171)	(.043)	(.070)					
Educ post.....	.034	−.000	.087	.008*	.047*	.032	.131*	.167	−.825	1.497
	(.071)	(−.001)	(.055)	(.117)	(.127)	(.035)	(.243)			

NOTE.—Asterisks signify coefficients at least twice their standard errors. Figures in parentheses are standardized coefficients. Variable abbreviations are as follows: moth ed, mother's education; fath ed, father's education; acquis, household acquisitions index; fath oc, father's occupational status; apt, academic aptitude; educ pre, age-adjusted years of school enrollment prior to marriage; age marr, age at first marriage; educ post, age-adjusted years of school enrollment after marrying.

High socioeconomic origin, in contrast, results in little direct benefit for either sex, with none of the four SES indicators being significant for men and only one (father's occupation) being marginally important for women. We note as well that the explained variance in these equations is modest, but this should not be very surprising since in this analysis the total variance in school enrollments is partitioned into two components (pre- and postmarital). Thus the explanatory power of these equations is considerably less than would be observed if similar background characteristics were used to predict educational certification (see table 2 for comparisons along these lines).

The consequences of schooling prior to marriage for attendance after marriage are, perhaps, somewhat surprising. For men but not women, greater investments in schooling prior to marriage increase modestly the likelihood of continuing in school after marrying. While this effect is not especially large, something of a trade-off between pre- and postmarital enrollment might well have been expected. This certainly is not at all evident in our results, however. Instead, for males a cumulative advantage is suggested, with additional schooling accruing most often to those with more premarital enrollment.

Last, we consider how age at marriage affects schooling subsequent to marriage. For men, the timing of marriage has little effect on later academic progression (the standardized coefficient for men falls below .100, a common standard for imputing substantive significance). For women, on the other hand, the consequences of marrying early are much more substantial. The metric coefficient for women is twice as large as that for men.[10] For each year that marriage is deferred, it is estimated that women realize about an eighth of an age-adjusted year of schooling. For women, then, early marriage is an educational liability, and no doubt it is also an important factor in women's reduced educational attainment relative to men. On the average, males obtain .629 of a year more postmarital schooling than do females in this sample. In terms of our age-adjusted measure, males obtain .409 of a year more postmarital schooling, suggesting that differences in typical age patterning of events account for approximately a third of the sex difference in years of enrollment. Therefore, two-thirds of the gap between male and female enrollments remains after adjusting for age differences in the patterning of school attendance. These results suggest that, if women married at the same age as men (raising their age at marriage by 2.35 years, on the average), they would obtain almost a third of an age-adjusted year more schooling, again on the average, than they do at present. This is approxi-

[10] An anonymous reviewer noted that this pattern, coupled with the sex disparity in the effect of premarital enrollment on postmarital enrollment, suggests that women make a kind of either-or decision with respect to marriage and education more often than men do. This strikes us as a plausible interpretation.

mately three-fourths of the observed sex difference in age-adjusted postmarital enrollment.

These last conclusions represent our best estimate of the consequences of marriage timing for educational attainment. What stands out as especially striking is the sex specificity of the liability associated with early marriage. Apparently only women's school progression is appreciably deflected by the disruptions attending family formation. However, would we have reached the same conclusion had we conducted the analysis along the lines of previous studies of these issues? The results in table 2 speak to this question.

Table 2 presents the critical effect of age at marriage on educational attainment from four different specifications of this relationship. The first set of results is taken from table 1. We take these as our benchmark for evaluating the others.

The second specification distinguishes premarriage enrollment from postmarriage enrollment, as does the first, but does not adjust these measures for the age patterning of school enrollments. Using these variables in the analysis, we get wholly different results, with early marriage seeming to be a substantial educational *asset* for both men and women. This illustrates—rather forcefully, we think—how misleading the analyses of life-history events can be when the characteristic age-grading or patterning of such experiences is ignored.

The third analysis corresponds to a recursive formulation in which marriage timing is accorded causal priority over school attainments without regard to the sequencing of marriage and schooling. Here total years of enrollment, instead of enrollment after marriage, is used as the dependent variable.[11] These results also differ in important detail from those discussed above. In this estimation, both men and women suffer substantially from marrying early. Moreover, although the earlier sex disparity is reproduced here, the relative difference between the sexes is not nearly as great. In the first specification the male coefficient is substantively trivial and barely half that of the female coefficient, whereas in this solution it is both large in absolute terms and almost two-thirds the size of the female effect. Overall, marriage timing appears to be much more important in this analysis.

The fourth set of results, produced by a nonrecursive specification of the

[11] To preserve a common metric across these analyses, total years of enrollment, rather than level of educational certification scaled to yearly equivalents, is used as the dependent variable here as well as in the nonrecursive specification. A certification measure is used most often in other studies. In this study, when such a measure is substituted for the enrollment variable, the major change is an attenuation of the male coefficient, making it just under half the size of the corresponding effect for females (respective metric coefficients are .139 and .296). Both of these are considerably larger than those we obtain for postmarriage enrollment, but they do reflect the sex disparity obtained in our original analysis.

TABLE 2

FOUR ESTIMATIONS OF THE CONSEQUENCES OF AGE AT MARRIAGE FOR EDUCATIONAL ATTAINMENT, SEPARATELY BY SEX

	MALES				FEMALES			
	Metric	Stand.	*N*	R^2	Metric	Stand.	*N*	R^2
Educ post, adjusted[a]	.065*	.086	720	.109	.131*	.243	892	.167
Educ post, unadjusted[b]	−.211*	−.284	722	.113	−.087*	−.169	893	.083
Total educ, recursive[c]	.213*	.180	722	.308	.336*	.365	893	.414
Total educ, nonrecursive[d]	.353*	.293	657	...	.401*	.428	794	...

NOTE.—Asterisks signify coefficients at least twice their standard errors.

[a] Figures repeated from table 1.

[b] Same predictors as in table 1 but without age adjustment for the enrollment measures.

[c] Dependent variable is sum of educ pre and educ post without age adjustments.

[d] In order to identify the nonrecursive equations, a measure of educational plans while in high school was used as an instrumental variable for educ and a dummy variable indexing whether or not the respondent had met his/her present spouse while in high school was used as an instrumental variable for age marr. The wording of the age marr instrument dictated that this analysis be limited to once-married respondents, which accounts for the sizable sample attrition relative to the other analyses. Jöreskog's progam LISREL IV was used for the nonrecursive estimation, which in this application is equivalent to a two-stage least-squares solution. For additional details on model specification and comparisons with other nonrecursive models from this literature, see Alexander, Reilly, and Fennessey (1980).

relationship between age at marriage and total years of enrollment, is very similar to the third in its general patterning.[12] The effects of age at marriage are, if anything, even greater in this solution, and they are even more alike for men and women here than in the immediately preceding results. Here again, the implications of the analysis would be very much at odds with our original conclusions.

It appears likely, then, that previous formulations of the consequences of marriage timing for educational attainment have both overstated the educational penalty suffered by early marriers and understated the sex disparity in this liability. Although previous studies of this topic have been inconsistent in their detailed findings, our assessment of alternative specifications suggests that their conclusions are probably biased along the lines indicated and should be adjusted accordingly in interpretation. Our own results reveal more modest, but nevertheless important, consequences of marriage timing for school attendance patterns. We also conclude that the extent and nature of family intrusions upon educational progress differ for men and women, with the disruptions that follow from early marriage being more severe for women.

In view of these conclusions, it is hoped that future research will shed further light on the ways in which both men's school attendance and women's are affected by family obligations. It would be especially interesting to explore whether, and how, changing patterns of family formation and changes in family-oriented sex roles and sex-role attitudes might account for recent increases in female college attendance. These inquiries should inform our understanding not only of individual academic progress but also of the conditions of the contemporary family—and in so doing provide much useful information for career planning and counseling.

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An Assessment of Japanese American Assimilation, Pluralism, and Subordination[1]

Eric Woodrum
North Carolina State University

Japanese Americans are an atypical American minority with traits characteristic of both European immigrants and subordinated racial groups. They provide a test for the influence of relatively free labor versus racial minority statuses. Japanese American assimilation, pluralism, and subordination are assessed with census data and with generation-specific surveys by the Japanese American Research Project. Assimilation trends in language, residence, primary relations, exogamy, and socioeconomic status affirm parallels with voluntary European immigrants. Continuing pluralism is indicated in value systems, family relations, and community participation. However, ratio indicators confirm that racial subordination persists, even among highly educated, professional Japanese Americans.

Japanese Americans are an anomaly in the study of racial and ethnic relations. As a racial minority they suffered institutionalized discrimination ranging from naturalization prohibitions to massive wartime incarcerations. In spite of this, the Japanese entered this country voluntarily and climbed the class structure more rapidly than many white immigrant groups. Neither the European immigrant assimilation model nor the nonwhite colonial subordination model of American ethnic relations fits this group's experiences well. Indeed, Japanese Americans provide a useful test case for assessing the relative impact on group adaptations of racial minority status versus relatively free labor status.

This study assesses Japanese American adaptations with respect to three prevalent patterns in American racial and ethnic relations: assimilation, cultural pluralism, and subordination. Assimilationists anticipate progressive acculturation, geographic and structural integration, and eventual group merger (Gordon 1964). Cultural pluralists emphasize the persistence

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of ethnic cultural traits, family practices, subcultural activities, and distinctive identities (Greeley 1974). Racial conflict and subordination theorists stress economic and political interests and the institutional context of group adaptation (Blauner 1972; Feagin and Feagin 1978). Unlike assimilationists and cultural pluralists, subordination theorists expect persistent inequality. Because Japanese Americans exhibit selected characteristics of both voluntary European immigrants and colonized nonwhite subordinates, their experiences can shed light on the relative impact of these factors.

DATA AND METHODS

This assessment first explores the extent of Japanese American integration and assimilation with the host society. Data on geographic, residential, cultural, primary structural, identificational, and marital variables will be presented. Next, continuing cultural and structural pluralism is considered with generational data on cultural traditionalism, distinctive familial practices, ethnic newspaper reading, visiting habits, and assistance securing employment. Finally, Japanese American–white group conflict and subordination are explored with information on comparative occupational distributions, education, and income, and with ratio variables.

The research utilizes two primary data sources, the Japanese American Research Project (JARP) and the U.S. Bureau of the Census. The JARP contacted a large sample ($N = 1{,}047$) of first-generation Japanese immigrants to the United States mainland, *Issei,* in the early 1960s (Bonacich 1975). The respondents were interviewed on a broad range of topics by trained bilingual Japanese Americans in sessions lasting an average of six to eight hours. The first-generation respondents' children, *Nisei* ($N = 2{,}304$), and grandchildren, *Sansei* ($N = 802$), were subsequently contacted with face-to-face and telephone interviews and mail questionnaires. The JARP information is critical for evaluating cultural, primary structural, residential, and identificational characteristics of each generation.

The U.S. census data facilitate comparisons between the distributions of Japanese Americans and whites across geographic and urban-rural areas. Census data are relied on primarily for information on geographic distributions and secondary structural adaptations including educational, occupational, and other economic achievements.

FINDINGS

The Extent of Japanese American Assimilation

Japanese immigrants were overwhelmingly concentrated on the West Coast upon entry, a condition which encouraged immigrant separatism and ag-

gravated anti-Asian hysteria among whites. Thus the extreme geographic concentration of Japanese Americans was both a manifestation of and a factor contributing to their general separatism. The extent of their relative geographic dispersion in recent decades can be determined from census data. The Japanese American regional distribution is gradually resembling that of U.S. whites, a trend accelerated by the World War II evacuation and internment, but very large differentials persist. Between 1940 and 1970 the percentage of Japanese Americans concentrated in the western states dropped from over 95% to 81%. Migration to the north central and northeastern states grew more rapidly than to the south. Net regional differentials dropped from 84% to 63.4% over the 30-year span. A contrary trend appears in the data on Japanese American–U.S. white population distributions in urban and rural areas: the differential is growing. Although the minority was initially slightly more rural than whites (45.1% vs. 42.5%), they are now significantly more urban (88.6% vs. 72.4%) than the majority group.

The JARP survey data refine these general findings by specifying the ethnic composition of respondents' neighborhoods. One finds progressive cross-generational and intragenerational trends toward residential integration. Only the earliest Issei, before 1908, lived more often in mostly Japanese American (47.9%) than in mixed (20.6%) or mostly non–Japanese American (31.5%) neighborhoods. Between 1910 and the World War II internment, approximately 70% of Issei and Nisei lived in mixed or mostly non–Japanese American communities. In the postinternment years, fewer than 10% of any generation has lived in mostly Japanese American neighborhoods, and over half of the Nisei and Sansei have lived in predominantly non–Japanese American areas.

Assimilation theorists traditionally emphasize cultural, primary structural, identificational, and marital integration of immigrants. Table 1 pre-

TABLE 1

ASSIMILATION INDICATORS BY GENERATION, IN PERCENTAGES

INDICATOR	GENERATION		
	Issei	Nisei	Sansei
Acculturation:			
English fluency	16.6	92.9	100.0
Western religious affiliation	33.5	52.6	55.2
Primary structural assimilation:			
Friends, over one-half non–Japanese Americans	3.3	30.3	54.9
Voluntary organizations, over one-half mostly non–Japanese American members	.9	44.9	61.5
Identificational assimilation:			
Neutral or positive attitude toward exogamy with whites	44.4	59.3	89.6
Marital assimilation:			
Exogamy rates	1.5	8.8	34.1

sents JARP data to assess the degree of merger with the dominant society in these respects among the first three generations. Although only a significant minority of Issei became proficient in English, most of the second generation achieved fluency. Nisei and Sansei, growing up in non-Japanese neighborhoods and attending American public schools, thoroughly mastered the host language, as the findings in table 1 affirm. Religious acculturation has been far less complete. Upon arrival in the United States, 81.4% of the Issei were affiliated with traditional Japanese religions, mainly Buddhism and Shintoism, and only 10.2% identified with Western religions. By the time of the JARP interviews, Western religious affiliation had tripled to include one-third of the Issei. Slightly over one-half of the Nisei (52.6%) and Sansei (55.2%) identify with Judeo-Christian religions. An alternative adaptation, nonaffiliation, is steadily increasing. Of newly arriving Issei, 8.4% did not identify with any religion or were atheists; by the time of the interviews this proportion had dropped to 3.3%. Nisei report no religious identification in 9.0% and Sansei in 19.5% of the cases sampled. Sansei youthfulness dictates caution in interpreting this finding.

The Issei rarely had white friends or belonged to predominantly white voluntary groups. Integration at the friendship and organizational level proceeded among Nisei, who usually spoke excellent English, lived in mixed neighborhoods, and attended white schools. The majority of the third generation reports over one-half non–Japanese American friends (54.9%) and membership in predominantly non–Japanese American voluntary organizations (61.5%). These behavioral changes accompany an attitudinal change probably associated with group identification: attitude toward exogamy. While 44.4% of the Issei report they would not be disturbed by their child's marriage to a white, 89.6% of their grandchildren would not regard it as disturbing. This change in attitude is correlated with a multiplying exogamy rate. Issei separatism and antimiscegenation laws limited the first generation's exogamy to 1.5% despite a shortage of Japanese American women. That figure climbed to 8.8% among Nisei and 34.1% among the JARP Sansei sample. Studies since the JARP surveys report even higher estimates of Sansei exogamy, approaching 50% in various areas (Kitano 1976, pp. 106–7).

Cultural Pluralism among Japanese Americans

The findings discussed above indicate a substantial acquisition of Western culture by the second generation; now we explore whether this was due to biculturation or to the displacement of a distinctive Japanese life-style by a contemporary American one. In table 2, generation-specific cultural, familial, and subcultural participation data help answer this question.

TABLE 2

PLURALISM INDICATORS BY GENERATION, IN PERCENTAGES

INDICATOR	GENERATION		
	Issei	Nisei	Sansei
Cultural traditionalism:			
Japanese fluency	100.0	19.3	2.4
Japanese religious affiliation	62.6	36.1	23.6
Familial traditionalism:			
Shushin instruction	97.8	68.7	55.7
Importance of family line continuity	79.5	44.2	48.8
Parental marital influence	83.9	30.7	42.2
Participation in Japanese American community:			
Japanese American newspaper reading	91.4	64.0	46.4
Regular visiting with relatives	56.9	71.5	68.6
Japanese American help securing employment	95.9	50.6	58.8

Japanese-speaking proficiency has steadily eroded. Of Nisei and Sansei, 19.3% and 2.4%, respectively, are fluent in their ancestors' language. This condition complicates communication between the first generation and their grandchildren, but evidence presented below suggests that significant cultural transmission occurs despite this obstacle. Although Japanese religious affiliation dropped from 62.6% of the Issei to 36.1% of the Nisei generation, a considerably smaller drop occurs in the Sansei generation (to 23.6%). The finding that nearly one-half of the Sansei either are Buddhist or do not identify with any religion indicates that this immigrant group has only selectively adopted the dominant culture.

The Japanese American family has regularly been cited as a mainstay of their solidarity and distinctiveness (Light 1972; Kitano 1976). A major child-rearing practice for cultural transmission of traditional ethnic-moral values was *Shushin* instruction, stressing standards for proper behavior, respect for familial authority, and pride in the Japanese community in keeping with Confucianism and Shintoism. As reported in table 2, 97.8% of the immigrant generation recalled Shushin instruction in Japan. Over two-thirds, 68.7%, of their children received similar training from their parents in addition to traditional moral instruction at Japanese language schools, which over 79% of the sampled Nisei attended. Among Sansei, 55.7% experienced Shushin socialization from parents or grandparents and over 79% believe they should know more about Japanese culture. Ethnic identity among young Japanese Americans has been growing with the Asian American movement.

The traditional value placed on continuity of the family line is also enduring according to these research findings. After reported concern to maintain the family line dropped from 79.5% of the first generation to 44.2% of the second, it rebounded to 48.8% among Sansei. The pattern is replicated in findings on parental influence on marriages, a traditional

Japanese family practice. Nearly 84% of the ever-married Issei indicate that their Japanese parents arranged their marriages, reflecting the "picture bride" adaptation of a long-standing practice. Only 30.7% of the Nisei report that their parents tried to influence their marriages; yet 42.2% of the Sansei respond positively to the same question. Parental attempts to influence the selection of one's spouse cannot be equated with an arranged marriage, but the weight of these findings and those of others (Kitano and Kikumura 1976) is that distinctive family practices and socialization continue among this group.

In order to assess the extent of participation in the larger Japanese American community, data on ethnic newspaper reading, visiting with relatives, and assistance securing employment are included in table 2. Reading of Japanese American newspapers dropped from over 91% of the highly literate Issei to over 46% of the Sansei, with the second generation intermediate. No doubt the gradual geographic dispersal and linguistic acculturation of Nisei and Sansei contribute to this trend. On the other hand, regular visiting with relatives remains commonplace. Of the aged first generation, 56.9% and 71.5% and 68.6%, respectively, of the Nisei and Sansei report at least monthly visits (see also Connor 1977). The Issei's record of collective economic adaptations is broadly recognized (Light 1972; Bonacich 1975; Woodrum, Rhodes, and Feagin 1980), and the finding that over 95% of the Issei sample received help from another Japanese in securing their first jobs is consistent with the "ethnic economy" account in the literature. Despite the community economic disruptions of the internment, over one-half of the Nisei and 58.8% of the Sansei report that another Japanese American assisted them in obtaining employment. This finding affirms a continuing instrumental as opposed to strictly expressive significance of Japanese American community relations.

Racial Conflict and Subordination of Japanese Americans

An overview of Japanese Americans' relative adaptations in the economy can be derived from table 3's occupational distributions and group differentials for decades between 1910 and 1970. The first differential column, for 1910 through 1920, establishes that the Issei were overrepresented in low-paying laborer and service occupations during the years immediately following immigration. The situation changed dramatically as the immigrants became established in their new homeland and put into operation community economic self-help mechanisms, including labor pools and rotating credit associations (Light 1972). The 1930 through 1940 differential column indicates rapid occupational mobility into the managers, officials, and proprietors and the farm owners, managers, and tenants categories.

TABLE 3

PERCENTAGE DISTRIBUTIONS OF JAPANESE AMERICAN AND U.S. TOTAL OR WHITE LABOR FORCE OCCUPATIONS ADJUSTED FOR REGIONAL AND URBAN-RURAL RESIDENCE

A. 1910–40

	1910	1908–24*	1920	1910–20	1930	1932–41	1940	1930–40
OCCUPATION	Total Labor Force	Japanese Americans	Total Labor Force	Differential	Total Labor Force	Japanese Americans	Employed Males, Adjusted	Differential
Professionals	5.4	1.5	6.2	−4.3	7.4	4.1	6.4	−2.8
Managers, officials, and proprietors	6.0	8.4	6.3	+2.2	6.8	25.4	10.9	+16.6
Farm owners, managers, and tenants	20.9	17.3	20.1	−3.2	11.2	31.4	12.8	+19.5
Clerical workers	6.2	1.6	10.1	−6.6	7.6	1.8	13.5†	−7.0
Sales workers	4.8	3.2	5.3	−1.8	7.8	5.7		
Craftsmen and foremen	8.1	1.9	8.0	−6.1	14.5	3.9	15.3	−11.0
Operatives	13.3	5.7	15.1	−8.5	11.1	3.6	17.0	−10.6
Service and household workers	9.7	17.7	8.7	+8.5	7.0	5.6	7.2	−1.5
Farm laborers and foremen	18.0	29.8	11.7	+15.0	15.9	10.2	8.1	−1.8
Laborers	7.6	12.9	8.5	+4.8	10.7	8.3	8.8	−1.4
Totals	100.0	100.0	100.0	0.0	100.0	100.0	100.0	0.0

SOURCES.—Japanese American Research Project; U.S. Bureau of the Census 1940*a*, pp. 20–21, 59–62; 1940*c*, pp. 81–83, 97; 1950*a*, pp. 101, 397–402; 1950*c*, p. 37; 1960*a*, pp. 217–18; 1960*b*, p. 108; 1970*a*, pp. 392–93; 1970*b*, p. 31.

* Japanese American occupations from 1910 to 1941 are calculated from Japanese American Research Project data on household heads for the periods indicated.

† The 1940 and 1950 U.S. census data from which these figures derive combine clerical and sales workers.

TABLE 3 (*Continued*)

B. 1950–70

Occupation	1950			1960			1970		
	Japanese Americans	Whites, Adjusted	Differential	Japanese Americans	Whites, Adjusted	Differential	Japanese Americans	Whites, Adjusted	Differential
Professionals	6.8	9.8	−3.0	14.2	13.1	+.8	18.9	16.5	+2.4
Managers, officials, and proprietors	7.1	9.9	−2.8	7.7	10.1	−2.4	8.2	9.4	−1.2
Farm owners, managers, and tenants	11.1	6.6	+4.5	7.9	3.1	+4.8	2.0	2.0	0.0
Clerical workers	15.5	20.7	−5.2†	16.7	16.4	+.4	20.2	19.0	+1.2
Sales workers				6.1	8.3	−2.2	6.5	8.0	−1.5
Craftsmen and foremen	5.5	14.6	−9.1	12.9	14.9	−1.9	11.7	13.7	−2.0
Operatives	13.3	18.2	−4.9	13.5	17.5	−3.9	11.7	14.9	−3.2
Service and household workers	15.3	9.9	+5.4	11.7	9.9	+1.8	12.9	11.6	+1.3
Farm laborers and foremen	16.3	4.2	+12.1	5.3	2.2	+3.1	1.9	1.0	+.9
Laborers	9.1	6.1	+3.0	4.0	4.5	−.5	6.0	3.9	+2.1
Totals	100.0	100.0	0.0	100.0	100.0	0.0	100.0	100.0	0.0

Sources.—Japanese American Research Project; U.S. Bureau of the Census 1940*a*, pp. 20–21, 59–62; 1940*c*, pp. 81–83, 97; 1950*a*, pp. 101, 397–402; 1950*c*, p. 37; 1960*a*, pp. 217–18; 1960*b*, p. 108; 1970*a*, pp. 392–93; 1970*b*, p. 31.

* Japanese American occupations from 1910 to 1941 are calculated from Japanese American Research Project data on household heads for the periods indicated.

† The 1940 and 1950 U.S. census data from which these figures derive combine clerical and sales workers.

Japanese Americans were overrepresented in those occupations by 16.6% and 19.5%, respectively. The 1950 differential column in table 3 records lingering occupational effects of the World War II internment. Japanese Americans remained underrepresented in professional, clerical, sales, craft, and operative jobs as well as experiencing a profound occupational depression in other elevated categories. The large percentage of Japanese Americans previously employed as managers, officials, and proprietors dropped from 25.4% to 7.1%, 2.8% below the host group level. The farm owners, managers, and tenants category dropped from its prewar high of 31.4% to a low of 11.1%. Since the 1950s, as highly educated second- and third-generation Japanese Americans replace Issei workers, the minority has become relatively overrepresented in professional and clerical occupations. This increasingly urban population now farms approximately as much as the white labor force. Japanese Americans remain slightly underrepresented in sales, craft, and operative jobs and overrepresented in private and household service and laborer positions.

An alternative way to summarize table 3 focuses on the gross percentage of Japanese Americans who must change occupational categories in order for their distribution to match the host group's. This percentage is 30.5% for 1910 through 1920. By 1930 through 1940, a time of rapid Japanese American occupational mobility, the gross differential grew to 36.1%. During the postinternment period preceding 1950, the differential dropped to 25%. Finally, with the emergence of a predominantly Nisei and Sansei work force, the 1960 and 1970 gross differentials fell to 10.9% and 7.9%, respectively. The overall trend is toward increasing similarity between Japanese American and majority group occupational distributions across the census bureau's categories. Within these broad categories Japanese and other Asian Americans are underrepresented in some occupations such as upper-level management, communications, construction, and entertainment (Lem 1976).

Building on inordinately high educational achievements, white-collar Japanese Americans are moving increasingly into middle-class economic positions. The fact that their median family income is now above that of U.S. whites has been cited, especially in the popular press, as evidence of an "assimilation success story." But this fact, when interpreted without consideration of several other factors, is subject to distortion. Japanese Americans are concentrated in urban areas of Hawaii, California, Washington, New York, and Illinois, where the wage structure and cost of living are far above the national average. The proportion of Japanese American families with multiple wage earners is higher than among all U.S. families, 60% versus 51%. And to return to an earlier point, the highly educated Japanese Americans are qualified for better-paying occupations than whites. All of these complications indicate that, to assess

the relative occupational and income experiences of the minority, more sensitive indicators than simple averages are required (Gee 1976, pp. 11–12). One methodological approach to this problem compares ratio indicators calculated from two or more variables.

In table 4, ratio social equality indicators for Japanese Americans, whites, and group differentials are displayed by decades from 1950 to 1970. These findings indicate that members of this racial minority today receive lower "rates of return" for their educational and occupational achievements in the labor market. Japanese Americans were overrepresented relative to whites in professional and technical jobs and underrepresented in managerial and official jobs for their educations in 1950 and 1960. By 1970 they were significantly underrepresented in both high-status occupational categories in view of their education. Income returns on advanced education and income returns for professional and managerial work have consistently been lower for Japanese than for white Americans, as the table indicates. Lower rates of return on education and employment are not necessarily strictly the result of direct discrimination; cultural factors influencing career choices complicate interpretation of these data. Nevertheless, the above time series suggests that the minority's occupational disadvantage is worsening and income disadvantages are persisting despite fluctuations. An irony substantiated by these findings is that precisely those college-educated, professional Japanese Americans celebrated as exemplifying an "assimilation success story" systematically receive less prestigious, authoritative employment and less financial compensation than similarly qualified whites.

A confirmation and extension of this pattern of group income disadvantage through 1975 is reported by the U.S. Commission on Civil Rights (1978, pp. 53–56). Their regression analysis of mean earnings adjusts for occupational prestige, age, education, annual weeks worked, hours worked the previous week, and average income in the state of residence. With these adjustments, Japanese American males earned 88% and females 58% of that of white males in 1975. Comparable figures for black males and females are 81% and 61%, respectively, not greatly different from those for Japanese Americans.

SUMMARY AND CONCLUSIONS

This study began by asking whether the experiences of Japanese Americans more closely approximate those of subordinated racial minorities or voluntary European immigrants to this country. That question was operationalized through assessments of the group's assimilation, continued pluralism, and subjection to inequality. The findings provide some evidence for both adaptive patterns, indicating that no narrow theoretical construct

TABLE 4

SOCIAL EQUALITY RATIO INDICATORS FOR JAPANESE AMERICANS AND U.S. WHITES AND DIFFERENTIALS, 1950–70

RATIO INDICATOR	1950			1960			1970		
	Japanese Americans	Whites	Differential	Japanese Americans	Whites	Differential	Japanese Americans	Whites	Differential
Occupational returns on education:									
Professionals (4+ years college).....	.708	.561	+.147	.997	.945	+.052	.890	.955	−.065
Managers, officials (4+ years college)	.737	.810	−.073	.543	.727	−.184	.386	.556	−.170
Professional, managers (4+ years college).....	1.445	1.371	+.074	1.540	1.672	−.132	1.276	1.511	−.235
Income returns on education:									
Median income (median years education).....	150.7	201.3	−50.6	263.8	276.4	−12.6	442.5	559.7	−117.2
Income returns on occupation:									
Income $10,000+ (professionals, managers)*.....	.388	.500	−.112	.292	.408	−.116	.941	1.007	−.066

SOURCES.—U.S. Bureau of the Census 1950*a*, pp. 96, 298, 397–402; 1950*b*, pp. 88-90; 1950*c*, pp. 18, 37; 1960*a*, pp. 207, 217–18, 230, 244, 406; 1960*b*, pp. 16, 55, 108; 1970*a*, pp. 368, 379, 392–93, 628, 833; 1970*b*, pp. 9, 13, 17, 31.

* The 1950 income returns on occupation ratios are calculated for persons earning $5,000+ per year.



is sufficient to understand fully their experiences. Still, by specifying aspects of this group's adaptation, the data above justify characterizing those particular aspects in terms of the original research question.

Assimilation trends in English mastery, residential dispersion, primary group relations, exogamy and attitudes toward exogamy, educational mobility, and mobility across broad occupational categories are evidenced by JARP and U.S. census data. These trends are continuing as the third generation reaches adulthood, but neither my findings nor those of other researchers (Kitano and Kikumura 1976; Connor 1977) indicate that Japanese Americans are losing all cultural distinctiveness and identification. Religious traditionalism and a growth of religious nonaffiliation are indicated, as well as the persistence of distinctive value systems, family interaction patterns, patronage of an ethnic press (recently reinvigorated with Asian American movement publications), intraethic economic aid, and interest in Japanese culture. Continued concentration on the West Coast, increasingly in urban areas, coupled with substantial past and continuing migrations from other Asian countries reinforces ethnic identity.

The median educational, occupational, and income earning achievements of this group are above those of other American racial minorities. To this extent their increasingly middle-class experiences approximate those of culturally plural European immigrants. In spite of the utility of the immigrant assimilation and pluralism models, my findings and others' underscore the limits of those theoretical constructs for analyzing Japanese Americans. Legislated and extralegal discrimination characterize their history more systematically than that of European immigrants. Their mobility was won at high cost and despite bitter opposition. Even today the most educated and professional class of Japanese Americans receives less compensation than white Americans with equivalent credentials or employment. These considerations confirm parallels between the experience of Japanese Americans and that of other racial minority groups.

The anomalous case of Japanese Americans documents the flexibility and resilience of patterns of race relations in the United States. A nonwhite people can achieve rapid mobility when the institutional context of their entry facilitates it. Nonetheless, minority status is transmitted to their descendants, and racial disadvantage persists, even in the middle and higher strata.

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Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

MEDIA DIFFUSION OF SUBCULTURAL ELEMENTS: COMMENT ON FINE AND KLEINMAN

In a recent article, Fine and Kleinman ("Rethinking Subculture: An Interactionist Analysis," *AJS* 85 [July 1979]: 1–20) review several problems in conceptualizations of subculture and develop a reformulation based on interactionist principles. Proposing that subculture be viewed as synonymous with group culture, they address the question of "how cultural material becomes spread throughout a subsociety in which most members are not in effective interaction" (p. 8). Or, if cultural items are products of face-to-face contacts (Blumer 1969), how can subcultures (or group cultures) whose limits transcend the interactional boundaries of small groups be explained? The authors' answer is that cultural elements are diffused through four types of communication interlocks: (1) overlapping memberships in two or more groups, (2) acquaintance relationships or "weak ties" between members of different groups, (3) structural roles (such as guest lecturer and traveling minister) that involve regular intergroup contacts, and (4) mass media (pp. 10–12).

I agree with Fine and Kleinman's interactionist approach to the study of subcultures but believe their analysis of the role of the media in subcultural diffusion is underdeveloped and produces as much confusion as clarification. Specifically, (1) they fail to distinguish media-transmitted cultural items according to origin within groups of the population segment

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(blacks, women, or youth) which serves as the subcultural referent or in groups of other subsocieties; and (2) they consider only mass media whose intended audience comprises several population segments and sometimes the national society, thus ignoring special-audience media (the black press) that are mostly directed at members of a single subsociety. As a result, Fine and Kleinman exaggerate the degree to which subsocieties denied access to the mass media are dependent on interpersonal communications for the spread of cultural material (p. 17) and ignore the increasingly important role of special-audience media in the expansion of subcultures.

They do discuss media diffusion of "idiocultural" elements (Fine 1979), the products of small-group interaction, throughout a subsociety, but most of the examples they provide (the influence of the movie *The Bad News Bears* on Little Leaguers, the impact of James Dean films on youth of the 1950s, and others) do not illustrate this process. Instead, they are cases in which groups adopt cultural items produced outside their subsociety. This type of diffusion is of obvious importance in shaping subcultural content, but it does not lead to an understanding of how idiocultural material is transmitted to other groups within the same population segment.

Although Fine and Kleinman's examples are ambiguous at best, the mass media do convey cultural innovations between groups in the same subsociety, the attention given to the fads and leisure activities of youth being an often-cited case (Brecher 1972; Irwin 1977). But the importance of this type of diffusion depends on access to mass media, and it is well documented (and recognized by Fine and Kleinman) that many groups, including blacks (Johnson, Sears, and McConahay 1971), Chicanos (Lewels 1974), women (Freeman 1975), and gays (Corzine 1977), have traditionally been excluded or portrayed through negative stereotypes. Mass media exposure for many of these groups has increased in recent years, but there is ample evidence that selective and biased coverage continues (Fedler 1973).

A group's lack of access to control of mass media presentations does not necessarily result, as Fine and Kleinman state (p. 17), in its being almost totally dependent on interpersonal communication channels for transmitting cultural material. Unfortunately, they ignore groups that publish their own periodicals or, less frequently, operate their own radio stations. With the rapid decline of the ethnic foreign-language press after World War II, the oldest of these special-audience media still flourishing is the black press, with an estimated circulation in excess of 3 million (Pember 1977). More recently, the politicization of wide segments of the U.S. population during the 1960s and the increased availability of cheap offset printing have spurred the development of significant alternative presses among Chicanos, women, gays, and countercultural youth (the so-called underground press [Glessing 1970]).

It would be impossible to summarize the extensive literature on special-audience media in the present context, but, to underline their relevance for the study of subcultures, I will briefly review studies of their effects on the centrality and salience of subcultural identifications (topics discussed by Fine and Kleinman, pp. 12–14).

As Klapp (1972) notes, a communications system that provides positive feedback about group members is essential for the transformation of atomized individuals into a self-identified collectivity or, in other words, the creation of a "we-feeling." By disseminating positive role models and information about the ongoing activities of scattered groups, special-audience media can establish the necessary conditions for an extension of identity beyond local referents. Singer (1973) and others have recognized the role of the black press in developing a strong racial consciousness that replaced identification with neighborhoods among U.S. blacks. The recent proliferation of gay periodicals in the United States (Corzine 1977; Humphreys 1979) has facilitated a similar change of identity from "homosexual" (based on sexual orientation, an individual characteristic) to "gay" (based on communal allegiance and involvement) among that segment of the population with a same-sex preference.

Breton's (1964) study of European ethnic communities in Montreal suggests strongly that special-audience media increase the salience as well as the centrality of subcultural identifications. The presence of one or more periodicals within an ethnic community was strongly and directly associated with the percentage of social affiliations members maintained within their own group. Recent studies of the gay subculture by Corzine (1977), Harry and DeVall (1978), and Humphreys (1979) offer some insight into the process through which special-audience publications may increase the salience of subcultural identities. The traditional gay world in the United States consisted of friendship networks and a few institutions, mostly bars, concentrated in large urban areas and linked together through informal communications (Leznoff and Westley 1956). The gay press, by providing a means of contact for individuals with similar interests and tastes besides sexual preference and an advertising outlet for businesses and organizations seeking to develop a gay clientele or constituency, has stimulated the formation of new types of institutions, including political and social clubs, churches, health and welfare organizations, and sports leagues. This increasing "institutional completeness" (Breton 1964) has enabled gays to maintain many of their nonsexual associations within their subculture, which provides a less stigmatizing environment that, for many, is preferable to that of the larger society.

As a final note, my purpose in this comment is not to question the usefulness of Fine and Kleinman's major arguments. Their proposal for re-

formulating the concept of subculture is persuasive and offers substantial promise of resolving at least some of the difficulties that have plagued past research in the area. By extending their analysis of the media's role in subcultural diffusion, I hope to make a modest contribution toward the achievement of the same goal.

JAY CORZINE

University of Nebraska at Lincoln

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MASS AND SPECIALIZED MEDIA: REPLY TO CORZINE

Corzine has provided a useful extension of our analysis of how the media affect subcultural diffusion and identification. He points out, correctly, that members of some subsocieties use specialized media by which cultural

information spreads among distant groups. We want to emphasize, however, that the existence, extent, and form of these media within any subsociety are empirical questions (as is also true of the three other cultural interlocks we suggested), not theoretical assumptions. Thus, while specialized media play an important role in the contemporary homosexual subsociety (e.g., Corzine 1977), we rarely find them in preadolescent subsocieties. Also, as we pointed out (p. 6), subsocieties are not static, and the influence of the media (either specialized or mass) may change over time. Corzine cites subsocieties for which the number of specialized media have increased, such as gays (compare Corzine [1977] with Leznoff and Westley [1956]). Some subsocieties, however, have experienced a decrease in specialized media, such as the black press (Singer 1973; Roshco 1967) and the underground press of the "youth culture" of the 1960s. We do not wish to examine here the decline of these media but to point out that specialized media shape and are shaped by the subsociety.

Before extending Corzine's argument, we will address two points of confusion in his comment. First, we do not, as he suggests, treat subculture and group culture synonymously (as Spector [1973], e.g., has done). Instead we argue that subcultures are grounded in group cultures or "idiocultures" (Fine 1979). In our discussion, group culture refers to the cultural elements of an interacting group. Some of these cultural elements spread to other groups through a variety of channels or "interlocks," and only these elements constitute the subculture. In sum, subcultural material is a subset of cultural elements which have at one time or another characterized interacting groups within a specific subsociety.

Second, Corzine implies that such a film as *The Bad News Bears* is entirely a product of people (adults) outside the subsociety (preadolescents). However, the producers of the film stated that much of the material was derived from observation of groups in that subsociety. Therefore, while adults "created" and sponsored the film, the cultural material they presented reflected preadolescent life; hence the film was credible to members of that subsociety. Most productions which subsocietal members find appealing have been connected in some way with their lives or interests (e.g., science fiction films scripted by science fiction buffs). Therefore, while agencies outside the subsociety sponsor media productions, they often use subcultural material.

We shall now extend Corzine's argument by describing differences between the mass media and specialized media which may have consequences for subcultures. We differentiate media which subsocietal members do not "produce" but for which they are one prospective audience from media for which subsocietal members are both sponsor and main prospective audience. For example, television is a mass medium for the black com-

munity, and the black press is a specialized medium for that subsociety. (We assume that several subsocieties exist for members of this demographic category, such as a black bourgeoisie, a black feminist subsociety, a black subculture of delinquency, etc.) We do not include television as a specialized medium for the liberal upper middle class because, while members of this subsociety create the productions (Stein 1979), they intend them for members of other subsocieties as well.

First, we argue that the mass media and specialized media are likely to present different images (and subcultural material) of a particular subsociety to their audiences and that specialized media are more likely to present a picture consistent with views of the members of their subsociety. The mass media are supposed to appeal to "the masses" and, because of this, the media must present a picture which a majority of members of the audience will accept. Hence, as Tuchman (1978, pp. 139–41) suggests in her examination of the mass media treatment of the women's movement, the mass media must rely on discovered spokespersons to represent the subcultural movement. The mass media's depiction of a subculture, then, is typically an outsider's product, a "translation" of insiders' perspectives, presented in a way that "makes sense" to people outside the subsociety (though grounded in subcultural material). Audiences for specialized media outlets (here we restrict our discussion to newspapers and magazines), on the other hand, will be relatively small and probably share cultural images and a specialized universe of discourse. Because of this homogeneity, specialized media audiences are more likely to control and participate in the productions than the mass media audience or any segment of the mass (such as the subsociety). We expect specialized media to provide for more communication among members of their audiences than do mass media. For example, the former tend to include more space for communications from readers and receive and print more unsolicited manuscripts. If it has specialized media, then, a subsociety is likely to find in them images of itself which differ from those in the mass media: the latter are based on outsiders' accounts and intended for outsiders, but the former are created by and for insiders. If one assumes that something is always lost in translation, one would expect the specialized media to portray their audiences more accurately.

Second, we argue that the specialized media are more likely to serve as an interactional interlock among groups than are the mass media. Some media (whether specialized or mass) are more interactional than others. For example, newspapers allow the public to respond through letters to the editor, while television has no equivalent. The content of the *specialized* interactional media may be similar to messages members of the subsociety transmit through face-to-face communication, since the printed informa-

tion is designed for the same audience. For example, Corzine and Kirby's (1977, pp. 179, 190–91) examination of homosexual encounters with truck drivers indicates that respondents transmitted the same information through personal and specialized print media. Subsocieties are not able to use the mass media in this way.

Also, specialized media are more likely to have larger and more active forums for information exchange (forums for interaction) than the mass media. Shortly after our article was published, the first author found an extensive interactional specialized media system operating in the fantasy role–playing subsociety. We do not have the space to describe the ethnographic setting here, but it can be found elsewhere (see Moramarco 1980; Freeman 1978; Weathers with Foote 1979; Fine, forthcoming). While this specialized urban leisure subsociety (Irwin 1977) probably has not more than several hundred thousand participants, they do have specialized media. The games have rule books (which should make gaming groups around the country similar), but because of the complexity of the rules, local variants rapidly developed. "These local variations produced problems in communication among groups: . . . with the Rules' vagueness and fans' inventiveness, if this [diversity] went on we would soon be unable to play in a non-local friend's game without succumbing to culture shock" (Gold 1979, p. 21). To resolve this problem, Lee Gold organized and edited the first major gaming magazine for this subsociety. That and other fantasy gaming magazines (and publications for other subsocieties) serve as information clearinghouses to which many members of the subsociety can contribute information and from which they can receive the comments of fellow members. About 50 magazines serve the rather small fantasy gaming public. Most of these informal magazines (or fanzines) include contributions from gamers around the nation who are part of the editor's network. Gamers usually write long, open letters, commenting on topics raised in previous issues and contributing personal gaming innovations. Thus, while the specialized media gamers come from many different groups, they nevertheless are able to share information and advice. Similarly, "Commentary and Debate" sections of academic journals serve as a forum for the exchange of ideas. Because of the greater homogeneity within audiences of specialized journals, we would expect such sections to be larger in them than in more general journals. In other words, because of interest contours, media serving an audience who share a set of problems and a universe of discourse can permit more intensive discussions on a single topic than media serving a more heterogeneous audience.

Specialized media, then, are more likely to be interactional than the mass media in that they allow greater participation by audience members. Specialized media do more than provide material which groups use to meet

their needs (see Fine 1977) or transmit to other groups in their subsociety through the other interlocks we suggested. By constituting a channel through which subsocietal members can communicate, the specialized media permit communication *among* members as well as *to* them.

GARY ALAN FINE

University of Minnesota

SHERRYL KLEINMAN

University of North Carolina, Chapel Hill

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ON THE NEOFUNCTIONALIST THEORY OF INEQUALITY: A COMMENT ON CULLEN AND NOVICK

The recent article by Cullen and Novick ("The Davis-Moore Theory of Stratification: A Further Examination and Extension," *AJS* 84 [May 1979]: 1424–37) is an enterprising attempt to evaluate empirically a

theory that has produced little more than polemic and attempts at ideological aggrandizement. There are, however, serious problems of method and interpretation that undermine the usefulness of the article as a basis for further work.

The major problem centers on the causal priority of the key term—"functional importance"—and the validity of the measures used to relate importance to rewards. Cullen and Novick employ a scale of "perceived importance" derived from an attitudinal survey to demonstrate a correlation of importance with the reward of prestige. They provide no basis, however, for concluding that these popular perceptions of functional importance are temporally antecedent to the popular perceptions of prestige. Shifting from situs to status as the unit of observation offers no comfort to those who might argue that perceptions of prestige are the source of perceptions of functional importance or, better still, that ***both*** sets of patterned attitudes are the product of institutionalized power relations (see Parkin 1971, pp. 40–44). Power, of course, means the capacity of actors to realize their interests against the interests of other actors (Lukes 1975).

In fact, the work by Goldthorpe and Hope (1972, 1974) on the validity of occupational prestige scores indicates that it is incorrect on the part of Cullen and Novick to use these scores as an index of the reward of deference and that, ultimately, power may be the factor behind this ranking. Respondents to prestige items apparently reply in terms of abstract views about the general goodness of occupations and their general desirability in terms of the economic rewards attached to them. The validity problem lies in the fact that the prestige scores do ***not*** reflect a consensual normative order guiding the regularized enactment of deference in the course of everyday life, a conception central to the functionalist theory (see Shils [1968] 1975). Moreover, insofar as the actual distribution of economic rewards is the main factor behind the ranking, it could be argued that power is the source of popular attitudes on prestige and the association with perceptions of functional importance.

The only effective method of resolving this dispute is to examine the sequence of historical changes involved in the assignment of prestige and other rewards to occupations. Cullen and Novick hold this option to the side, asserting that "in the short run, a functional stratification theory does not necessarily require that positional rewards be determined by true functional importance. . . . Time and survival answer the analytically separate question of whether the perceptions of importance accurately assess true importance" (p. 1425). They hold correctly that true importance cannot be assessed synchronically. Nor can the effects of power. A diachronic assessment is necessary to establish temporal order. If we draw on the historical evidence presented by Berlant (1975) and Larson (1977) with regard to the placement of the medical profession in its present exalted

position, the conclusion is that a combination of corporate organization, professional monopoly based on state licensing and formal education, and the institutionalization of an ideology of public service created occupational control over material rewards and popular acquiescence to the type of medical services being offered. The practice of medicine is, of course, only one occupation, but if we accept parallel cases (see Bibb and Form's [1977] analysis of blue-collar occupations; Collins's [1979] analysis of occupational credentialing at large), the main dynamic of occupational change appears to have been the acquisition of market power and the subsequent institutionalization of legitimating ideologies, not performances that have met the test of time and survival. The synchronic analyses of prestige, importance, and economic rewards, then, can be interpreted as tapping this historical conquest of market power.

A final problem pertains to the sample. Because of the lack of precise matches among the occupational titles in the various data sets, Cullen and Novick are compelled to drop 45% of the individuals from the sample. The problem is that the remaining 55% appears to be biased, as their appendix (p. 1436) shows. Only 35% of managers and administrators are retained; 38% of sales workers; 17% of operatives; 17% of nonfarm laborers; yet between 63% and 85% of professional-technical personnel, clerical and service workers, and transport equipment operators are used. The sample differentially excludes individuals in the categories with the highest rewards (managerial-administrative) and the lowest (nonfarm laborer) and disproportionately retains those in medium-level positions. Talent and training might make a difference within this attenuated sample and yet be relatively unimportant once the full range of powerful and powerless occupations is included.

J. Craig Jenkins

University of Missouri at Columbia

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TESTING FUNCTIONAL THEORY WITH OCCUPATIONAL POSITIONS: A REPLY TO JENKINS

Jenkins questions several aspects of our recent test of the Davis and Moore (1945) functional theory of stratification: (1) the causal location of functional importance; (2) the lack of occupational power as predictor of rewards; (3) the use of prestige as a measure of reward; (4) the validity of a measure of perceived functional importance; (5) the nature of our occupational sample; and (6) the problem of a cross-sectional test of Davis and Moore's theory. To some degree, the first three criticisms intermix problems inherent in the Davis and Moore theory with problems related to our specific test using occupational positions. The remaining criticisms raise legitimate issues concerning our study, but, contrary to Jenkins's suggestion, these weaknesses encourage rather than discourage future research.

The assumptions that functional importance is causally antecedent to positional rewards and that prestige is a possible positional reward are elements of functional theory proposed by Davis and Moore and not by Cullen and Novick. Our "normal" science test of the theory necessarily remained faithful to those assumptions—which, nevertheless, might be tested longitudinally.

Jenkins does argue plausibly that both prestige and perceptions of functional importance might be determined by aspects of occupational power. Also, he states that "the actual distribution of economic rewards is the main factor behind the ranking." Again, his disagreement seems to be with Davis and Moore, who recognize but then discount the possibility that political power might be a major basis of stratification. Moreover, Davis and Moore (1945, pp. 246–47) argue "that a position does not bring power and prestige *because* it draws a high income. Rather, it draws a high income because it is functionally important and the available personnel is for one reason or another scarce."

Referencing several studies of professionalism and the credentialing processes, Jenkins argues that it is not contribution to system survival but control of the market and success at institutionalizing legitimating ideologies that lead to occupational group status achievement. However, our extension of the Davis and Moore theory through a causal model of the variables and our use of a perceived measure of functional importance actually bring us closer to Jenkins's position than to that of Davis and Moore. Like Jenkins, we assumed that prestige could be affected by income. Moreover, our model explicitly took into account the fact that *perceptions* of functional importance (which might be part of legitimating ideologies) can be affected by a range of factors such as length of education and the physical demands of an occupation. Since education can

be lengthened artificially (possibly affecting labor supply as well as perceptions of importance), and manual labor can be assigned to subordinates, our model accepts, at least implicitly, the potential for conscious efforts by occupational groups to manipulate rewards. In fact, evidence from my data, based on a model including additional variables measuring the extent of licensure and the nature of occupational organization, suggested that organizational strength and credentialing do have small effects on education and lead in turn to perceptions of greater functional importance and to larger occupational rewards (Cullen 1978). However, the 1978 study derived its variables from theories of professionalism, while the 1979 study used only those variables that seemed consistent with Davis and Moore's propositions.

As we noted in our paper (p. 1425), the concept of functional importance has caused much prior debate. We took the commonly agreed on position that functional importance represented the demand side of the model. Hence, we remain convinced that functional importance need only exist as a *desire* for an occupational service. However, just as for any other product or service, we suspect that the demand for an occupational service may be affected by factors such as advertisements or good public relations.

Jenkins points out correctly that evaluations of occupational titles in terms of prestige or importance tend to be highly correlated. However, it must be noted that our measure of functional importance was computed for Morris and Murphy's (1959) occupational situses (e.g., finance and records) and not for occupational titles. All occupations within one situs received the same score on importance. The prestige rankings of specific occupational titles varied widely within situses. However, the correlation between perceived functional importance and prestige was a moderate .54.

Jenkins suggests that a broader range of occupations might affect the study's major conclusions. Although this point cannot be refuted directly, a recent study by Spaeth (1979), using data based on the complete range of 1960 census titles, reported similar correlations among education, two measures of task complexity, income, and prestige, even though the income and prestige measures were different from those in our study.

In sum, I think Jenkins makes some legitimate criticisms of the Davis and Moore theory and identifies problems associated with an occupational test of the theory. Although our study suffers from problems typically associated with cross-sectional research, I see no reason why it cannot serve as a basis for future research on occupational positions which could both be longitudinal and include new variables measuring power. Some key variables have already been examined longitudinally (Spenner 1979), and some measures of occupational power have already been constructed (Cullen 1978).

JOHN B. CULLEN

University of Nebraska at Lincoln

Review Essay: A Scholar's Monument

Qualitative and Quantitative Social Research: Papers in Honor of Paul F. Lazarsfeld. Edited by Robert K. Merton, James S. Coleman, and Peter H. Rossi. New York: Free Press, 1979. Pp. xviii+413. $17.95.

Herbert L. Costner
University of Washington

Pharaohs are symbolically immortalized by the pyramids, princes of the Church by altar-crypts in great cathedrals, kings by grand monuments, musicians and poets by busts, university administrators by portraits in administrative halls, and generals by equestrian statues. For each order or class of the great among us there is a special and fitting way of symbolizing their greatness for posterity. For scholars and scientists, the prevailing mode is the memorial volume, a collection of papers by colleagues and students whose work has been influenced by the one they honor.

Memorial volumes are not like books of other kinds. The contributors are selected because of their relationship to the central figure and not because their papers will illuminate a special topic or research area. Memories of years past are permissibly intermixed with the scholarly analysis of current issues, but all of the pages cannot be devoted to reminiscing and praise; there must be scholarly content designed to illustrate the fertility of the master's work and the continuity between it and the frontiers of current knowledge. But the genre is not so completely ritualized that all of its requirements are clear. It is not certain, for example, whether the scholarly papers in a memorial volume are really expected to break new ground or whether it suffices simply to show that persons of high scholarly accomplishment trace their intellectual roots to the master.

Qualitative and Quantitative Social Research is a collection of papers written to honor Paul Lazarsfeld by a few of those whose lives were enriched by him. The editors, Robert Merton, James Coleman, and Peter Rossi, have assembled papers by a distinguished set of authors. The 26 contributors include one Nobel Prize winner, one Pulitzer Prize winner, seven members of the National Academy of Sciences, three past presidents of the American Sociological Association, and others whose scholarly achievements have been honorifically recognized by awards, offices, and appointments around the world. Some of the papers focus on Paul Lazarsfeld as a person—"his work-style and thought-style," as the editors describe the first grouping of papers. Others present methodological and substantive contributions that are explicitly attributed to Lazarsfeld's influence or clearly tied to his work. Some authors present an aspect of their own current work with no attempt to point out the tie to Lazarsfeld, although it is always possible

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to see some connection. The pervasive theme is homage to Lazarsfeld—as scholar, teacher, person, and inspiration.

For those of us who were not fortunate enough to know Paul Lazarsfeld well as a person, the papers focusing on the man himself provide fascinating insights about a remarkable life. Marie Jahoda, Lazarsfeld's collaborator even in his Vienna days, insists that his identity as a methodologist was as much pose as reality, because he was actually so much more. Her remarks do not center on recollections of the past so much as they explore the meaning of his career. Hans Zeisel, another early collaborator, reminisces; he presents an engaging picture of Lazarsfeld in the lively and wide-ranging intellectual life of Vienna in the 1920s and of his role as the leading figure in an intensely intellectual group of young socialists. Robert Merton, Lazarsfeld's colleague at Columbia for a third of a century, recalls the discomfort Lazarsfeld felt (or was it feigned as a ruse for stimulating discussion?) about his ascribed identity as a sociologist, and his pleasure at being named Quetelet Professor of Social Science, which thus "coupled Paul's prime conception of himself as a maverick social scientist with the name of one of his few culture heroes" (p. 21). Other papers, by Charles Glock and Ann Pasannela, candidly recall the many facets and frequent revisions of Lazarsfeld's ideas about centers for collective social research and the utilization of social research findings. But the real gem of this set is a marvelously vivid portrait written by Bernard Bailyn, the Pulitzer Prize–winning historian who married Lazarsfeld's daughter. Bailyn begins by describing his father-in-law's visits: "A visit by Paul was like some wonderfully benign hurricane" (p. 16). He ends by imagining Lazarsfeld enjoying heaven in a uniquely Lazarsfeldian way, and those who know even a little of Lazarsfeld's character cannot fail to read this succinct bit of fantasy without an immediate sense of recognition.

The first set of papers attests to Lazarsfeld's charisma, but the memorial volume genre requires more. Such a volume, to be true to the established pattern, must demonstrate Lazarsfeld's influence on current work. This volume is, indeed, true to that pattern. But more than simply seeking to illustrate his scholarly influence, the editors seem also to have been intent on demonstrating that the widely held perception of Lazarsfeld as "merely" a quantitative methodologist was a fragmentary perception of the man—Marie Jahoda's thesis in the first paper. I certainly do not intend to challenge that thesis; it is, I believe, irrefutable. But I do wish to call attention to the nature of the grouping the editors have imposed on the papers —presumably to highlight Lazarsfeld's varied interests and achievements—and what that grouping suggests about current conceptions of the distinction between method and substance in sociology.

Raymond Boudon's paper would certainly *seem* from the title, "Generating Models as a Research Strategy," to be a paper on method (or perhaps on "strategy"), and since it has unmistakable quantitative aspects, it might seem that the editors have quite appropriately classified it under the general heading "Quantitative Methods and Reasoning" as opposed to "Qualitative Methods and Reasoning" or "Substantive Social Research." But the

actual content of Boudon's paper suggests otherwise. Boudon presents a conception (Lazarsfeld might have called it a "story-line"; Boudon calls it a "model") of choice behavior in a "lottery," which he then proceeds to use in understanding certain features of data on promotions (from *The American Soldier*) and on enrollment in institutions of higher education in contemporary France, with allusions also to the application of the same general model to some of the observations of Tocqueville and Durkheim and to current data on social inequality. Why is this not more appropriately classified under "Substantive Social Research"? It is true that Boudon presents his substantive model and its applications sandwiched between "methodological" (or "strategy") comments, that is, comments on the desirability of developing a "generating theory," which means a general model "that includes the observed empirical structure as one of its consequences" (p. 52). But that may simply mean that Boudon learned well the lesson of his mentor, viz., it is useful to pose as a methodologist so that one can be free to range broadly across the boundaries of substantive specialties as these are conventionally conceived. And it is true that the editors have called their general heading "Quantitative Methods and Reasoning" rather than simply "Quantitative Methods," so they are technically accurate in their classification—Boudon is engaged in quantitative reasoning.

But what purpose is served by grouping Boudon's substantive argument with, for example, papers on statistical methodology, instead of with papers labeled "substantive"? Does "substantive" mean an exploration of many aspects of a single data set, such as the analysis of the changing social origins of American academics by Lipset and Ladd, or Peter Blau's analysis of professional schools and their contexts (both of which are classified in this volume as "substantive"), as opposed to the explanation of selected aspects of many data sets, such as Boudon's application of his model? Does "substantive" mean concretely historical, such as Sigmund Diamond's paper on immigration in American history, as opposed to Boudon's model that seems to apply in many historical settings? Is the presentation of a tightly reasoned, general, explanatory argument sufficient to disqualify a paper as "substantive"? All of these meanings might be inferred from the editors' classification. Any of these meanings would have unfortunate implications for substantive sociology—unless, of course, foxy people like Lazarsfeld and Boudon continue to pose as methodologists to escape the confines of the "substantive," so conceived.

Boudon's paper is not the only one "posing" as a methodological treatise in this volume. There are also R. Duncan Luce's paper on the "choice axiom," and Anthony Oberschall's, entitled "Social Exchange and Choice." Of course, from one perspective, it does not really matter how particular papers were classified in this volume. The grouping does serve its presumed purpose of highlighting the variety of Lazarsfeld's talents and interests. But it is surprising, not to say exasperating, to find the friends of Paul Lazarsfeld reinforcing the folly of making "substantive" almost synonymous with "descriptive."

The editors might have considered the classification of "study aims" dis-

cussed in Hanan Selvin's paper in this volume as an alternative way of grouping the papers. Selvin indicates that some studies are purely descriptive, others are purely explanatory, while a third class is both descriptive and explanatory. Not surprisingly, these "types" are generated by Selvin cross-classifying two dichotomies in a classically Lazarsfeldian manner, and so there is a fourth type that Selvin calls the "nonstudy" (which would include his own paper on strategy). But one can understand why this more reasonable classification was not adopted. Not only does it fail to highlight Lazarsfeld's range over the cleavages that sociologists traditionally argue about, but it also carries a pejorative connotation for those studies that are called "purely descriptive." And one of the unwritten rules of the memorial volume genre is that the editors must not offend any of the invited authors by suggesting, however subtly, that their paper is among the lesser contributions to the honor of the master.

Although not strictly required by the established pattern of the memorial volume genre, the glory of the master may be enhanced by showing that he was not only influential but also the originator of the foundations for what is now the cutting edge of the field. In this respect, the contributors to this volume have not done Lazarsfeld full justice. There are some fine papers in this collection, and I call attention especially to Herbert Simon's paper, "The Meaning of Causal Ordering," an admirably clear exposition of problems entailed in applying the kind of causal reasoning that can be traced from Lazarsfeld to Simon and thence, with an infusion from economics and other sources, into the mainstream of contemporary sociology. But with this and a few other exceptions, the papers in this volume do not give one the kind of intellectual exhilaration one expects from the cutting edge. But, of course, even distinguished scholars may be reluctant to submit their best work to the obscurity that is the typical fate of memorial volumes. This one will probably escape obscurity longer than most—both because it includes a few papers of substantial (even if not always "substantive," in the editors' sense) scholarly merit, and because of the towering influence of the man it honors.

Review Essay: In Search of the Real Marx

Main Currents of Marxism: Its Rise, Growth, and Dissolution. By Leszek Kolakowski. Translated by P. S. Falla. 3 vols. Vol. 1: *The Founders.* Vol. 2: *The Golden Age.* Vol. 3: *The Breakdown.* New York: Oxford University Press, 1978. Pp. 480; 542; 548. $19.95 each.

Zoltán Tar
New School for Social Research

Revolt and revulsion against the brutality of Stalinist practices and its simplistic, dogmatic-theoretical cover-up in the name of Karl Marx prompted the younger generation to search for the "real" (humanist) Marx all over Eastern Europe in the 1960s. Kolakowski in Poland, Kosik in Prague, diverse groups of young intellectuals in Hungary, and the philosophers and sociologists of the *Praxis* group in Yugoslavia are different manifestations of and responses to a similar historical condition. *Main Currents of Marxism,* Leszek Kolakowski's magnum opus, cannot be fully understood and appreciated without keeping in mind the special larger political and intellectual context of its genesis.

The groundwork for this monumental study, finally completed in Oxford, was laid in Warsaw. It was engendered by the Eastern European revisionist movements of the 1960s, a thoroughly exciting period of intellectual ferment. In the late 1960s, as promising beginnings in East and West were in sight, the grand old man of Eastern European revisionism, György Lukács, was hoping for and predicting the coming of a Marx Renaissance. The prognosis came true, albeit not exactly in the sense envisaged by Lukács. In retrospect, we can attest to a modest renaissance of Marxist scholarship; but what was really flourishing was the new "Marxology," that is, the institutionalization and domestication of a vulgar Marxism, on a lower level than the so-called *Kathedermarxismus* at the turn of the century in Germany, as the academization of the North American New Left proves. One of the few original works of lasting value that came out of this significant and turbulent period is Kolakowski's *Main Currents.* This seminal work not only will outlast the revolutionary movements of the 1960s, East and West, but also will serve as a guide and inspiration for a long time to come for those really interested in Marxian thought.

The three volumes of *Main Currents,* consisting of about 1,500 pages, bear the titles *The Founders, The Golden Age,* and *The Breakdown,* which give a good first indication of Kolakowski's perspective. He is guided by two basic questions: first, What are the fundamental elements of Marx's philosophy? and second, What in Marx, or in his philosophy, is responsible for the degeneration of the socialist idea into "actually existing so-

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cialism," or into Stalinist despotism? These are indeed important and legitimate questions. Even though they had been asked by others before Kolakowski, his long-time involvement with Marxist theory and direct experience with its practice justify his asking anew, and whet our appetite for his answers.

Who was Karl Marx, and What is Marxism? are questions answered differently by different writers in our century. For Lenin, Marxism was the synthesis of German philosophy, French utopian socialism, and British political economy, plus the creative continuation thereof, and its application (praxis) to concrete historical situations. In his now classic *History and Class Consciousness,* Lukács identified totality and revolutionary dialectic as the quintessence of Marx's legacy. Somewhat later, Joseph Schumpeter cut the "Marxian doctrine" into pieces, which he discussed one by one: Marx the prophet, the sociologist, the economist, and the teacher. The philosopher Leszek Kolakowski follows his acknowledged onetime master, György Lukács, and shifts the emphasis to philosophy. "Karl Marx was a German philosopher," declares Kolakowski in the opening sentence of volume 1 of his trilogy. The tone is thus set for what is to follow.

In volume 1, *The Founders* (Marx and Engels), Kolakowski identifies three chief motives of Marxism: the romantic, the Promethean-Faustian, and the deterministic-rationalistic Enlightenment. The first represents the idea of a return to some kind of society with *Gemeinschaft*-like organic human bonds, which would enable man to regain his authenticity and develop to their maximal capacity his faculties, which are denied or perverted under capitalist-industrial civilization. Marx, Kolakowski argues, took over certain elements of romantic anticapitalist sentiments, such as the theory of alienation, the theory of the power of money, and the belief "in a future unity in which the individual would treat his own forces directly as social forces" (p. 410). However, points of agreement with romantic anticapitalism do not mean complete identity with that sentiment or the advocacy of some kind of "back-to-nature" philosophy and program. After all, it was Marx, Kolakowski states, who praised the civilizing mission of capital, and also of the bourgeoisie, which in Marx's words "has created enormous cities . . . and has thus rescued a considerable part of the population from the idiocy of rural life." The Promethean impulse is embodied in the collective subject of history, the proletariat, which will liberate mankind and lead it from "the realm of necessity to the realm of freedom." Finally, the rationalist-scientistic motive is expressed in the belief in historical determinism, in the iron laws of historical necessity for the "prehistory" of mankind, leading to the proletarian revolution in which men consciously take the direction of history into their own hands. This problem takes us to the most controversial issue in the interpretation of Marx and Marxism: Was Marx a determinist or a voluntarist? Did he believe in inevitable historical laws or in the value of human initiative? In Kolakowski's opinion, such questions have no meaning in the light of Marx's own conception of his work. But when precise political strategy was to be derived from his

social philosophy, the practical interpretations differed greatly; the Leninist-Stalinist version of socialism was only one of the possible interpretations of Marx's doctrine. A related issue would be Marx's responsibility for Gulag and for most of the evils and deficiencies of all actually existing socialist societies—an idea proposed by many, most recently by the "New Philosophers," the formerly Marxist New Left philosophers in France, in a manner reminding one of Freud's "murder of the father." Their proposition has received worldwide attention, mainly because of mass media dissemination. But what seemed to me the most innovative idea for Marxian scholarship was the brilliant chapter on the precursors of Marx, discussing in detail Marx's link with Plotinus, a seldom-mentioned theme.

Volume 2, *The Golden Age,* focuses on the theorists of the Second International and on Lenin. The former include orthodox, reformist, and left-revolutionary representatives, such as the towering figures of Karl Kautsky, Eduard Bernstein, and Rosa Luxemburg. French socialism is represented by Jean Jaurès, Paul Lafargue, and Georges Sorel, who was called a "Jansenist Marxist" because of his aesthetic-moralist tendencies. Next in line is Antonio Labriola of Italy.

All the thinkers listed receive solid, reliable coverage. Perhaps most important for any Western reader unfamiliar with Poland's language and intellectual history is the introduction of a galaxy of important Polish thinkers working in the Marxist tradition, though mostly in the revisionist or heretical one. For Ludwik Krzywicki (1859–1941), Marxism was an instrument of sociology and not an end in itself. He rejected the idea of the inevitability of socialism and believed that a future social order after capitalism "might be the work of either the proletariat or the bourgeoisie" (p. 200). He also pointed out the possibility of a "state capitalism." While Krzywicki acknowledged the dominant role of economic forces for the genesis of ideas and societal institutions, he emphasized their relative autonomy once in existence. He also criticized the idea of simplistic and unilinear progress and was skeptical of the idea of unequivocal benefits from the domination of nature, a theme picked up by Horkheimer and Adorno of the Frankfurt School in the 1940s and increasingly recognized and confirmed by ecological and economic events today. Stanislaw Brzozowski (1878–1911), who died at the age of 33, was probably the most original of the group and was the first to call attention to the many contradictory aspects of the work of Marx and Engels, even before Lukács's *History and Class Consciousness* (1923) and Marx's early writings (1932) appeared. Indeed, if the time for a new discovery or idea has come, it is usually brought forth by more than one person, as the history of natural and social sciences amply illustrates. After the turn of the century, the time seemed ripe for turning against the mechanical Marxism of the Second International that had followed in the footsteps of the late Engels. Even Max Weber's "bourgeois Marxism" may be interpreted along these lines. At one time, Brzozowski articulated his peculiar brand of Marxism with the aid of the Nietzschean *Lebensphilosophie;* then he moved toward an anthropocentric viewpoint which he called the "philosophy of labour."

The Austro-Marxists (Max Adler, Otto Bauer, Rudolf Hilferding, and Karl Renner), who present another interesting national variation on Marxian themes, are thoroughly assessed. This politically and intellectually heterogeneous group is distinguished by "certain common tendencies and particular interests," asserts Kolakowski (p. 240). Its members emphasized the links between Marx and Kant and subscribed to the idea of "ethical socialism," a term borrowed from Hermann Cohen.

Marxism in Russia, with special emphasis on Lenin, is critically dissected in this second volume; due tribute is paid to Lenin, the master figure of post–World War I events. Fortunately, Kolakowski does not abide by his opening declaration in volume 1, that he will limit himself to philosophy, but rightly includes such topics as the political economy of Lenin, Rosa Luxemburg, and Rudolf Hilferding as he had done with Marx in the first volume.

With volume 3, *The Breakdown,* Kolakowski moves into the present, closer to his contemporaries, and loses some of his cool scholarly objectivity (though not his scholarly brilliance) because of the lack of historical distance. Lukács, his onetime hero, still gets high marks and rightly so. In the midst of a flourishing "Lukács Industry"—ranging from Australia to New York City and California, in which writers are busily engaged in cutting Lukács into pieces and selling "the young Lukács," "the middle-aged Lukács," and "the old Lukács," according to the demands of the market—it is refreshing to read a scholarly appraisal of the other giant of Eastern European revisionism. This chapter represents the best short introduction to the lifework of Lukács.

Members of the Frankfurt School get less favorable marks, and again justly so. The muddiness of Adorno, especially, is brilliantly dissected by Kolakowski in the grand tradition of Enlightenment philosophy. In discussing *Negative Dialectics,* he criticizes Adorno for his "pretentious obscurity of style and the contempt that it shows for the reader" (p. 357). The sterility of the Frankfurt School thought, consisting of "*ex cathedra* statements using concepts that are nowhere explained" (p. 367), is properly noted. Kolakowski calls Adorno's magnum opus, *Negative Dialectics,* a "model of professorial bombast concealing poverty of thought" (p. 368). In his judgment, Frankfurt thought is nothing more than an "assortment of ideas borrowed uncritically from Marx, Hegel, Nietzsche, Lukács, Bergson and Bloch" (p. 367).

Kolakowski concludes his long intellectual journey with a summary of his ideas concerning the relevance of Marxism today and a presentation of his own position. Both summary and presentation are somewhat disappointing; he considers Marxism useful only as one of the sociological approaches to the study of past civilizations. He is less positive that Marxism is an explanatory system and method for understanding the present or predicting the future. As far as his own position is concerned, Kolakowski stands between democratic socialism and left liberalism.

In sum, my critical remarks are not meant to detract from the value of

a work of great scholarship and analysis, the most exciting and impressive work in intellectual history to appear in a decade—probably the only lasting accomplishment of Eastern European revisionism and recent Marx scholarship. The search for the "real Marx" will continue, but no serious scholar can ignore Kolakowski's monumental trilogy.

Book Reviews

Manufacturing Consent: Changes in the Labor Process under Monopoly Capitalism. By Michael Burawoy. Chicago: University of Chicago Press, 1979. Pp. xvii+267. $20.00.

Anthony Giddens
King's College, Cambridge

Manufacturing Consent is one of the most significant contributions to the sociology of industry in many a year. The book is based on research material the author collected while working as a machine operator in the engine division of Allied Corporation. The observations Michael Burawoy made when he began working there struck him as very similar to those made some three decades earlier by Donald Roy in his well-known articles on output restriction. Burawoy was led to look up Roy's original dissertation, the source of those articles. Extraordinarily enough, he had unknowingly been carrying out his research in the same factory that Roy had studied 30 years previously. This provided him with a chance to compare his findings with Roy's and to investigate the changes that might have occurred in the nature of work in the factory over the intervening period.

Roy's work, Burawoy argues, was closely involved with the assumptions of industrial sociology of the 1930s. Roy was concerned to show how workers limit output on the shop floor, and his writing bore the strong imprint of the views of Elton Mayo. Burawoy reverses the question that guided his predecessor's research. Instead of asking, "Why is it that the workers restrict output?" he poses the question, "Why is it that the workers work as hard as they do?" If one examines Roy's descriptions of the labor process, he suggests, one finds that they actually portray a picture of hectic and unrelenting activity. This remains true today: Burawoy was impressed with the apparently furious pace with which the workers apply themselves to their labor tasks. Indeed, he found himself drawn into the same phenomenon: "It wasn't long before I too was breaking my back," he avers, "to make the quota, to discover a new angle, and to run two jobs at once, risking life and limb for that extra piece" (p. xi). The Marxist researcher found himself, to his own disquiet, driving himself to increase Allied's profits.

Why should this be so? Burawoy rejects the homilies of orthodox industrial sociology, which presumes that workers and management share common objectives in working as hard as they possibly can. But he also questions two perspectives associated with Marxist views. One is the conception that the hegemony of capital is sustained above all by institutions

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outside the workplace—the family, schools, the media, and so on. In contrast to this standpoint, Burawoy wants to show how class relations are reproduced within the process of production. However, he also diverges from those—most notably Braverman—who, while accentuating the centrality of the labor process, tend to treat workers as passively incorporated within a division of labor organized from above, thus increasingly intensifying management control.

In the factory he studied, Burawoy tries to show, the organization of work relations on the shop floor is dominated by "making out"—a sort of competitive game workers play with the rules governing the labor task as laid down by management. The labor process is experienced by the worker as an encounter with technology and regulations that are imposed externally and impersonally. But workers do not confront these imposed conditions of labor as passive objects; they actively seek to manipulate them as players do within the context of the established rules of a game. Some workers, those who do tasks that are strategically important for the activities of the rest of those on the shop floor, are particularly important in this respect: they are frequently called on to do "hot jobs." Making out means meeting the production quota assignments: in this there is often an element of risk, and the operator will have to balance the chances of turning out scrap or breaking tools against the possibility of attaining production targets. Where operators deem that making out is unlikely or impossible, they will forgo the attempt and take things easy, because they receive their base earnings in any case. This is one form of quota restriction, "goldbricking," to which Roy refers. A second type is where a ceiling is placed on output. When Burawoy was working in the factory, this was fixed at 140%, where base earnings are 100%. Workers consider, correctly, according to the author, that turning in more than this would lead to cuts in the real rate of their earnings per job batch. But this is not really an "output restriction" because operators routinely produce more than 140%, although they never deliver more than this figure: they keep the remainder as a fund to use in operations where making out is difficult.

The dominance of making out in the culture of the shop floor has remained largely unchanged, Burawoy says, over the period from Roy's study to that of his own. Its very continuity, across times involving various sorts of technological change, demonstrates the hold that it has over the workers; and both Roy and Burawoy found themselves drawn into it almost against their will. Playing the game serves various ends: it inhibits boredom, provides challenges, and links the workers in relations with one another. These relations are not, however, ones of consensus in opposition to management. Far from it: making out both produces and helps to absorb a range of "lateral" conflicts that cut across worker-management confrontations. It may even lead to circumstances, Burawoy tries to show, where workers actively clash with management in order to defend conditions of profitability.

Among the most important changes the author detects in the factory

between 1945 and 1975 is an alteration in the focus of worker-management conflicts. In 1945, operators on the shop floor were closely supervised by time-study men—the agents of Taylorism, as it were. But these men have now been replaced by industrial engineers who are located in offices away from the shop floor and who only rarely venture onto it. More hostility is directed by workers against those who try to bust rates or otherwise go against the "rules of the game" than against the planning personnel. Whereas conflict in the former situation had a "political" dimension, in the sense that it led workers into clashes over the very nature of the industrial authority to which they were subject, the newer conflicts are both more sporadic and confined to "economic" issues. Burawoy relates these changes to the broader backdrop of the transformation from competitive to monopoly capitalism. Marx correctly anticipated the demise of "classical" capitalism but did not foresee that class struggle could not only be contained within a transformed capitalist system but could also become an essential stabilizing element in it. "Class struggle was not the gravedigger of capitalism but its savior" (p. 195).

Although I have reservations about some of Burawoy's more general arguments, *Manufacturing Consent* is an outstanding study. It is bound to stimulate controversy, but its mode of approach and conclusions conform closely to two other major pieces of work that have appeared recently. In studying men on the shop floor as active, knowledgeable individuals rather than as mere "dupes" of the wider institutional system (yet retaining an overall awareness of that wider system), Burawoy's book has affinities with Paul Willis's brilliant *Learning to Labour* (Lexington, Mass.: Lexington, 1977). The arguments that class struggle on the level of day-to-day practices on the shop floor is not given sufficient prominence by Braverman and that such struggle has opposite consequences to those projected by Marx are made in Andrew Friedman's *Industry and Labour* (Atlantic Highlands, N.J.: Humanities, 1977). The second of these books supplies a possible corrective to some of what Burawoy has to say. The workers studied by Burawoy have won a position of what Friedman calls "responsible autonomy," in which management concedes a considerable amount of control of the labor process to employees in exchange for cooperation in reaching production targets. But there are other sectors of industry where management still applies strategies of "direct control" (Taylorist techniques, involving the close supervision of the labor task). These tend to be sectors in which the labor force is composed largely of women, ethnic minorities, or immigrant workers. Varying types of managerial strategy can here be connected to the analysis of segmented labor markets and to the relation between different types of internal and external labor markets.

The Working Class in the Labour Market. By R. M. Blackburn and Michael Mann. Atlantic Highlands, N.J.: Humanities Press, 1979. Pp. x+359. $25.00.

William P. Bridges
University of Illinois at Chicago Circle

Rarely has an academic monograph made as timely an appearance as this one. Just as sociologists are attempting to redirect the study of social inequality toward the study of industries, firms, and jobs, R. M. Blackburn and Michael Mann have published the results of their survey of nearly 1,000 British workers and their nine employers. Their good timing results from more than luck because the authors had the foresight, as early as 1969, to link their interview survey of industrial workers with systematic observations of the jobs done by these men and with personal interviews of management personnel at each plant.

The research for *The Working Class in the Labour Market* was carried out in Peterborough, a city of about 90,000, among "unqualified" male manual workers (those with jobs not requiring formal apprenticeships). Respondents were selected, not by means of rigorous probability sampling techniques, but through a procedure which might be called "theoretic-example" sampling, a practice aimed at reproducing in the sample the local labor market as it is perceived by the working population. Thus, because knowledge of large firms is widespread and information on smaller employers is distributed idiosyncratically, the sample is biased toward large employers. No firms with fewer than 50 workers were included, and the largest employers were sampled with certainty. In addition, jobs within firms were chosen on the basis of whether or not they provided "examples of the main types of work available [in a firm]" (p. 46). The authors justify the choice of Peterborough itself by the fact that it is small enough for knowledge of jobs to be widely shared and isolated enough to be self-sufficient in terms of labor supply. Rather than a cross section, Blackburn and Mann are constructing a microcosm.

Using this empirical base, they drive straight to the heart of the major issue in the economics and sociology of the labor market—choice versus constraint. In fact, much of the organization of the book derives from a set of conditions which are listed on page 8 as necessary conditions for the effectiveness of worker choice (never mind employer choice). Essentially, these reduce to issues of job diversity, job availability (no discrimination), information and its distribution, stable and diverse worker preferences, and a rough isomorphism of the latter with actual job characteristics. Subsequent chapters of the book evaluate each of these conditions empirically.

The overall presentation plan is designed to appeal to most "structure determines consciousness" sociologists. That is, the opening empirical chapters are devoted to an exploration of the nature of these jobs through the

analysis of the "objective" observational data. First, they present a correlational analysis to assess whether these job characteristics (including wages) are hierarchically or compensatorily structured—that is, whether or not work attributes are packaged in such a way that good features tend to balance out bad ones in most jobs. (They tend not to.) Next, they look at the distribution of job characteristics and wages by firm, again hunting for patterns that would allow workers to make trade-offs. (Only limited ones are possible.) This is followed by an investigation of the workers' knowledge of the kinds of jobs offered by different firms. Of all the empirical chapters, this is by far the most direct and interesting.

At this point, the focus shifts to a lengthy discussion of worker preferences. We are firmly in the grasp of British industrial sociology here, as the discussion is couched exclusively in terms of "job orientations" (à la Ingham, Goldthorpe, and others). The grasp is a bit too firm. While the authors are motivated by the appropriate scientific concern of addressing theoretically given questions, one wishes previous theory had provided more engaging questions. Not only the validity of job orientations as a concept is in question here, but also their genesis, strength, and number. (Is there a "fringe benefits" orientation?) A little relief is provided by a final empirical chapter, which explores the relationship between social background characteristics (primarily ethnicity and imputed social status of parents) and job placements and rewards.

Throughout the text, the authors uphold one worthwhile tradition of British social research—the presentation of data as a means rather than an end. Tables are used parsimoniously and appear in the context of broader questions. Unfortunately, this tradition has not protected them from making some questionable interpretations of these data. First, we are led to believe that cross-sectional data can inform us about the causal direction of the relationship between job orientations and job characteristics. Second, because the authors never test the assumption that the excluded 25% of jobs in small firms are outside their respondents' frame of reference, their failure to include these jobs substantially weakens their findings about the relative absence of market segmentation. Finally, they make frequent and unsubstantiated generalizations about the position of women in the market (perhaps to assure us that their decision to study only men does not reflect "sexism"). While we are cautioned about the first two of these limitations, they are not taken seriously enough.

Despite these problems there are many interesting and better-documented assertions here. These include the importance of internal labor markets, the availability of limited job choice, the restricted generality of the orientations perspective, the dubious relevance of skill as technique, and the proclivity of workers to engage in a kind of Thurowian statistical discrimination against employers. In some ways, however, even these points appear to slip their empirical moorings. I think the main problem is that the book has been written too obliquely: For all the talk about constraint, it is the abstract conditions of *choice* which have been evaluated rather

than the mechanisms of constraint. Despite the concern with workers *in* the labor market, the data concern ex post patterns that the market produced and workers' responses to hypothetical job offers and do not report much about actual market behavior and experience. Although the authors apologize to their Marxist audience for focusing on the *market* for labor, the problem may be that they never really got there.

The Working Class in Welfare Capitalism: Work, Unions and Politics in Sweden. By Walter Korpi. London and Boston: Routledge & Kegan Paul, 1978. Pp. xxii+448. $17.50.

Kirsten A. Grønbjerg
Loyola University of Chicago

The Working Class in Welfare Capitalism is an important book on several counts: its focus is central to Marxist theory; it is a study of an advanced welfare state where workers have considerable political influence; it is a study of union politics; and, finally, it is an important contribution to the sociology of work. Along the way, several issues receive attention: the role and position of workers in society, historical changes and the development of workers' organization, work experiences as sources of dissatisfaction with position in society, the role of work-related social relations as the basis for solidarity and social organization, the functions of labor unions, political views and choices of workers, the importance of industrial conflicts, and the relationship among work, family, and the community.

The primary data consist of historical and archive data from labor unions, as well as several sample surveys, the central one being a comprehensive survey of Swedish metalworkers. The data are analyzed within the context of wider developments in Swedish society. This is a very rich body of data and provides a lasting contribution especially to the sociology of work and union politics.

The theoretical emphasis of the book is a macrosociological perspective on the role of working classes in capitalist societies, using Sweden as a prototype because of its advanced capitalism and strong working class. Walter Korpi argues that if the working class has any role in capitalist societies, it should be here. He sees Sweden as a test case of whether the working class can be successful as its own liberator by organizing internally and developing cohesion among wage earners (Marx's "gravediggers of capitalism"). Korpi contrasts this perspective with the "mainstream" argument, represented by Weber and Disraeli, among others, in which workers are seen as "angels in marble" who become domesticated and their organizations decomposed and integrated into the larger society ("embourgeoisement").

The arguments of Marx, Weber, Giddens, Dahrendorf, and others are reviewed in some detail as they pertain to the focus of the book. Korpi,

however, comes down on the side of the Neo-Marxists when he postulates (p. 21) that the sphere of production is in many ways a more basic category than the market and area of consumption. The means by which he arrives at this conclusion—he acknowledges that this is the central issue of contention between Marxists and Weberians—are unclear. However, the conclusion forms the basis for his subsequent theoretical analyses. As a result it is not surprising that he questions the "embourgeoisement" thesis when developing his elaborate model of mobilization of the working class. The model incorporates nine major concepts (actual differences in power, perceived differences in power, expected costs of reaching goals, utility of reaching goals, rate of exchange between power groups, level of aspirations, relative deprivation, and expectancy of success—all of which affect the probability of mobilizing an actor) in a causal diagram, specifying 11 major hypotheses.

Korpi spends more than 50 pages developing the theoretical framework and the causal model, and although the framework remains visible in most of the remaining 300 pages, he does not return explicitly to the major hypotheses. A careful review of the data would tend to cast some doubts on several of the hypotheses or at least suggest more complex interpretations. Nevertheless, Korpi concludes that "the working class in the advanced industrial societies continues to pose a challenge to the capitalist economic system. . . . Capitalism engenders societal problems which cannot be solved within the framework of capitalism. It [capitalism] can also generate the conditions for the solution of these problems on the terms of the sellers of labour power" (p. 335). Thus he argues that in Sweden the basic prerequisite of capitalism, internal competition among wage earners, is about to be abolished and that the grave diggers of capitalism are still at work and the future of capitalism is open.

Although Korpi contends that Sweden is an appropriate test case of this major interpretation, a more in-depth discussion of the specific features of Swedish society and how these relate to the central argument would have been useful. Are the late state of industrialization, the composition of the Swedish population, the particular history of Swedish union movements, and a series of political compromises intervening or major contributing factors in shaping the role of labor unions? This also raises the critical question whether Sweden is a unique case or the results of this study can be generalized to other societies. Korpi clearly appears to be assuming the latter, while also indicating that Sweden is unique on several important dimensions. It may not be possible to come fully to grips with the generalizability issue, but a discussion of some of these questions in the conclusion would have been in order.

Unfortunately the book does not make for lively reading. It is quite dry and very long. The typeface does not help either. Although the book is ostensibly written with undergraduates in mind, it is the rare undergraduate who will be able to appreciate adequately the theoretical arguments or voluminous data.

In spite of its various shortcomings, this is clearly an important book. It addresses a central issue in sociology, namely, the role of social classes in social and economic change. It also provides a wealth of data useful even for those less enthralled by the theoretical framework. And, finally, it is a valuable addition to the sociological literature on societal studies. Perhaps it is the very richness of the book that is its shortcoming; so much is attempted that it may have been impossible to do it all, or do it all well.

Critical Sociology: European Perspectives. Edited by J. W. Freiberg. New York: Halsted Press, 1979. Pp. xvi+408. $18.50.

Arturo Perez-Reyes
University of Chicago

Critical Sociology is an outgrowth of a summer seminar series organized at Boston University by the editor, J. W. Freiberg. It collects papers by European authors that are to be illustrative of critical sociology. "Critical" is taken to mean on or about "historical materialism" (p. 11). This reduced program is itself not represented well. With the exception of a handful of papers which are summarized below, the book contains little that enriches social science or advances the cause of critical social science.

In the past three centuries of the Western philosophical tradition, "critical" has come to signify different types of self-reflective philosophies that lay claim to revolutionizing consciousness through epistemological turns that unmask previous understandings as having been unaware, misconstrued, and locked within a camera obscura. The work of Marx and of others who have expanded on his ideas has deepened the self-comprehension fought for by this tradition. The task of a critical sociology should be to contribute to this self-knowledge of science and society. Today, as in the time of Socrates' dialectics, self-ignorance dooms us to fall prey to ourselves. Critical social science has the duty of reminding us of our incompleteness and ignorance—that we may not take half for whole.

Unfortunately, many of the papers in this book make a virtue out of ignorance and take "critical" to mean criticizing the bourgeoisie and rooting for the working class. At best this works out to clichés and an interesting remark or two; at worst, language and concepts are thrown about with a fatuity that stretches credibility. The editor tells us, among other things, that for a majority of Americans it is "epistemologically impossible to comprehend 'Eurocommunism' " (p. 7) and that historical materialism is "ontologically different from other 'sociological theories' " (p. 11). Ideologizing, radical conventionalism, ontologically true essences—you name it, this "critical" stew has it. Turning to an essay entitled "Epistemological Practice and the Social Sciences," by M. Castells and E. de Ipola, one finds all Weber's methodological and epistemological reflections, thus

all his work, dismissed by quoting Mao Zedong (pp. 244–45). This essay goes on to conclude that epistemology as the attempt to distinguish truth from error should cease and be replaced by "the fusion of Marxist theory and the working class movement, from the common view-point of class-struggle" (p. 248). Meanwhile, in another space and time, A. Abel-Malek attempts to concoct this bastardized contradiction in terms of a structuralist definition of specificity. The common characteristic here is confused halfway thinking served up as "critical science."

Despite this there are papers worth noting. Adding to the growing body of criticism on Habermas are two articles that take him to task for his Hegelian deflection of the problem of emancipation into a dialectic of self-reflection. A. Giddens's "Habermas' Critique of Hermeneutics" begins by summarizing Habermas's epistemological theory and conceptualization of critical theory as an analysis of distorted communication. Habermas's theory is then said to pose too rigid a polarization between the hermeneutical and the nomological and to make a misconceived distinction between labor and interaction. The main thrust of Giddens's criticism, however, is against Habermas's failure to elaborate on how a critical theory conceived as self-reflection is to gain power and achieve a transformation. U. Jaeggi's "Critical Theory and Revolutionary Consciousness," like Giddens's article, points out that Habermas's emancipatory intentions run aground on his metatheory's failure to treat political practice. This article is a sustained criticism of Habermas's and the Frankfurt School's conclusion that the modern world approaches "closure" as the utopian political moment has been lost. Jaeggi claims that the "self-sealing system of alienation and reification" (p. 72) is not nearly as actual as the alienation of the theorists that spoke of it. Specifically, he attacks their notions of the containment of class conflict, their Eurocentric historical focus, and their absolutization of the critical theorist as the sole repository of freedom from reification through a "discourse" that has become the whole of freedom from domination. Also of interest in the vein of criticism is J. O'Neill's "For Marx against Althusser," previously published in *Human Context* (vol. 6, no. 2 [Summer 1974]). Here Althusser is criticized for his derailment of humanism in the name of creating a scientistic Marxism.

Critical Sociology closes with two papers that serve to point beyond the confines of the book to theorists that have been significantly expanding the study of comparative politics by adopting an eclectic but ultimately historical materialist approach to the analysis of the relations between state and society. This highly sophisticated body of social science is represented here in an adumbrated fashion. Both C. Offe and V. Ronge's "Theses on the Theory of the State," previously published in *New German Critique* (no. 6, Fall 1975), and N. Poulantzas's "The Political Crisis and the Crisis of the State," previously published in French as the introduction to his edited book *La Crise de l'état* (Paris: Presses Universitaires de France, 1976), seek to sketch out a list of general considerations for a historical materialist study of the complexities and novelties that inhere in the state-society relations of the characteristically politicized

capitalist regimes of our day. Both papers accept that the capitalist state has a high degree of autonomy, as it is charged with the double imperative of safeguarding both the proper functioning of the economy and the social welfare of the populace. The overdetermination of this state is described by an elaboration of the political demands and impasses that attend the economic system's "endemic" tendency to crisis. Both articles conclude that the long-run result of political intervention in the capitalist economy is to aggravate the structural dysfunction of the accumulation process, thus worsening economic problems instead of alleviating them. At the same time, this politicization of the conjuncture between state and economy leads to a political crisis, through the warring of different interests and factions and through an erosion of the state's legitimacy as just arbiter. From these conclusions, one paper hopes for a change for the better and the other foresees increasing authoritarianism.

In view of the previous availability of many of the better pieces and the manifest inferiority of many of the others, one is forced to question the value of this compilation. Is critical awareness so well established that it can bear yet another barrage of marginal chatter? Signifiers parade as referentials at the cost of a terrible decline; we should not go gently into that good night.

Social Exchange, Dramaturgy and Ethnomethodology. By Jack N. Mitchell. New York: Elsevier–North Holland Publishing Co., 1978. Pp. x+187. $14.95.

Barry Schwartz
University of Georgia

"Heroic ages of poetry are often followed by hesitant ages of prose, literary creativity by literary criticism, and the doing of sociology by the sociology of sociology" (E. Digby Baltzell, "Epilogue: To Be a Phoenix—Reflections on Two Noisy Ages of Prose," *AJS* 78 [1972]: 202). As I see it, the "heroic" age of contemporary social psychology lasted about two decades—from the early 1950s to the late 1960s—and produced three distinctly sociological models for the analysis of interaction: exchange theory, dramaturgy, and ethnomethodology. The 1970 decade seems to have been a more hesitant and prosaic (though no less important) age devoted mainly to criticizing and refining these theories and to setting them in proper relation to one another. Jack Mitchell's book is to be read as a contribution to the latter program. It is a deep and rich contribution, as far as it goes. The shortcoming of the book is that it does not follow through on the implications of its analysis. Mitchell is unwilling to recognize the larger intellectual prospect to which his own reasoning leads.

Social Exchange, Dramaturgy and Ethnomethodology presents us with a hierarchical organization of perspectives. Social exchange is the master

process. Dramaturgical analysis and ethnomethodology are important to Mitchell only because they fill in the gaps of exchange theory.

The argument of the first two substantive chapters goes like this. Among the many differences between George Homans and Peter Blau, the most important is Blau's systematic attempt to trace social structures and institutions to the "interest psychology" of individuals. This tracing (not derivation) process represents an advance beyond Homans. However, the one weakness in Homans which Blau cannot overcome is the taking of the need structure of the individual as given. Having failed to establish factually the nature of human strivings, both men must assume the rationality on which their theoretical edifice stands. "In Homans, this rationality took the form of the determinism of drives and needs; in Blau, it was represented by the motive for self-interest gain" (p. 77).

The shadow between need and fulfillment, interest and satisfaction, is a long one. What is required, therefore, is a social psychology which makes intelligible the problematic elements in human interaction. This is precisely what Erving Goffman's dramaturgical perspective supplies.

Appearances and support are the things exchanged in Goffman's conception of social interaction, which is ably described in chapter 4. The dynamism of this interaction inheres, not in the internal drives and motives of the individual, but in external, social contexts. Inasmuch as it is bound to these contexts, the hard core of the self melts away. Its performances rather than its purposes become problematic. Interaction thus becomes a matter of managing impressions. These difficulties are typically overcome by a "conspiracy of exchange" (pp. 106–10), that is to say, by protective practices which sustain the definition of the kind of person each participant in an encounter claims to be. In Mitchell's words, "Men tend to conspire to exchange with each other such forgivings and supports as are necessary to maintain the sanctity and continuity of the situation" (p. 122).

According to Mitchell, the conception of the "total institution" represents the beginning of an organizational analysis which Goffman never completes. That is Goffman's first major shortcoming. His second shortcoming (shared with Homans and Blau) is that his ideas are applicable mainly to the modern Western world. Goffman never gets down to the rock-bottom, invariant elements in human interaction. Ethnomethodology does.

The point of Mitchell's discussion of Harold Garfinkel (chap. 5) is to show how the social exchange process is routinely produced and maintained by certain transsituational, atemporal assumptions and practices of the participants in any social situation. These include the assumption of mutuality of perspective, rules for the use of talk in creating meaning, acceptance of appearance as reality, and the recognition that talk conveys meaning only with the help of "glossing techniques"—the use of "et cetera," "let it pass," and other expressions. This list is part of a broader inventory of the universally taken-for-granted qualities of the "invariant interpretive scheme" (p. 147) that underlies all interaction. Through the employment of this scheme, selves and social structures everywhere take

on a compelling "sense of reality." There is no reality "out there" awaiting discovery. Social facts (which include the normative groundwork of social exchange) are "accomplished" in social interaction.

Jack Mitchell's acute critical intelligence, so much in evidence throughout the book, peters out by the time he gets to the chapter on Garfinkel. Not one critical insight of any importance is offered. It is as if Mitchell felt that the power of his argument depended on the certainty of a final theoretical statement that is beyond criticism.

The overall picture emerging from Mitchell's analysis is one in which the social context is made broad, rich, and full, while the individuals in that context are depicted as narrow, poor, and empty. This image does not ring true. Of course, the counterimage of man driven to maximize pleasure and profit carries with it many problems. Too often, invariant needs are invoked in our attempts to explain variable behavior. But the need model has the merit of raising questions about the purposes for which men engage one another to begin with. If man is a constructor and exploiter of the social order, as the ethnomethodologists rightly insist, why does he construct and exploit it in one way and not another? Why are some of us given to aggression and others to benevolence? Why the gallows in one case, the psychiatrist's couch in another? Why freedom here and slavery there? It may be the case that microsociology is not meant to answer these questions. It may be that need psychology cannot answer them. Still, there remains the question whether exchange theory, dramaturgy, and ethnomethodology can continue to skirt these issues and so conceal their impoverished conceptions of institutional order and human nature.

At certain points in the book, Mitchell seems to be heading toward this same conclusion. Repeatedly, he probes for links between the subtleties of exchange and dramaturgy and the more massive structures of society. The establishment of such a connection is one of the points of reference for his criticism. "Homans' theory," he writes, "is weakest when it attempts explanation (to the extent that it even tackles the task) of such objectified and institutionalized social phenomena as power, authority, domination, exploitation and the general structural effects of organization, stratification and equality" (p. 45). And if, in this connection, Blau is more successful than Homans, he is also less successful than he must be. Blau overlooks the enduring tension between the psychoeconomics of self-interest and the higher morality of common interest and common welfare (p. 75). Instead, "Blau . . . suggests that the exercise of power that might otherwise be seen as exploitive is accepted as legitimate by subordinates when they feel they are deriving 'needed benefits' through their subjugation" (p. 76). Assimilating morality to self-interest, Blau's theory underestimates the role of force and fraud in social relations. It takes too much from Marx and gives too much to Durkheim. Similarly, Goffman finds in the oppressiveness of total institutions the basis of a conflict structure within which the individual's "essential self" can emerge—where individuals "in themselves," so to speak, become individuals "for themselves." But this tone of "Marxian

resistance" (p. 116) and "critical sociology" (p. 118), says Mitchell, never becomes dominant. Goffman emphasizes an "underlife" of "secondary adjustments" rather than the conditions of organized resistance and rebellion.

Mitchell is making trenchant and fundamental criticisms. But where do they lead? Perhaps to a chapter on the Frankfurt School? In fact, we are presented with a non sequitur: a discussion of ethnomethodology. The reader expects to be raised up and told of the institutional superintendency of interaction; instead, he is brought down to a discussion of its technique. He waits for a majestic, concluding bang but gets only a whimper.

Jack Mitchell's synthesis may prove to be useful as a model for the observation and analysis of social interaction. But has he (and have we) confused the model with the reality? Clearly, it is one thing to "integrate" the concerns of exchange theory, dramaturgy, and ethnomethodology; it is another to assess the merits of what has been produced. If part of the product of Mitchell's integrative effort seems to be less than plausible, it is because he has not carried his program far enough. He could have chosen to expand the boundaries of interactional social psychology; he has chosen instead to imprison himself within it.

Essential Interactionism: On the Intelligibility of Prejudice. By Barry Glassner. London and Boston: Routledge & Kegan Paul, 1980. Pp. xvi+185. $20.00.

Richard Harvey Brown
University of Maryland, College Park

In *Essential Interactionism* Barry Glassner has written an important and elegantly argued contribution to the logic of the social sciences as well as to the theory of prejudice. Glassner's strategy is threefold: he criticizes orthodox sociology for its confusions of logic and its resultant difficulties in theoretical advancement; he proposes a form of essentialism drawn from social phenomenology as an alternative; and he demonstrates the efficacy of such an approach by applying it in depth to a substantive area of social theoretical inquiry.

Glassner begins by contending that causal, statistical, hypothetical, deductive, and other mechanistic or reified forms of sociology are ontologically and epistemologically naive. He further argues that symbolic interactionism has failed to provide an adequate interpretive sociology of meaning to complement deterministic sociologies of structure. The reasons for the failure of both these schools, argues Glassner, are the inadequacy of their epistemologies and the instability of their concepts. These limitations show themselves most clearly in the confusions in orthodox sociology regarding the dichotomies between idealism and realism, between causality and intentionality, and between explanation and understanding.

Glassner explains how the phenomenological study of social essences—what he calls essential interactionism—can be a way out of these dilemmas. Essences for Glassner form a "complex of characteristics without which the phenomenon cannot be conceived" (p. 34). Essences are not the same as ideal types, however, for the essence of a phenomenon is its necessary and immutable structure, not its ideal or general structure. Such a phenomenological examination of the constitution of our subject matter, and of our own sociological concepts, is not an abstract idealism but an expanded empiricism. Traditional empiricism assumes that certain types of experiences, such as experimentation and correlation, are appropriate for investigating all types of phenomena. For the essential interactionist such an assumption is gratuitous. Instead, it is necessary to determine the appropriate use of appropriate methods according to the essential nature both of the phenomena and of the methods in question.

Essential interactionism also goes beyond conventional symbolic interactionism. Unlike causal or structural sociologists, symbolic interactionists focus on meanings, but they tend to see meanings as resources for explanation rather than as topics to be explained; moreover, they tend to treat meanings as somehow more "symbolic" than other social constructions that are taken as "real." In contrast, the essential interactionist brackets all such distinctions and instead focuses on the essential properties of the phenomena that are either the topics or the resources of inquiry. In addition, while not denying the importance of meanings, the essential interactionist attempts to look at other essences of social life as well, essences that may not have been available to the conscious awareness of the particular actors under study.

So far all this may sound familiar, especially to those who have taken the Long March through Hegel, Heidegger, and Husserl. But Glassner carries the project of social phenomenology beyond previous works by the clarity of his exposition and by his detailed treatment of a specific domain of social scientific inquiry—the study of prejudice. Unlike some who have placed roadblocks against intruders into the domain of social phenomenology, Glassner has prepared maps and guides. Indeed, his book offers a complete travel kit for the uninitiated. This is a considerable achievement.

To highlight the importance of Barry Glassner's work, we should note that the epistemological theme that he is extending has been visible in European literature, philosophy, linguistics, anthropology, and even sociology for many generations. This theme—that reality is socially constructed—goes back to Vico, to Hegel, to Marx, and to Dilthey. But since the turn of the century, discourse on the foundations of the human studies has fragmented into parochial disciplinary concerns. Even today, however, philosophically literate social scientists agree in principle that reality is a social construction. The question is, how much do we adhere to this view in our sociological practice? And how exactly would we adhere to it should we wish to? What kinds of realities are construable, and by what methods can we know them? Durkheim and Mauss began to answer

these questions in *Primitive Classifications,* but Americans read only the Durkheim of *Suicide* and *The Rules of Sociological Method.* Max Weber sketched a social phenomenology in his *Critique of Stammler,* but it too is little read. Husserl treated historical and social phenomena in the *Crisis* and in the fifth of his *Cartesian Meditations,* but Husserl is usually considered a "subjectivist" whose concerns must be irrelevant to social science.

What a pity. What a pity that Wittgenstein had not read Durkheim, that Weber made no use of Husserl, that Merleau-Ponty seemed unaware of Evans-Pritchard, that Mead ignored Marx. Thus the conversation on the foundations of the human studies became muted. And thus it is possible for Lévi-Strauss to announce his "discovery" that primitive thought has a logical basis, even while ethnomethodologists are being hounded out of sociology for exploring the prelogical foundations of modern thought, although both these topics were basic in the intellectual agendas of our classical social thinkers.

All this highlights both the limits and the importance of Barry Glassner's book. Glassner's vital contribution lies not in having invented a new approach but in having articulated an old one in language that contemporary social scientists can grasp. By revealing limitations and contradictions in conventional sociology, by elucidating the methods of a social phenomenology, and by applying these methods concretely to the case of prejudice, Glassner has made us newly aware of seminal sociological truths.

The Social Construction of Mind: Studies in Ethnomethodology and Linguistic Philosophy. By Jeff Coulter. Totowa, N.J.: Rowman & Littlefield, 1979. Pp. ix+190. $18.50.

Peter R. Hecht
Illinois State University

It is rare that the major issues that confront social scientists are resolved as neatly or as quickly as happens in the pages of *The Social Construction of Mind.* In one shot Jeff Coulter has attempted to resolve the problems of mind/body relations, causal explanations in the social sciences, and the appropriate level of analysis for human behavior. Rarely, however, are such efforts convincing, and unhappily this book follows the usual trend.

The answer proposed is cut from the cloth of Wittgenstein's and Ryle's linguistic philosophies. In this tradition Coulter focuses on descriptive analyses of the logical grammar in persons' situated language. This approach is tied to ethnomethodological concerns about the grounds for intelligibility of everyday actions between members of a society. The major thrust of the text is aimed toward indicating the primacy and appropriateness of this method for dealing with aspects of human behavior usually studied by sociologists, social psychologists, and cognitive psychologists under the rubric of subjective experience.

Coulter takes "mentalist" and "behaviorist" positions to task over the use of such concepts as the "I" or ego, understanding, emotions, thinking, and memory. The criticisms advanced in each case follow a similar pattern in preferring the literal over the figurative, the social over the individual, and the normative over the determinist.

A preference for literal over figurative usage is a bias Coulter shares with ordinary-language philosophers and which results in an inability to deal with metaphor. Here it leads him to reify the metaphors of other theorists with whom he is at odds. This unwarranted literalism is apparent, among other places, in chapter 6, "Phenomenological Residua," in remarks that role theoretical conceptions of human conduct lead to implications of faking and detached acting. Coulter says, "Am I, for instance, to be understood as now playing the *role* of the author of a book or paper? If so, then this would strongly imply that I am not *really* the author of a book or paper, but masquerading as such" (p. 114). Comments such as these constitute a reification of the metaphor "role" usually found only in the hoary arguments of behaviorists, and this misunderstanding is compounded by a narrow and unusual use of the term "play." It is not through the regular use of metaphor, but only through this sort of peculiar misuse, that the reifying generalizations Coulter inveighs against occur in theorizing.

Within the same chapter, reification of metaphor is compounded by occasional gross misinterpretation. Attempting to show that the concept I is not a referent to a substantive being, but only an indexical expression, Coulter manages to lump together G. H. Mead, Freud, Husserl, and Sartre. Freud is particularly abused by the author's comment that "I am unable to take his [Freud's] theory of mental functioning seriously enough to do it expository justice" (p. 115). This is understandable because Coulter's literal reading of metaphor leads him to assume that Freud actually believed "that the 'ego' *is* a subsystem of neurones" (p. 117). Freud's position on the ego ended up as a metaphorical depiction rather than the physical topology Coulter accuses him of. Adding insult to injury is the even more peculiar statement at the conclusion of the chapter that Mead, Freud, Husserl, and Sartre each claimed "to be providing a literal and scientific account of mind" (p. 123). Such a bland generalization stretches one's belief in the seriousness of the author in this undertaking.

Misstatements like those described above pervade this book and render it difficult to give Coulter's arguments the attention they probably deserve. This is unfortunate because there is much of interest in what he has to say that is obscured by a cavalier and self-serving manner of dealing with other perspectives on human behavior. The chapter "Affect and Social Context" provides a stimulating account of how social context may be described as accrediting or denying certain emotion statements in social interaction. The account does not convince one that social context and linguistic negotiation are the primary arbiters of emotions, however, as the author would have us believe.

There is a temptation for authors to view their own theories as unique

repositories of intellectual virtue. That may be acceptable if one is dealing in theology and intends to create a supporting dogma, but in the social sciences it is more than slightly out of place. *The Social Construction of Mind* may appeal to the true believer in ordinary-language philosophy; the rest of us will have to wait for a more reasoned presentation.

The Systems Approach and Its Enemies. By C. West Churchman. New York: Basic Books, 1979. Pp. xii+221. $12.00.

Alan Sica
University of Kansas

Imagine an author whose goal is to contribute seriously to the debate over "systems analysis" (or "the systems approach") and its applicability to problem solving at the macro level in contemporary society, an author whose entire battery of intellectual resources includes Saint Paul, Kant, Bentham, Jung, and, most crucially, someone named Edgar A. Singer, Jr. (who bested Kant, it is claimed, in moral philosophy), the author of such monuments to thought as *On the Contented Life* (New York: Holt, 1936) and *In Search of a Way of Life* (New York: Columbia University Press, 1945 or 1948 depending upon whether the citation on p. 97 or that on p. 139 is believed). Continue to ponder an author who hopes to add something to the "sociology of religion"—without apparently being aware of the field—and whose sole sourcebook is Gerald Heard's *Social Substance of Religion* (London: Allen & Unwin, 1931), in preference, say, to the work of Troeltsch, Weber, Wach, or anyone since. Then picture this same author claiming to have written a book structured around "logic," worrying constantly over whether "tautologies" are creeping into the prose from his "rational mind," and who is capable of this: "As for aesthetics, I feel that it's largely feminine, but that's primarily because I'm married to a woman with a deep aesthetic appreciation" (p. 170)—and whose only important example from the world of aesthetics is the *Ninth Symphony* (by Ludwiganna Beethoven?). Is it also possible that this writer believes that Kant's book is entitled *The Critique of Pure Practical Reason* (pp. 97, 108), while posing as an astute and long-time student of Kant's work? Does he believe it is wise to write a chapter titled "Logic: A Theory of Reality, or Kant for Planners," based ostensibly on reconstructing Kant's categories into a pragmatic "scheme for planners," in which Kant's quality, quantity, relation, and modality are converted into Client, Decision Maker, Planner, and Systems Philosopher (pp. 79–80)? Perhaps he also does not realize that the 1889 Abbott translation of Kant's *Critique of Practical Reason* has been superseded by the Beck version published in 1949, or that Nietzsche's works are best read in Walter Kaufmann's translations, not in those he uses? Does the author not know that one cannot break the German for "worldview" like this: "Weltans-chauung"

(p. 105), or that the phrase "military test sight" (p. 61) leaves something to be desired? Less trivially, has it occurred to him that "dialectics" means a good deal more than throwing two polarities together (Hegel said something about this) or that it is somewhat imprudent to write, "The point [*sic*] he makes in the *Critique of Pure Reason* is the one that needs making over and over again today—namely, that in order to do our jobs as systems approachers, we must construct a theory of reality which will then guide us in the observations we make, which in turn will guide us in the revision of our theory of reality. Surprisingly, Kant's very elementary point . . . is frequently ignored in the current writings on the systems approach" (pp. 43–44). Surprisingly? Perhaps so, given the central emphasis Kant's work receives in all graduate programs of planning and systems analysis.

What does one make, in a monograph loaded with learning, of these sentences: "I was brought up a Catholic, a faith whose mark on the soul can never be fully eradicated" (p. 98); "When I was young, my mother told me I would discover the cure for cancer; mothers should never do such things, because for a long, long time I thought I would. Eventually I became a planner-holist" (p. 155); "There was a time when I seriously considered becoming a Catholic monk" (p. 187); and "Our protection of the whooping crane [through the Endangered Species Act] may be a case in point: although we humans were certainly the cause of its decline, it may have reached the point where it 'wanted' to become extinct" (p. 177).

There is something called "scholarly discourse," the precise nature of which has recently drawn the attention of many writers, especially those influenced by current French philosophy and literary criticism. One of the cardinal principles of "discourse"—in order to be genuine—is to deal honestly with sources, to give them proper hermeneutic treatment. Although it is sometimes refreshing, even "creative," when a sophomore juxtaposes fragments of Machiavelli's thought with bits from Buddhist logic, similarly forced experiments, unless done with true virtuosity, do not come off for senior scholars. The author of this book, C. West Churchman, has been publishing important works in systems analysis (he repudiates the term now) for 35 years, and is professor of business administration at Berkeley, plus research philosopher at the Center for Research in Management Science. *The Systems Approach and Its Enemies,* then—especially since it deals with "ethics," "politics," "morality," "religion," and "aesthetics"—could be considered a life's summing-up, a synthetic statement of real use to workaday planners and other social scientists with interest in the field. Instead one finds a raw, undisciplined, illogical, fraudulently "broad-ranging" agglomeration made of Jung's "individuation"; Kant's moral dictum that humans are ends, not means; Singer's "modern form of absolutism"; his concept of the "ideal" (p. 135); plus other tidbits, all designed to illuminate the definitive distance Churchman sees between the "ideal hero-planner" and the "objective planner," whom he has come to detest and pity. And what constitutes this distance? Very

simply, the former seeks a "comprehensive," holistic view, the latter does not. This is the book's singular idea.

Churchman's heart is in the right place, and he is clearly an inventive, whimsical, interesting writer. But why he has taken this peculiar path in offering his view to his colleagues—or did he have some phantom readership in mind?—escapes me. He wants to make the simple point that planners should stop applying mechanistic, scientistic "models" and "solutions" to human problems encumbered with "meaning," which, by their nature, resist linear, "C-B" (cost-benefit) analysis. Here Churchman would have profited from the *Methodenstreit* between Menger and Schmoller in the 1880s and its continuation through Rickert, Windelband, and Weber. But since no sociologists, past or present, appear in the book (except Mannheim, once), perhaps Churchman does not realize that others have already considered at length his "big" problems: the nature of rationality and rationalization; organizations, their birth and death; the relation between belief systems and social changes; deviant roles and their power in bureaucracies; knowledge as conditioned by social structure; secularization and its impact upon faith-based creeds. And so on. What Churchman has managed to do is trivialize Kantian philosophy beyond recognition, promote usage of "dialectics" of a pre-Kantian caliber (though, oddly, being aware of Hegel [p. 115]), accuse the four undefined congeries listed above (politics, morality, religion, aesthetics) of "being" the "enemies of the systems approach," but then embracing them all—*Aufhebung,* perhaps?—by "becoming them," by swallowing their objections to "rational planning," and thereby liberating planning from its pseudo-objectivity, while enlightening the "enemies." This is the sort of hyper-general, heavily anecdotal foray into "social philosophy" that one had assumed was dead. In fact, if the book has value, it lies in the stories from Churchman's life: founding an early department of systems and management planning; assessing the Berkeley medical school in its infancy; going to one New Year's Eve party in his town, Mill Valley; serving as a reviewer for journals and funding agencies; and, most of all, puncturing through reminiscence the pretensions of planners and systems analysts who don the white coat of science at every opportunity.

Churchman does not care for "narrow-minded assistant professors, who study small problems in accordance with accepted standards of research" (p. 13). Who does? Even more obnoxious, however, are aging full professors who lard their work with gratuitous or farfetched references in a "homespun manner" (p. 157), knowing that publishers will accept their manuscripts with little scrutiny, unlike, of course, those of younger writers. The really bold move is to write a book free of any references—as did Nietzsche, one of Churchman's heroes—laying out one's ideas uncluttered and unprotected. (Had that been done, this book would have shrunk to an article.) Or, if reference need be made to other work, let it be in the spirit of professional discourse, scrupulously rendering others' ideas, while advancing one's own. This sort of thing can be done. Max Weber managed

it. But then Churchman, analyst of "social systems," does not know of this man. Edgar A. Singer, Jr., is no substitute.

Applied Mathematical Demography. By Nathan Keyfitz. New York: John Wiley & Sons, 1977. Pp. xxiv+388. $19.95.

Aage B. Sørensen
University of Wisconsin—Madison

Population theory is unique among the social science theories outside of economics. It is formulated in a set of equations that express relationships among the basic quantities governing population processes: births, deaths, and time. The mathematical formulation of the theory provides a machine that—in Nathan Keyfitz's apt formulation—has levers that can be moved to provide answers to a wide range of questions. The questions include those the theory was designed to answer concerning the effect on population characteristics of changes in the various quantities governing population processes, as well as questions the theory was not designed to answer, for example, the question of how the speed of promotion in bureaucracies depends on population growth.

Applied Mathematical Demography is about how to use formal population theory. The range of topics covered and the insights provided into applying the theory represent an impressive achievement. This reflects both the strength of the theory and the creative uses of it by Keyfitz, who in other publications, particularly *Introduction to the Mathematics of Population* (Reading, Mass.: Addison-Wesley, 1968), has made significant contributions to the theory.

The main topics covered in the various applications of the theory are the standard topics of demography: mortality, fertility, age distribution, and population growth. For those concerned with population problems a number of important questions are dealt with in a manner that richly illustrates the strengths of the theory. Demographers will find numerous answers to questions about what can be expected to happen to a population characteristic (for example, the age distribution) if a basic quantity governing population processes (say the birthrate) changes. Demographers will also find instructive discussions of the properties of measures and indices, as well as information on the construction of a life table, on the calculation of population characteristics from inadequate data, and on the hazards of population projections. Furthermore, they will find important discussions of the current limitations of the theory and on the uses of empirical data.

The value of the book to demographers is obvious and need not be elaborated further. Other audiences for the book are identified on the jacket as commercial research organizations and family planners. This appears to reflect the publisher's concern for identifying audiences that are par-

ticularly involved with applied research. *Applied Mathematical Demography* should be useful for commercial research organizations. There is no doubt that the book can be very instructive to sociologists concerned with modeling social processes. It serves as a source of concrete ideas about how to apply population mathematics to problems other than the standard population questions. And, equally important, it serves as an instructive example of the power of formal theory of social processes.

The applications of population theory are of two types. One is the use of the theory to analyze the demography of organizations and other subsystems of the national populations usually focused on in mathematical demography. Keyfitz here mostly reviews contributions by others. The book is particularly rich in illustrations of the second type of application, the use of the theory to answer questions it was not designed to answer but for which it is shown to be of great relevance. An important example is the treatment of how promotion opportunities in hierarchical organizations depend on the rate of population increases. Keyfitz's stimulating treatment of how opportunities vary with cohort size is supplemented by an interesting discussion of the difference between the analysis of this problem using formal theory and an analysis that would rely on survey data. Other examples in this category of applications include the calculation of kin numbers, the comparison of pension systems, and the importance of changing age distribution for student-teacher ratio.

The many, varied, and convincing applications of population theory show that mathematics is a powerful tool for the construction of theory. The achievements of population theory remain largely the hope of mathematical sociologists. There are, of course, models for all kinds of processes and structures in sociology. However, often these models are not very interesting theories and/or they are empirically inadequate. So sociological theory remains something generally considered very different from mathematical sociology, and applied sociology is mostly the application of sociological methods rather than of theory.

There are several reasons for the differences in accomplishment between formal theory for population processes and formal theory for other social processes. Population processes may have simpler mechanisms than most other social processes. The origins of mathematical demography are closely linked to actuarial concerns with important practical and financial implications, and these concerns provided population theory with important contributions from gifted users of mathematics, such as Lotka. Finally, demography offers modelers a wealth of data with which to test formal theory so that models can be corrected and improved. None of this would preclude mathematics becoming an important tool for theory construction in sociology. There may not be very conspicuous movement in that direction, as mathematical sociology remains a rather small and to many a quite esoteric specialty. However, *Applied Mathematical Demography* should serve as an important source of stimulation and motivation for the efforts that are being made in mathematical sociology by demonstrating what can be achieved with formal theory.

Conversations at Random: Survey Research as Interviewers See It. By Jean M. Converse and Howard Schuman. New York: John Wiley & Sons, 1974. Pp. x+111. $5.50 (paper).

Paul B. Sheatsley
National Opinion Research Center

The tardiness of this review, some seven years after the book's publication date, probably reflects its status as something of a "sleeper." Although it received a laudatory review by Herbert Hyman in *Public Opinion Quarterly* in 1975, its considerable reputation among survey researchers seems to have been achieved almost entirely by word of mouth.

The authors, Jean Converse and Howard Schuman, are affiliated with the University of Michigan's Institute of Social Research. As part of a teaching course in survey methods, graduate students who had spent a week in Detroit conducting interviews on the Detroit Area Study were asked to write a brief essay on "insights gained in the course of pretest and final interviewing" (p. v). The essays of some 150 students collected over the years constitute the raw material of *Conversations at Random.*

Converse and Schuman have not attempted to code and quantify these protocols—which sometimes came in the form of poetry, satire, or fiction—but instead have used them as illustrative material to "present the reality of survey research as interviewers see it" (p. v). While the students' reactions to the interviewing experience are demonstrably different from those of more experienced interviewers, it is what the authors call the "freshness" of their views that gives the book its persuasiveness and immediacy. These are not jaded professionals or armchair researchers talking. They are young people freshly emerging from their first exposure to the task of administering an hour-long questionnaire to a probability sample of individuals in their homes.

There is great poignancy in some of the experiences related in these essays. Example: The interviewer screwing up courage to ring the doorbell of an inner-city apartment, fearful of who or what might be found inside; the respondent on the other side of the door, behind the triple locks, quaking with apprehension of who might be there. Each is terrified of the other. If an interview results, is that not a triumph?

The reactions of these bright young students to the interviewing tasks assigned to them raise anew some persistent problems that the world of survey research has never fully resolved. For example, how does one reconcile the objective of administering a standardized questionnaire in a uniform way with the infinite variety of respondents and situations with which the interviewer must cope? Most of these interviewers were very uncomfortable just reading the questions, using prescribed probes, recording the precoded answer, and going on to the next question. Respondents wanted to relate to them, and they wanted to acknowledge the individuality of their respondents.

Although some interviewers heartily disliked some of the respondents, almost uniformly they came away with an expanded and more sympathetic view of other people and their problems. Indeed, they found themselves identifying with the respondents rather than with their own home office when the questionnaire did not work as intended. Forced to read a question that was obviously repetitious or inappropriate, they felt like apologizing and mentally cursed the study director.

Many felt very uncomfortable in the task of forcing respondents' replies into prescribed answer categories. They listened to, and dutifully recorded, the respondent's attempt to answer "both" or to hedge the response with elaborate qualifications, and it was frustrating to mark the answer irrevocably as "Code 2—Disapprove" when the opinion was so much more complex. The authors refer to this process as "a kind of perverse *unpainting to number.*" "The interviewer and the respondent have cooperated painstakingly in fashioning an individual portrait, but . . . all manner of opinions and feelings will be ruthlessly stripped down . . . and fed into the maw of the computer" (p. 76).

The authors, of course, do not and cannot solve all the problems of data collection in survey research. But *Conversations at Random* reminds us of these problems in a witty and provocative manner, and it does offer some sound advice. There is an appendix on interviewer training that provides a useful list of exercises to prepare interviewers for the field. There are good tips on questionnaire writing, such as avoiding long batteries of questions with identical answer categories and varying the pace with occasional open-ended or other different types of questions. There are recommendations along the lines of those in Schuman's article "The Random Probe" (*American Sociological Review,* vol. 31 [1966]), urging us to explore the respondent's understanding of our question and the meaning of his precoded response.

I heartily recommend this book to anyone interested in survey research. With the quality of our product dependent upon the activities of field interviewers, and with interviewers' voices too often heard only by their respondents and their immediate supervisors, it certainly behooves us to understand more than most of us do about what goes on out there. This little paperback can stand alongside Stanley Payne's *The Art of Asking Questions* (Princeton, N.J.: Princeton University Press, 1951) as a classic in the field.

The Double Standard: A Feminist Critique of Feminist Social Science. By Margrit Eichler. New York: St. Martin's Press, 1979, © 1980. Pp. 151. $15.95.

Patricia Ward Crowe
University of Chicago

In this highly polemical work, Margrit Eichler assumes the role of feminist gadfly, spiritedly attacks numerous approaches to the study of women, and concludes with her own vision of what a nonsexist society would include. Although I agree with many of her assertions, they follow only loosely from the body of the book, which itself suffers from a shotgun approach to the intellectual foibles and inconsistencies of those who have written about women. Despite certain structural problems and a few striking omissions, however, *The Double Standard* usually hits its targets with remarkable precision.

The "double standard" refers to the universal practice of evaluating similar human attributes differently depending on the sex of the person. A major thesis of the book is that many feminist writers unwittingly reinforce the very inequities they seek to abolish by using sexist language, by adopting andro- or ethnocentric perspectives, by confusing normative and behavioral standards, by failing to differentiate structural divisions within societies, by not taking into account the differing interpretations of various actors, by overemphasizing sex-linked differences (however they may be defined by that society), and by minimizing women's accomplishments. Eichler performs a valuable service in relating these problems of ethnographic interpretation specifically to studies of women.

In pointing out the conceptual difficulties of defining egalitarian societies, she asks us to imagine what a society without a double standard might be like, then adds immediately, "Such a society does not, *of course,* exist anywhere" (p. 14, emphasis mine). This is bold stuff, and the reader hopes to discover in the course of the book why "of course" no such societies exist. Instead of confronting this basic issue (the universal second-rate status of women), however, the book enumerates cross-cultural examples of sexual inequity and demonstrates the questionable premises of various theoretical works that do seek to answer the question. In so doing, Eichler ignores several sources that certainly ought to have been mentioned and fails to offer any positive solution to the problem, except for a few poorly substantiated, pragmatic suggestions such as paying women for women's work, eradicating sexist language in teaching materials, and the like. *The Double Standard* attempts to combine theoretical critique, enumeration of contemporary ills, and a political agenda for social change; these three separate endeavors are not well integrated. Nonetheless, the descriptive and critical commentary deserves attention.

In addition to pointing out the difficulties of ethnographic interpretation, Eichler illustrates the varied tasks performed by men and women in

different societies. In focusing primarily on productive labor, however, she omits other aspects of life equally subject to a double standard and ignores the persuasive and generative work of Margaret Mead (e.g., *Male and Female* [New York: Morrow, 1949]), which would have greatly enhanced her argument for the variability and flexibility of masculine and feminine as culturally defined. Eichler does mention personality variables in her discussion of the concept of androgyny, which she justly claims reinforces sex stereotypes by implying that androgynes combine intrinsically male and female traits rather than merely exhibiting fundamentally human ones. This discussion is not integrated with the economic analyses, however.

As the title implies, it is the differential evaluation of male and female attributes more than the nature of the attributes themselves that plagues women, and feminism itself may be seen as an existential quest for identity and meaning. Yet Eichler ignores several theorists who use cross-cultural materials to grapple directly with the disparate valuation of men and women, notably Nancy Chodorow, Sherry B. Ortner, and Michelle Zimbalist Rosaldo (see, e.g., their essays in *Woman, Culture and Society,* ed. M. Z. Rosaldo and L. Lamphere [Stanford, Calif.: Stanford University Press, 1974]), who discuss the implications of infant relations to the mother, the association of women with nature rather than culture, and the disproportionate association of women with things domestic and private rather than public.

Eichler reserves her most trenchant and most theoretical criticism for Marxist studies. She elucidates theoretical problems inherent in different definitions of class, social units of analysis, women's relationship to means of production (though she does not explore the consequences of rates of labor force participation), and the nature of women's products; however, she does not transcend certain basic Marxist assumptions about the value of labor. This discussion would have been greatly extended and deepened by reference to Hannah Arendt (*The Human Condition* [Chicago: University of Chicago Press, 1958]), one of Marx's most incisive and coherent critics and a philosopher fundamentally concerned with what it means to be fully human. On this basis alone, Arendt contributes enormously to an understanding of feminism. *The Double Standard* becomes so absorbed with description, specific criticism, and exhortations for change that it seems to lose sight of this most central issue: what it means to be fully human and why most women get defined as less so. A good critique of much of the existing work on women, *The Double Standard* nonetheless leaves central theoretical issues almost untouched and fails to suggest a theoretical framework superior to those it criticizes.

Families in Former Times: Kinship, Household and Sexuality. By Jean-Louis Flandrin. New York: Cambridge University Press, 1979. Pp. xvi+265. $32.50 (cloth); $8.95 (paper).

Wini Breines
Northeastern University

Families in Former Times is a highly specialized historical account of early French domestic life in the tradition of the *Annales* school. Its detail is massive and its learning prodigious; it is a fascinating book devoted to changing domestic ideology and behavior in France, and to a lesser extent England, from the 16th to the 18th centuries. Working from documentary evidence, such as diaries, notebooks, and confessional manuals, Jean-Louis Flandrin studies the structure of kinship and households and the accompanying customs, morality, and feelings. He is most interesting about changing values and behavior, which, he suggests, are often a result of morality imposed, not without resistance, by elites, particularly the Catholic church. It is the harsh influence of the Church on domestic life and reproduction, in conjunction with excessive fertility and poverty, and the popular classes' adjustment and adaptation that form the core of the book: how did kinship and families change in character between the 16th and 18th centuries and a new morality of family relations come about?

Let me list the topics Flandrin includes and then suggest what might be of interest for sociologists of the family and of everyday life. The first chapter discusses, relatively technically, the ties and shape of kinship, particularly the distinction between family and household and historical kinship variations in France. The second major chapter considers the household, its size and structure, and the powerful effects of conditions of agricultural production and of "rootedness in ancestral soil" on its structure. As does Philippe Ariès in *Centuries of Childhood* (New York: Vintage, 1965), Flandrin devotes considerable attention to the material context of domestic life, considering the evolution of interior architecture, and sleeping, eating, and evening activities. In the third chapter, entitled "Domestic Morality"—a topic clearly close to Flandrin's heart—he considers the imposition of a dominant Christian morality, characterized by hierarchy and authoritarianism, on relationships between husband and wife, master and servant, and parents and children. It is fascinating to read of the clergy's condemnation of love and passion between spouses (seen as pleasurable and therefore selfish), including all forms of coition except the "missionary position," and their emphasis instead on married love (Christian love), selfless and dutied. His examination of the slow development of the word "love" toward today's meaning is pertinent for those of us interested in understanding the changing nature of family bonds. Finally, the last chapter is devoted to the evolution of reproductive and sexual life, again presenting a struggle between Christian teachings and everyday life and realities, particularly demographic realities in the

form of infant mortality. Fertility, contraception, infant mortality, and wet nursing formed the material realities of daily life, and Flandrin explicates their fluctuating incidence and meaning.

Flandrin has clear differences with the Cambridge group of family and household demographers (as well as with French historians), who have attempted to "relegate the extended family to the museum of sociological myths" (p. 3). They do this by ignoring censuses in the south of France that confirm its existence and the coterminous character of kinship and neighborhood, thus underplaying the solidarity of kinfolk. Kinship was inseparable from belonging to a village community, according to Flandrin, and customs relating to private and family life were based on village membership. Neighbors had obligations to and solidarities with one another which were reinforced by kinship and vice versa. A sense of such interconnectedness can be gained from Kai Erikson's study of the community in Buffalo Creek (*Everything in Its Path* [New York: Simon & Schuster, 1976]). Flandrin's description of evening get-togethers of the community, not simply the family, reinforces this proposition. His point is that to emphasize conjugal structure simply on the basis of censuses and parish registers is misleading, since there was an "abundance of ties of solidarity in past times" (p. 49). Tamara Hareven has argued that, because of high fertility and infant and maternal mortality, families in the past were subject to involuntary forces which made their lives more chaotic and unpredictable than family life today ("Family Time and Historical Time," *Daedalus* 106, no. 2 [Spring 1977]: 55–70). This argument, too, looks narrowly at the biological family facing life alone and pays little attention to the neighborhood and community in which it was embedded, which provided support unseen by demographers considering only household or birth statistics.

Another objection to the Cambridge group is that their "statistical approach artificially reduces the importance of complex structures" (p. 74). Flandrin argues that it is inaccurate to average household size, because from the 10th to the 18th centuries a variety of household types coexisted; averages calculated on the basis of parishes cannot indicate this. Flandrin points to an alternative picture of a France in which complex families were common.

Flandrin's evaluation of past family life is refreshingly nonmoralistic. He says, for example, that most contemporary historians are repelled by the communal living and lack of privacy of former times, unable to separate themselves from their own time-bound family sentiments and "present day delicacy" (p. 101). That servants and masters, adults and children, and parents and children often slept in the same room and sometimes the same bed disturbs us. It has not "occurred to historians to see in this practice the cohesion of the family in past times" (p. 101), and Flandrin proposes that we must at least admit the possibility that communal sleeping among the peasants and other poor people was one of the "most interesting manifestations of the communitarian spirit, with the communal bed as one of the privileged places of family life" (p. 101). In any case,

he concludes, the communal bed was almost the *only* meeting place of poor families, given their work life and the discomfort of their homes.

Finally, Flandrin's most powerful contribution is in elucidating the role of church morality in reinforcing and creating hierarchical and authoritarian relationships between people, particularly husbands and wives. The patriarchal purposes of the Church in winning complete obedience to God were attained through the central importance in Christianity of the authority of the father or the lord and the subordination of servants, wife, and children to him. One could not call oneself a Christian and question the right of the father to obedience. God was the creator-father, the sovereign-master, and his authority was legitimized and reinforced by the authority of the father in the family and vice versa.

By the early 18th century the status of the wife in the daily life of the household gradually improved, although Flandrin's evidence of the husband's authority over and inhumanity toward her throughout the period, encouraged by law and by the Church, is shocking. Flandrin does not equivocate about the purpose of the family: reproduction. For the Church, marriage was, in addition, the remedy for avoiding immoral sexual practices. A dramatic conflict was set up, familiar in its outlines to women today, between conjugal duties, primarily sexual, and maternal duties, primarily nursing babies, preventing premature conception, and preserving infant and maternal health. In our own period tension exists between woman as individual and woman as wife and mother, as is highlighted by the anti-abortion and feminist movements.

Controlling fertility, which the Church opposed, was a way in which the conflict could be eased and was therefore an arena of struggle on many levels. Flandrin makes it clear that birth control, primarily coitus interruptus, was in the interest of the wives and that they led the way in its adoption. There was no "decline in morals," but a growing interest in preventing excessive numbers of children and infant mortality. The Church's insensitivity to the excessive fertility which hampered its *own* ideal of family life is underscored by Flandrin. He suggests that the decline in fertility was the result of a transformation of conjugal relations in which parents accepted more responsibility for their offspring and refused to acquiesce in infant mortality, and in which the balance of power and responsibilities shifted between husband and wife. In short, a new family morality, not unfamiliar to us today, was being constructed.

American Journal of Sociology

Sex and Age as Principles of Social Differentiation. Edited by J. S. La Fontaine. New York: Academic Press, 1978. Pp. vii+188. $16.50.

John G. Galaty
McGill University

In this volume of essays originally presented at the 1977 meeting of the Association of Social Anthropologists of the Commonwealth, two sociological fixed points are examined anthropologically. The resulting volume, *Sex and Age as Principles of Social Differentiation,* typifies the range of contemporary theoretical perspectives and approaches pursued by British-trained anthropologists, and—if one reads between the lines—their current controversies and academic conflicts. For sociologists, the proceedings should be valuable in making readily accessible the following *anthropological* contributions to the study of a very old and fundamental problem, the relation between age and sex differentiation: first, it offers a comparative perspective on an apparently universal configuration of age and sex; second, it proposes a systematic juxtaposition of two principles often considered separately; and, third, it develops and emphasizes the relativity of these two apparently natural anchors of human society.

For the comparative perspective, the book includes a useful theoretical and topical overview in the introduction, a concluding chapter which deals with general issues of East African age organization (reprinted from a recent work on that topic), and six cases dealing with the Laymis of the Bolivian Andes, the Pirá-Paraná of Colombia, the Nyamwezi of Tanzania, an intertribal association of Australian aborigines, Hausa of urban Kano in Nigeria, and the Dassanetch of Ethiopia. I doubt that a more representative sample of ethnographic cases could have been compiled (apart from the exclusion of most Asian societies) that would have accounted as adequately for ranges of interest in self-sufficient/complex, relatively untouched/relatively acculturated, urban/rural, or politically decentralized/centralized societies. Evidence is provided for both high and low degrees of role differentiation by age and sex, and the generalized cultural emphasis of either age, or sex, or both, or neither. In short, the classical function of comparative material is served here: the refutation of any universal hypothesis by the "apt example." Sociologists, beware.

The editor, J. S. La Fontaine, rightfully notes that anthropological findings on age- and sex-differentiation systems have largely developed different theoretical concerns, those of age focused on power and politics, those of sex on male/female symbolism and the question of female authority. The two issues have been further divorced by the confusion of the study of sexual differentiation and the study of women and female inequality. La Fontaine concludes that the two principles of social differentiation show many features in common, since both are "formal systems [with] certain logical properties," "constructed of selected elements drawn from the processes of human physiology" (p. 18). However, she

fails to pinpoint the major contribution of the case studies taken together, that is, the interacting and mutually constitutive role of the two, rendering an account of either an age or a sex system alone inadequate.

The editor makes clear in her introduction that the "natural differences" involved in issues of age and sex are fully social and cultural. In an anthropological discussion of age or sex differentiation, she asserts, we are faced "first with a conceptual model which establishes distinctions based on the perception of significant 'natural' differences. . . . The clusters of contrasted qualities are accorded the status of intrinsic 'facts of life' by association with what are asserted to be natural differences. That these are, on the contrary, 'facts of culture' has been established beyond question" (p. 2). Indeed, although "natural" bases for age and sex differentiation appear in the ideologies of most cultures, their "nature" changes with the symbolic definitions and elaborations of the case, and with those changes is found concomitant variation in the modeling and shaping of behavior.

The contributions in this volume emphasize either symbolic or political dimensions of age and sex differentiation, with special focus on economic processes and the division of labor. Three contributions within the African area represent high-level examples of the focused, limited, and disciplined interest of British anthropology in structural-functional approaches. In particular, the connections between resource control, property, and forms of social organization and the political process are examined with respect to African age organization. But while the editor points toward the use of age differentiation as a framework for political organization, Baxter and Almagor, the authors of the survey article on the topic, maintain that age systems have been inappropriately endowed by analysts with political functions which they are ill equipped to perform, since they do not control corporate resources. Their conclusion is that age systems should be understood not as performing practical societal functions but as conferring the benefits of ritual upon society, of "giving ageing a cultural stamp." While appreciating the abstract attention given to the ritual domain, I feel uncomfortable with the theoretical split between ritual and politics, which fails on the one hand to recognize the political and economic roles carried out within the ritual process and on the other hand leaves the political domain, as it were, "exposed" without the institutional basis within the age system which I think can be adequately documented.

La Fontaine notes that the study of "symbolic" aspects of age organization remains undeveloped, but she appears less aware that this analytical tendency rests on the sort of theoretical dualism she expresses in asking for study of the relation *between* the "symbolic system" and the division of labor by age or sex. The essay I found the most exciting and innovative in the collection rejected this formulation, for in "Food for Thought," his analysis of the Pirá-Paraná, C. Hugh-Jones sets forth a quite different theoretical program, one exploring the "ordering of socio-economic activity as an integral part of a wider symbolic system" (p. 42). The production,

preparation, and use of such foods as pepper, manioc, and coca are related to the reproduction of social units: for example, pepper, a female cultivated plant, is seen as an agent of sexual conjunction and the wifely status of women. In short, the distinction between the conceptual and the material domains, so prominent in much of this book, is broken down by the view of symbolic processes as an admixture of the two.

The nature of ritual well illustrates the theoretical tensions within this collection. If in many contributions ritual is seen in contradistinction to political and economic domains, in the case just discussed ritual is seen to be "an expansion of everyday life," involving a "re-use of products symbolising processes of reproduction of social groups in order to control the same processes" (p. 64). If we apply similar insights directly to the cases of age and sex, it appears that the ritualization through symbols of the particular nature of sexual and aging processes renders those domains intrinsically cultural and demands that we investigate—as many contributions here do—not just the natural underpinnings of social institutions, but the meaningful intervention of symbolic systems into the nature of differentiation by age and sex as a prerequisite to studying their political and economic ramifications.

Postwar Fertility Trends and Differentials in the United States. By Ronald R. Rindfuss and James A. Sweet. New York: Academic Press, 1977. Pp. x+225. $12.50.

Amy Ong Tsui
University of Chicago

Of all modern demographic phenomena, the rapid rise and decline in American postwar fertility which produced the Baby Boom generation has become a self-sustaining enigma to social scientists. Demographers forecast neither the dramatic climb in birthrates after World War II nor the more dramatic curtailment of childbearing to levels below replacement that followed after only a decade of increase. The impact of relative cohort size on the allocation of resources and privileges will be with us for many years; and the sociohistorical development of the Baby Boom, unique by virtue of sheer size, will create interesting patterns of social change in the ensuing intercohort transfers of rights and rewards. Many of these disruptions are already familiar themes—declining enrollment in higher education, a deflated labor market, postponed marriage and childbearing, depleting social security funds, and so on.

The antecedents and consequences of this demographic bulge are intriguing. In *Postwar Fertility Trends and Differentials in the United States,* Ronald Rindfuss and James Sweet have chosen this distinctive period, 1945–69, to examine group differences in fertility. They emphasize that their task is to describe reproductive trends and differentials rather

than to explain the basic patterns. With public use samples from the 1960 and 1970 censuses, they apply the Cho-Grabill "own children" technique to arrive at annual estimates of total fertility for the nation and for racial and educational subgroups. The larger virtue of this study, in its extension beyond L. J. Cho, W. H. Grabill, and D. J. Bogue's *Differential Current Fertility in the United States* (Chicago: Community and Family Study Center, 1970), is the extensive microlevel multivariate analysis of fertility differences made possible with individual census information. The dependent variable here is the number of children under age three born to currently married women under age 40, a surrogate measure of recent marital fertility. Novel material is also found in the chapters about fertility trends and differentials both among minority groups and between urban and rural sectors, and about migration and fertility in Puerto Rico (chaps. 5–8). The differentials of interest include education, husband's occupation, husband's income, region, and metropolitan area of residence. The first of two principal findings is that almost all social and economic groups experienced a substantial rise in fertility following World War II and continuing through the 1950s, and then a substantial decline in fertility in the 1960s. Second, the social and economic differentials in cross-sectional fertility levels have not converged significantly as expected over the censal decade.

The monograph accomplishes its objectives admirably, clearly detailing the expected indirect relation of education to fertility, the positive effect of husband's income, the impressive decline in marital fertility among minority groups. But the total research product for this particularly dynamic period of American demographics seems incomplete. In my opinion, in spite of a stated intention to be descriptive, the study is sufficiently multivariate in nature to allow a stronger interest in causal effects. It may be that the pervasive trend of a universal pattern in postwar birthrates prevented a more lively investigation of the temporal shifts in social and economic composition and their effects on fertility. However, the authors seem complacently determined to avoid grappling with some of the major determinants of fertility, such as women's labor force status, family income, and changing marriage patterns. Neither the impact of current poverty status nor that of female employment is explored; the authors cite the limits of census data in discounting the value of such efforts.

While census data will not permit an extensive exploration of complex associations, there have been innovative applications of census statistics to produce socially powerful research (e.g., Paul Glick on the family, Frances Kobrin on the household, Philip Hauser on labor force utilization). Since a dominant share of the causal forces behind postwar birthrates has been attributed to changing economic conditions and the labor force participation of women, the constrained analyses here do not approach the substance of Richard Easterlin's or Gary Becker's work on modern fertility. Rindfuss and Sweet themselves correctly suggest that the "explanations needed must be of a broad and historical nature. 'Broad' here refers

to factors that cut across boundaries of the social structure, and 'historical' implies explanations that refer to specific events or series of events. Such explanations are not uncommon in the literature; examples include the relative prosperity of the 1950s, the introduction of the pill, or the growing impact of the women's movement" (p. 192).

As a result, this study raises more questions about the pervasiveness of the fertility pattern than it wishes to answer. What conditions created the rise and fall in fertility, and could it happen again? The findings provoke deeper considerations of the social consequences of the American Baby Boom and Easterlin's predictions of a future boom ("The American Baby Boom in Historical Perspective," *American Economic Review,* vol. 51 [1961]). Beyond the selective treatment of causal issues in postwar fertility, there is disappointingly little sociological interpretation accompanying each set of analyses. For example, minority status differentials in fertility support both assimilationist models of converging fertility behavior and models of independent effects. The authors do not pursue a theoretical resolution of these ambiguous findings but instead suggest the need for a more specifically defined framework of effects.

It may be that these criticisms should be levied at the objectives of the research. Not to go beyond portraying trends and differentials to seek insight on the substantive changes that led to the postwar fertility pattern seems to be an opportunity lost. The systematic design of the analysis renders its presentation easily understood by a nondemographer; the investigation of differentials is as thorough as one will find in the literature. But the impassive stance toward the origins of social change expressed in the absence of trend variations and persistent social differences is what places this monograph in a position decidedly more exciting than a census publication but short of being a provocative treatise.

Our Way: Family, Parish, and Neighborhood in a Polish-American Community. By Paul Wrobel. Notre Dame, Ind.: University of Notre Dame Press, 1979. Pp. xv+192. $12.95.

Joseph Galaskiewicz
University of Minnesota

After all that Paul Wrobel has been through with this research, it is difficult to give his monograph a harsh review. He has been quoted out of context by newspapers across the country, chastised by middle-class Polish Americans, and harassed by TV news teams. One has to be sympathetic. If it has not happened to you, at least it has happened to a close colleague.

In *Our Way* Wrobel presents a traditional community study of a Detroit Polish working-class neighborhood. Like others in this tradition, he lived in the area for an extended period of time (over four years) and spent

a year and a half collecting data. His goal was to describe the social organization of this ethnic neighborhood using a basic field methodology.

His main thesis is that there exists a distinct Polish-American culture. His choice of field setting suggests that it is found in its purest form in the working-class ethnic community. His discussion focuses on three institutions that are central to the social organization of the community: the family, the parish church, and the neighborhood. He criticizes earlier works on Polish Americans, including that of Thomas and Znaniecki, on the basis that they failed to recognize that the culture of "hyphenated Americans" is shaped by the current position of the group in the host society, as well as by the folkways and mores of the country of origin.

The account presented in this book suggests that the position of Polish Americans in the class structure of American society has an especially important effect on their attitudes and behavior. Ethnicity or, more exactly, ethnic religion is important to the culture but only as it helps integrate individuals into community networks and provide a personal code of ethics. It plays no role in defining economic or political interests. Wrobel alludes to the importance of social class, but, in my opinion, he does not explore it enough. For example, much of the controversy surrounding the book has focused on the poor self-image that people in this neighborhood supposedly had of themselves. Many commentators quoted Wrobel's discussion of how parents lived rather austere lives and had an intense desire for their children to go to college, get a professional degree, and live in the suburbs. From this they inferred that these people not only were dull but had a poor self-image as well. Wrobel should have argued more strongly that these people's hopes for their children were not due to the fact that their forefathers came from Poland but rather to the fact that they are working-class Americans who have been victimized by the American dream. I agree with Wrobel that there is a Polish-American culture, at least in some working-class neighborhoods. I am just emphasizing that the "American" part of this culture, which has been primarily urban and working class, should have been given even more attention.

Finally, I would like to quote Geno Baroni, who writes in the foreword: "I have always had problems reading books written by social scientists. . . . The books seemed far too complicated and the scholarship too burdensome to reveal anything appealing about the subject matter. . . . Wrobel's success has to do with his own erudition, but also his method. He doesn't write as a detached observer. Lord, save us from that kind of social scientist" (p. xiii). I probably should not have quoted this in such a brief book review, but Fr. Baroni has identified what to him is the strength of the book and to me is an important shortcoming. After reading the epilogue, in which Wrobel describes his problems with the press, one is not surprised that he takes it upon himself to champion the life-style of the Detroit Polish working class. It is always refreshing to have an author's values up front, but they should not get in the way of good scholarship.

Wrobel occasionally makes empirical statements that his methods just do not allow him to make, and we can only suppose that his desire to

say something meaningful about Polish Americans leads him to do so. For example, he "surveyed" 20 married couples and asked about self-image, friendship, gossip, birth control, aspirations in marriage, and several other matters. The respondents made a number of interesting observations, but the author carelessly generalizes that these observations are characteristic of all Polish Americans in the community. You just cannot make such statements when the couples you interviewed were the parents of the seventh-graders you taught in the neighborhood parochial school. In an equally careless manner, he makes generalizations about "steering" in racially changing neighborhoods based on two visits to local realtors and a newspaper article (pp. 130–31), about the role of women's organizations in linking families in a common information network without a systematic sample of friendship choices (p. 57), and about the role of the pastor in linking the neighborhood to the larger society without any evidence whatsoever (p. 88). Lord, save us from *this* kind of social scientist.

The definitive work on the American white ethnic is still to come. In all fairness to Wrobel, I do not think that he ever thought he had it here. I hope that whoever does write it is as sensitive to the uniqueness of a Polish-American culture as Wrobel was, yet more conscientious and respectful of social science methodology.

Mishpokhe: A Study of New York City Jewish Family Clubs. By William E. Mitchell. New York: Mouton Publishers, 1978. Pp. 262. $19.95.

Richard Benkin
Illinois Department of Mental Health and Developmental Disabilities

Jews have always fascinated Western scholars and writers, who have devoted volume upon volume of sociological, historical, and other studies to them. The output certainly outweighs the Jews' meager 0.34% of the total world population, yet more pieces are constantly appearing. Just when a reader is near the point of oversaturation, another work appears that indicates the fertile field still left to furrow. William E. Mitchell's *Mishpokhe* is one such work.

Mishpokhe can be fruitfully understood from various perspectives. The author proposes certain theories about kinship ties in urban society (e.g., that industrialization has given rise to new kinds of descent groups, evolving from traditional forms [pp. 196–97]). One can also abstract certain data concerning the evolution of organizational forms, especially within the communal setting. Whereas kinship theories are reserved for a special chapter toward the end of the book, most of *Mishpokhe* unfolds as Mitchell attends numerous family club events, interviews informants, and analyzes secondary sources. The author captures the emotional component of Jewish ethnicity, too often ignored by scholars. His field notes are replete with aspects of *Yiddishkeit* that bind the community together in lieu of formal,

religiocultural mores; we also find an explanation for the persistence of attachment, even after external similarities (economic, residential, etc.) have faded. Readers obtain a rarely glimpsed view of Jewish life from an ethnic outsider. The picture is one of a conflictful ethnicity and of ethnics genuinely torn between comforting traditions and the seductive rewards offered by the urban-industrial society.

The Jewish family clubs—family circles and cousins' clubs—are highly unusual forms of kinship organization erected by American Jews of East European descent. (While "Old American" Protestant family reunions are similar, they differ from the Jewish family clubs in their loose organization and episodic nature [p. 39].) Changes in the clubs since the turn of the century parallel a transformation of the Jewish ethnic community, as Jews have flung themselves avidly into the urban-industrial social structure in a way no other American ethnics have. Immigrating from an environment that excluded them rigidly from all socially productive and lucrative enterprises, Jews responded vigorously to the comparative openness of American society. Their response included an extensive network of communal organizations that aided individuals in gaining a toehold in the socioeconomic system by offering interest-free loans, fellowship, and emotional support. The early family clubs conferred similar benefits while promoting family solidarity; thus they served the needs of an immigrant group with a sizable lower class.

As members of the Jewish community became more solidly entrenched in the middle and upper-middle class, family clubs and community organizations abandoned such functions. The larger American society now satisfied most Jews' economic and fellowship needs, making the older family clubs (that is, family circles) seem out of step with the demands of their environment, yet the idea of "family feeling" still had strong appeal. Thus, Mitchell tells us, arose the cousins' clubs. Like contemporary Jewish organizations, cousins' clubs are age segregated (unlike family circles) and emphasize entertainment rather than "social service" or Jewishness. Moreover, cousins' clubs are less formal than family circles and require less commitment in time and personal investment; and form, not "Jewish" content, is primary. Yet they help contemporary Jews resolve a host of dilemmas inherent in their life in the urban American society.

Jews have been characterized as family oriented yet at the same time socially mobile. This presents a problem for individuals whose careers are at variance with those of other family members; commonalities, and therefore family interaction, would thus be at a minimum. As a formal organization, however, the cousins' club structures periods of limited interaction which it defines as sufficient to qualify as closeness among cousins. The family club also provides a context for certain trends that disturb many Jews (e.g., the decline of Yiddish and of several religious traditions). Mitchell demonstrates that when conflict arises over such issues, the clubs almost invariably opt for family solidarity over "fond traditions" (p. 118). His point is that Jewish ethnicity is constantly changing and that the

family club has the flexibility to provide a stable arena for family/ethnic interaction.

Some conspicuous flaws mar *Mishpokhe*'s quality at points. The author's unfamiliarity with Jewish history is glaring in his review of Jewish life in eastern Europe as a prelude to American-Jewish community life. We read, for example, that "until the first partition of Poland, Polish Jews for the most part were unmolested" (p. 30). This is a terrible inaccuracy, as any history of the era will bear out. Nor is he familiar with pertinent sources; for instance, he documents a Jewish penchant for creating voluntary organizations "today" with sources no more recent than 1961 (p. 172). The book suffers also from the author's use of the male pronoun as the generic, because it is clear that the familial and religious relationships treated involve a substantial proportion of females.

Mitchell has accomplished a great deal in penetrating the heretofore remote world of Jewish family clubs—a feat all the more impressive in that he is not Jewish. The only other major study of these clubs was a 1939 WPA-sponsored work, written in Yiddish. And Mitchell has been able to capture the subtlety and nuance of intimate interaction while offering broader sociological theories at the same time.

Neighbors in Conflict: The Irish, Germans, Jews, and Italians of New York City, 1929–1941. By Ronald H. Bayor. Baltimore: Johns Hopkins University Press, 1978. Pp. xiv+232. $14.00.

Peter I. Rose
Smith College

In the opening pages of their landmark study, *Beyond the Melting Pot,* Nathan Glazer and Daniel Patrick Moynihan reported their conclusion that "the melting pot never really happened, at least not in New York and, *mutatis mutandis,* in those cities which resembled New York." *Neighbors in Conflict* adds substantial historical evidence to support the claim. Ronald Bayor's objective was "to analyze the roots, development, manifestation, and culmination of [interethnic] conflicts within a multi-ethnic urban setting."

Bayor offers first a useful review of New York's ethnic history, with a primary focus on the 1930s, the period of the greatest amount of interethnic (i.e., white ethnic) competition. Then, in a series of interlocking essays, the author sets the stage; discusses the boom of the 1920s and the bust that followed; describes the key role the half-Jewish, half-Italian, Protestant mayor Fiorello La Guardia played in the "New Ethnic Order"; considers the effects of nazism and fascism in Europe on German and Italian Americans and the consequences of red-baiting and Jew-hating especially among the Irish; looks at how members of each group felt about the war; and examines the politics of bloc power and the intense rivalries

in two arenas of Irish-Jewish conflict, Washington Heights and the South Bronx.

Reading these essays, it becomes apparent that, while there were collisions between all of the groups that existed, for a variety of reasons the Jews, who were to become the most mobile and the most powerful, bore the brunt of the others' antipathy. Anti-Semitism is a leitmotiv that weaves its ways through the pages of Bayor's book.

According to the author, in the mid-1800s, the Irish were the principal target for the hostility of the Protestant nativists who resented their intrusion and feared what their apparent allegiance to the papacy might portend. The Irish suffered discrimination and had to struggle to obtain a meaningful place in the body politic. By the last decade of the century they had established themselves as a significant community and a potent political force. New immigrants, including several million Russian Jews and Southern Italians, took their place at the bottom of the economic ladder, moving into neighborhoods still inhabited by some Germans and many Irish. Soon new rivalries emerged.

Bayor points out that from 1910 to 1930 the trend in New York was away from unskilled labor. By the time of the Great Depression such workers were the hardest hit. Chief among them were the Italians. Predominantly of peasant background, they had become—and many remained —common laborers (though some were to become barbers and cobblers and garment workers). The Jews, in contrast, were already skilled or semiskilled and found employment as furriers, tailors, dressmakers and seamstresses, peddlers, cigar makers, and merchants. They differed in other ways. The Jews considered education a main path to social and economic well-being. The Italians were wary of the public schools, which many felt threatened to subvert traditional values and took time away from productive labor. Although there was relatively little overt conflict between the Italians and the Jews, the former experienced some resentment and envy of the latter's success, especially in the needle trade where Italians frequently worked in Jewish-owned factories or as members of unions controlled by Jews.

But it was the Irish who most resented the Jews, particularly Jews of the second generation who challenged their hard-won control of the political machine, the teaching profession, and the civil service. Many Irish-American spokesmen suggested that the real problem in New York was not anti-Semitism but "anti-Christianity." Bayor illustrates the point with a sardonic quote from Father Charles Coughlin's newspaper *Social Justice.* Claiming that the Jews were setting unfair standards for civil service jobs, it was suggested that "a man must be up in his Greek, mathematics, zoology, astronomy, and Hebrew before he can be a good cop" (quoted on p. 28). Many other published statements were far less subtle in their anti-Semitism. The charges were to become more frequent during the long period of La Guardia's control of City Hall, a time when the hegemony of the Irish machine had diminished and many Jews gained both appointive and elective office.

In one of the most interesting chapters in the book, Bayor shows that the Irish were not the only group that felt threatened by the new coalition. The Germans did too. They had more or less removed themselves from politics in the aftermath of World War I when they were subjected to considerable ostracism and discrimination. With the rise of Nazism, many feared a revival of anti-German sentiments among old Americans. Others, identifying with the "Fatherland," gave credence to the fears of many, not least the Jews. In fact, the Bundists did make some inroads into the German-American community. Even though Bayor suggests that that community "never saw [the Bund] as an American organization—a fact which kept membership low" (p. 63), few German-Americans, including those active in the Steuben Society, spoke out against anti-Semitic policies until very late in the decade, after *Kristallnacht*.

"Although Germany presented a problem for Jewish-German relations, it had little effect on the Irish or Italians" (p. 76). For different reasons many members of both groups were not unsympathetic to German Nazism in the mid-1940s. Bayor reminds the readers of the agreements between Mussolini and the Pope in 1929 and the Concordat between Germany and the Vatican in 1933.

Still, while the influential Italian-American press had long supported the Fascist cause and many Italian Americans were sympathetic to Italy's new regime, *Il Progresso* and other papers spoke out in opposition to anti-Semitic practices, pointing to the security of the Jews in Mussolini's Italy. Here they parted company with the right-wing Irish publications and with publicists like Father Coughlin. Italian Americans remained deeply divided over the issue of fascism but, according to Bayor's research, were quite united in their rejection of Nuremberg-like regulations adopted by the Italian government in 1938 and seemed eager to maintain good relations with Jews.

For the Irish, the "ism" that represented the menace was not nazism or fascism but communism. The Jews were identified as the main supporters of that atheistic anathema. The vast majority of Jews were not Communists, but the fact that many of the Communists in New York were Jews lent credence to the charges. Moreover, to Coughlin and his followers, Jews were not only Communist conspirators but the quintessential plutocrats. In 1938 the phony "Protocols of the Elders of Zion" was published in *Social Justice*. The culmination of the coalesced anti-Jewish sentiment occurred with the birth of the Christian Front.

Neighbors in Conflict ends with a brief and incisive concluding chapter in which the author, a historian, offers some important sociological observations about the nature of interethnic conflicts based on economic rivalries, differing cultural values, attempts to achieve and maintain political control, external influences, and, not least, misconceptions of others. Bayor suggests that changes can occur and points out that they have. (One need only note the principal sources of support for the likes of such Irish pols as Robert Kennedy, Daniel Patrick Moynihan, and Hugh Carey in

recent years or of the new conservative coalitions in the 1960s which included Irish, Italians, and Jews.)

All told, this is an important book that provides sociologists with what we so often lack: sound historical analyses of problems that concern us and serve as a baseline against which we consider those contemporary issues that preoccupy us so much of the time.

1943: The Victory That Never Was. By John Grigg. New York: Hill & Wang, 1980. Pp. 254. $12.50.

Morris Janowitz
University of Chicago

Given the vast expansion of the social science enterprise during the past three decades, the "modal" sociologist complains persistently that he is overburdened in his efforts to keep abreast of the research literature produced by his sociological colleagues on subjects that interest him. If he wants to maintain an interdisciplinary stance, the task of following the relevant literature is almost impossible. But the problem is even broader, for "modal" sociologists should also be reading and digesting the relevant literature produced by perceptive authors who are not social scientists or sociologists, but which is effectively sociological without consciously or explicitly using sociology. The important literature of this genre is produced by journalists and free lance writers and some novelists.

But the sociological profession has no mechanisms for identifying the appropriate titles and informing colleagues. It is indeed rare that a sociological journal reviews such writings. Randall Collins's review in the *American Journal of Sociology* (84 [July 1979]: 190–94) of Martin Mayer's *The Bankers* is a refreshing and noteworthy exception. In effect, the modal sociologist cannot expect the editors of sociological journals to monitor the flow of trade books regularly for the limited number of relevant titles. Sometimes the initiative must come from the journal readers themselves.

In my view, John Grigg's *1943: The Victory That Never Was* is a splendid example of this genre. It is one of the most trenchant descriptive and compact accounts of the formulation of Allied global political and military strategy during the Second World War. I recommend it as high priority reading for sociologists who have come of age in the Vietnam period and thereafter and are interested in an overall account of that earlier military confrontation. In addition, it is a brilliant analysis of and argument about the factors which fashioned both the military strategy and its shortcomings. In other words, it has direct importance for the sociologist interested in decision making in large-scale (immense, I would say) organizations.

The author focuses on the very top leaders: a dozen men and their

immediate entourages. He has available a mass of documentation in the form of archival materials (most relevant archives have been opened), an extensive collection of secondary studies, and, especially important, the extensive biographical and autobiographical accounts by the key decision makers and by members of their staffs. In fact, it appears to me that the documentation is at least as detailed and comprehensive as that used in the typical local community study dealing with decision making in neighborhood institutions.

The basic problematic issue—that which Grigg seeks to explain—can be stated as follows: Both Churchill and Roosevelt, in their own right and reflecting the political coalitions which they represented, believed that the Nazis had to be defeated before the Japanese. They were firm in their conviction that the defeat of the Germans required a second front in northwest France. Moreover, the earlier the Nazis were defeated the less would be the human misery and the more satisfactory would be the ensuing political settlement and reconstruction. (This was hardly an anti-Russian objective since the Russians were demanding a second front in 1942 to relieve the German pressure on the Eastern Front.) Although Roosevelt and Churchill doubted that the Second Front could be launched in 1942, they were committed to a northwest European offensive in 1943 and a strategic European victory in 1943. Certainly, if the Second Front were postponed to 1944, the Allied forces would clearly be in a position to achieve victory during that year. Roosevelt and Churchill were in constant communication and had repeated and long face-to-face meetings which included joint meetings with the top military leaders of both nations. However, although the Axis powers were thoroughly defeated, these two men achieved none of the strategic goals mentioned above. Grigg argues that their primary strategic goal—defeat of the Nazis in 1943—was feasible. Instead, though, the war in the Far East obtained higher priority than was anticipated or planned for. In addition, in Europe more of the military resources were allocated to the North African and Italian fronts than was warranted, given the plan that the major thrust would be in northwestern Europe. Moreover, the delay in opening the Second Front meant an additional year during which the Germans could improve the Atlantic wall, and a year in which the Nazis could eliminate vast numbers—millions of Jews and East Europeans—and make the final victory more costly in terms of human lives. One must include the deaths of these civilians as part of the true total of human destruction.

In essence, we are dealing with a typical process by which top leadership goals are drastically modified by the persistent elements in an organization. Since Grigg does not find it necessary to engage in fierce and excessive sociological reductionism, his analysis rests not only on institutional and organizational factors but on the consequences of personalities as well. Discussion of Churchill makes a fascinating contribution. His commitment to a Second Front in 1943 was explicit and represented the well-known and tough political leader with an ability to face the blood-and-guts issues and drive ahead ruthlessly. But part of him, less visible,

had doubts about the morale and effectiveness of the British soldier as exemplified by the limited resistance offered at Singapore and Tobruk. This configuration restrained him from dealing effectively with officers who resisted the agreed-on military strategy. Roosevelt was more direct in pressing for the implementation of the northwest European strategy, but his lack of patience, concern, and aptitude for administrative and organizational problems was immense and highly counterproductive with respect to his desired ends.

Grigg's analysis of the organizational factors is subtle and "ruthless" in facing the facts and in exposing the extreme weakness in the Allied efforts; I can indicate only the main elements. First, the capacity of the component services and branches in the military to subvert directives and to press for short-term self-interest expansion borders on the incredible. The main British offender was the RAF with its nighttime area attacks which consumed immense resources and had most limited effects. To involve the RAF in direct support of ground combat was itself a large-scale battle. Likewise, the Far Eastern theater got more than its allocated resources because the U.S. Navy believed that its long-term future depended on the record it could achieve in the sea battle against the Japanese. Its efforts were highly conspicuous compared with the patrolling and ground-support tasks in the European theater.

Second, the professional orientations of top military strategists and of the operational commanders operated as a strong barrier. In particular, both nations had too few generals who thought in terms of maneuver, rapid movement, encirclement, and breakthrough. Most accepted the approach of attrition.

Third, the piecemeal decision-making process employed by the planners of both nations (especially by the British) meant that short-range decisions limited and undermined the ability to fight in terms of the grand strategy. The decision to land in northern Africa was the first in a series of steps which decreased the ability of the Allies to land in Normandy in 1943. Griggs points out that General George Marshall saw this trend from the very start and objected, but being a "good soldier" he failed to object with adequate force. In short, the general category of goal displacement worked in this momentous enterprise as it does in small-scale enterprise: institutional self-interest, professional and skill perspectives, and the internal dynamics of decision making on a piecemeal basis are the relevant variables.

The author proceeds in a systematic fashion, balancing successes against failures in the decision-making process. In fact, he resorts to the counterfactual argument repeatedly and offers hypothetical alternatives which clarify the analysis. It is not a superficial argument based on hindsight but a closely reasoned statement of what had to be done to achieve the ends that Roosevelt and Churchill sought. The book demonstrates that the sociological inquiry works and that one does not have to be a sociologist to produce relevant sociological analysis. Grigg builds his conclusion that victory in Europe would have been possible in 1943 by ex-

amining all aspects of the Grand Alliance, including the political warfare and the propaganda dimension. I disagree only with his discussion of "Unconditional Surrender." He sees only negative consequences in that formula. In my view the importance of the "Unconditional Surrender" formula is overstated, and it had the advantage of speeding up the final phases of the military battle by eliminating the hope of a political settlement worth last-ditch resistance. On the other hand, Grigg's analysis of the political and psychological consequences of the air warfare and its limited military usefulness, together with his moral evaluation, is one of the strongest parts of the study. In particular, he documents the failure of the British air warfare effort. Not only was it ineffective and costly to the British and the combined effort, but from his point of view it was morally unjustified. This is the judgment of a British citizen who writes, on page 150, "So far as bombing was concerned, Britain far exceeded the worst excesses of the Germans. . . ."

The power of the book, from a sociological point of view, is that it makes a vast and complex organizational enterprise understandable without oversimplification. It demonstrates that conflict and the management of conflict can be put in a causal perspective and need not be limited to small and localistic struggles such as youth gangs.

In the 1960s sociology was caught unprepared and unaware about the emergence of student conflict; the discipline still remains relatively out of touch with the mass movements of contemporary international conflict. I am frequently asked by other social scientists and interested persons involved in public affairs whether the ideological block of contemporary sociologists against the study of war and international conflict has weakened if not passed away. Unfortunately, while some progress has been made, indifference and hostility remain very powerful. Of course, I do not think the frontier to be the study of military organization as a discrete object of analysis—although we need such work. I refer to the total neglect of the role of the military and militarism in a wide range of social institutions and societal change. How can one write about changing race relations in the United States without reference to the role of the all-volunteer military? In the lower ranks of the enlisted personnel we have created a new pattern of social stratification in which black personnel have more education and higher "mental" scores than the comparable white personnel. Or how can one write about states and revolutions by merely stating that the armed forces are key elements and not pressing the analysis further than we have to date? There is a rich sociological literature about armed forces and society and about war making and conflict management, but it is not being written, read, or acknowledged by modal sociologists in the United States.

State and Class: A Sociology of International Affairs. By Ralph Pettman. New York: St. Martin's Press, 1979. Pp. 270. $25.00.

David Allan Mayers
University of California, Santa Cruz

Traditionally the formal study of world affairs has been the preserve of both diplomatic historians and political scientists. They have concerned themselves principally with such conceptual problems as the nature of war, the foreign national interest, and the mysteries and nuances of the balance of power. Typically these scholars—William Langer, Hans Morgenthau, for example—have focused attention on the diplomatic-military interactions among states and have stressed the essentially competitive character of international politics. Recently, however, since the so-called behavioralist revolution, a number of scholars—Morton Kaplan and Kenneth Boulding, for example—have tried to broaden and enrich the study of international affairs by employing the findings and methods of rigorous social science. This school tends to emphasize the self-regulating, predictable processes of the international system, itself composed of diverse, interdependent state "organisms." Finally, quite recently, a third set of scholars, primarily interested in theories of economic development, have emerged as serious students of international relations. In particular, Immanuel Wallerstein, largely beholden to Marx and Marxist intellectual traditions, has promoted widespread interest in the political economy of international relations. Yet regrettably, comments Ralph Pettman, the study of world affairs has not benefited much from the concepts and theories developed by sociologists and successfully used by them. In order to correct this imbalance—to demonstrate the applicability of sociological method to the study of global politics—Pettman has written *State and Class.*

He posits an emergent world society that is assaulted by rapid, profound economic and political changes. And he contends that sociology, as an academic discipline, is especially well endowed to understand the "compulsive forces" now transforming the structure of international society. Moreover, Pettman is keenly interested in issues of epistemology and the sociology of knowledge. He poses the following two questions: What are world affairs that we may know them? What are "we" that we may know world affairs? (p. 14). Such issues are rarely raised explicitly by writers on international politics; in raising them Pettman sets himself apart from most other analysts.

The book is divided into three major sections. In the first one, "Social Consciousness," Pettman attempts to answer related, difficult questions. What is the structure of global society and what are its distinguishing features? What are the values and norms professed by individual actors—countries, international institutions, prominent political leaders, etc.—in the world society? How have the social forces associated with modernization and industrialization manifested themselves in world political culture?

Pettman believes that the answer one offers to these questions depends heavily on whether one adheres to a predominantly pluralist theoretical construct or to one mainly structuralist. However, strict adherence to either paradigm, suggests Pettman, is limiting. He holds instead that "the most useful image of world affairs will only emerge from a composite discussion of the way they [structuralist and pluralist approaches] interrelate: the compromises that have been effected between them and their manifold contradictions" (p. 65).

In the second part of the book, "Social Structure," the author first examines the emergence and universality of the nation-state. Then he launches an inquiry into the inequality of countries and explains the class antagonism—exemplified by North-South recriminations—which disturbs contemporary international politics.

The third section, "Social Ordering and Social Change," depicts the global impact of industrialization, urbanization, and bureaucratization. In this last section Pettman looks also at the role and nature of international law and world conflict. His predictions for the latter are gloomy indeed.

As the foregoing synopsis indicates, *State and Class* is a very broad, ambitious work. Some readers will surely be frustrated by the general and sometimes hasty treatment of various topics. Moreover the author is, at times, somewhat incautious. At one point, for example, he talks rather too easily, too vaguely about the imminent fall of entire (unspecified) populations to totalitarian control (p. 110). And his discussion of the seemingly inexorable progress of industrial culture around the world largely ignores theories incompatible with his own view—thus his analysis is less well balanced, less persuasive than it might otherwise be. His perspective is essentially Europocentric, and he does not question seriously the veracity of teleological theories that claim all human societies are moving along the same path and will eventually realize a similar degree of economic, political, and technological uniformity. Pettman neglects arguments—put forth by Leonard Binder, for example—that a multiplicity of starting points, paths, and outcomes may exist for different human societies. Value-laden comparisons among them, according to Binder, usually fail to recognize that equally valid—though varied—forms of social development take place. For some social scientists, then, neither normative nor scientific reasons exist for supposing that European styles of modernization will or should be the models for Third World development—a point which Pettman does not address adequately.

One of the most interesting sections of the book deals with the advent of global norms and values, such as human rights. Pettman's discussion of human rights, though it proposes nothing new, is both sophisticated and skillful. Ably he contrasts the liberal version of human rights—stressing freedoms of the press, speech, worship, assembly—with the socialist emphasis on economic rights, such as guarantees of work, income, food, and shelter. Both ideological systems together, argues Pettman, form the basis for an emerging set of universally acknowledged political values. Unfortunately the evidence Pettman marshals to support this thesis is

scanty, and he gives too much credence, I believe, to the rhetoric of governments as they solemnly proclaim their devotion to United Nations declarations on peace, justice, human rights. International public posturing by political leaders comes cheap. Yet the sad fact is that the principle of force in world affairs forms its own realm, with its own distinct laws, themselves usually impervious to the appeals of higher morality. Let us hope, though, that Pettman is right, that the crude conduct and rough manner of world politics is being displaced by a refined, coherent, new system of beliefs and behavior. However, I remain skeptical and wish that Pettman had grappled more directly with the issue of power politics.

Prejudice and the Old Politics: The Presidential Election of 1928. By Allan J. Lichtman. Chapel Hill: University of North Carolina Press, 1979. Pp. xiii+366. $20.00.

John R. Petrocik
University of California, Los Angeles

Al Smith lost the election of 1928 because he was a Catholic and because his Catholicism made it possible for the Republicans to stimulate religious and cultural hostilities of many kinds. That, in brief, is the first thesis of *Prejudice and the Old Politics*. While Allan J. Lichtman concedes that Hoover might have defeated any conceivable Democratic opponent, the religious dimension of the election is important because it allows Lichtman to construct a second and more important argument: the Smith-Hoover contest was, not the first clash of the New Deal realignment, but the last reprise of a party system which structured electoral conflict around moralistic themes.

The conclusion that religion provided the centerpiece of the election is based on a six-chapter analysis of the correlates of the Smith vote. Using census and election data for 2,100 counties, Lichtman finds the religious composition of the counties more highly correlated with the vote for Smith than was support for Prohibition, the proportion of immigrants in the counties, urbanization of the counties, their racial composition, or their relative wealth. Many of these other variables were correlated with the vote, of course, but none so strongly or consistently as religion. A Republican survey of local leaders and various private papers and letters corroborate Lichtman's conclusion about the overwhelming importance of religion in the voting decision in 1928.

The election, then, was dominated by a short-term force: Smith's religion. Because religion played such an important role in the 1928 contest and because it was specific to that election, Lichtman feels justified in arguing that the contest was something of an aberration, an egregious manifestation of the moralistic and segmental politics that characterized elections from the late 19th century to the New Deal era. In Lichtman's

data, the correlation of the 1928 results with prior and subsequent elections is so poor that there seems to be no reason to see it as anything other than an unfortunate (for the Democrats) perturbation.

If Lichtman's analysis stands, it raises difficulties for critical portions of the standard explanation of the rise of the New Deal party system. His most troubling data are those which show no substantial increase in the Democratic electorate in the major cities. It has been something of an orthodoxy among students of the New Deal that Smith mobilized the urban Catholic population for Roosevelt and the Democratic party. Lichtman's data do not show this mobilization.

While his conclusions must be addressed by subsequent analyses of the 1928 election, *Prejudice and the Old Politics* has several demonstrable flaws. One example is the attempt to refute the "urban revolt" thesis. The standard interpretation of the "revolt of the city" is that the nomination of Al Smith extended recognition to Catholics, who responded by voting for him and, over the longer term, the Democrats. Lichtman does not find this shift to Smith and the Democrats, probably because in examining the effect of the urban-rural measure he controlled for the religious-immigrant-etc. composition of the counties. That is, he held constant the social characteristics that provided the dynamic process of the change. The question, therefore, is whether his data undermine the "revolt thesis" or whether he destroyed its operational meaning in his analysis. I suspect the latter. Furthermore, he misreads some of the data which lend credence to the "revolt" thesis (especially the Andersen data), and he pointedly fails to indicate why his analysis should be accepted and what is wrong with the existing material on the subject.

Other pieces of the analysis also have peculiarities. Some tables have as many as 14 measures of the 1928 vote, and it is almost never clear what piece of social reality each is supposed to be capturing. Generally Lichtman uses the different ways of counting the Smith vote to test the efficacy of his independent variables: correlations which are similar across all of the vote measures, for example, the relationship of the Smith vote to religion, are taken seriously; independent variables whose correlation with the vote is sensitive to the way the Smith vote is measured are treated as less important, for example, the coefficients for the Smith vote and Prohibition. There are other interpretations of this variation. Many students of party change see substantive significance in a relationship which is strong or weak depending upon whether the Smith vote is treated as (1) a percentage of turnout, (2) a percentage of the eligible electorate, or (3) a residual Smith vote which has been reduced by the mean Democratic vote between 1916 and 1924. Depending on how the different vote measures correlate with the independent variables, the variation could be used as evidence for the electoral mobilization that Lichtman did not find. Also, portions of the analysis are infused with conjecture and innuendo. Especially offensive is his campaign against Hoover. Lichtman insists that Hoover played a role in orchestrating appeals to the religious bigotry of voters. As "proof" of Hoover's responsibility, he repeats the claims of

others; often he simply observes that Hoover's knowledge of and support for campaign tactics that stimulated anti-Catholic prejudice is "plausible" or "likely." It is possible that Hoover was more involved in the anti-Catholic portions of the 1928 campaign than many people realize, but Lichtman provides no supporting evidence.

There is also a needlessly polemical and argumentative quality in large portions of the book. The attacks on pluralist theory (chap. 1) and standard analyses of realignment (chap. 9) lack depth. Lichtman presents no serious discussion of problems in the way in which these concepts have been formulated and used to interpret party-system stability and change.

In short, this is a book with which most students of the American party system should be familiar: its thesis must be taken seriously. But *Prejudice and the Old Politics* is not a withering criticism of pluralist interpretations of American society; the data analysis is neither as elegant nor as persuasive as Litchtman might like to believe; and he is not the first person to point up weaknesses in the standard theory of realignment.

The Pursuit of Attention: Power and Individualism in Everyday Life. By Charles Derber. Cambridge, Mass.: Schenkman Publishing Co., 1979. Pp. x+110. $5.95 (paper).

Mary F. Rogers
University of West Florida

Treating attention as a social resource closely connected with individualism as well as power, Charles Derber conjoins interactional and structural considerations in *The Pursuit of Attention.* His effort also incorporates a healthy measure of cultural criticism. As a result, this study offers a refreshingly balanced, strongly grounded exploration of the routines Americans exploit in competing for attention. The author argues convincingly that those routines exhibit many features of market behavior.

Derber's reliance on Erving Goffman's dramaturgy is considerable, though not excessive. In fact, Derber largely fulfills his claim to extend Goffman's model by relating everyday interactions to the class divisions and patriarchy of American society. Having completed field studies aimed specifically at the competition for attention in everyday life, Derber is able both to support some of Goffman's own formulations and to extend some of his key generalizations. Further, Derber's attention to societal factors is explicit and generous. He refuses the temptation that overcomes many students of everyday life, fascination with the peccadilloes and paradoxes of daily life at the expense of careful attention to their relationship with the structure of society.

Yet his analysis, like all adept sociological work, does debunk the phenomenon under consideration. The "conversational narcissism" Derber dislodges is both more invidious and more subtle than even critical observers

probably suppose. His data illustrate the routine subtleties used to produce a lack of responsiveness in daily life. He also shows that trite conversational fillers like "you know" function as requests for acknowledgment of listeners' attention (p. 33). Moreover, "shift-responses," which effectively fulfill commonplace codes of civility but at least temporarily shift attention to oneself, often preface a narcissistic takeover of a conversation. Yet the recipients of such responses cannot immediately determine whether a takeover or a dialogue is imminent (pp. 27–28). Thus the codes that appear to minimize the likelihood of monopolized conversations in fact obscure the shifts that dominant participants typically initiate. In effect, then, ritual restraints favor those persons socialized to be more assertive, confident, expressive, and self-oriented.

The American social structure, resting on patriarchal and class-based distributions of privilege, interplays with everyday rituals to provide an attention-getting edge for most males inside the home. For males in the dominant classes, that structure favors their receiving disproportionate attention outside the home as well. Derber analyzes the female psychology of attention giving that can be inferred from women's conversational patterns and its male counterpart, the psychology of attention getting. Significantly, he suggests that the conditions that invite male attention giving commonly involve female dependence or male sexual interest. Thus our culture supports varieties of male attention giving which underscore male dominance. At the same time it obscures the possibilities for egalitarian sharing of attention between women and men to the extent that attention from males routinely implies the stereotype of the helpless female and the possibility of sexual expectations (pp. 60–61).

Although the distribution of attention among social classes seems nearly to defy description, Derber undercuts that difficulty by focusing on the purchase of attention in American society. In "Attention for Sale: The Hidden Privileges of Class," a chapter coauthored with Yale Magrass, he treats attention as a commodity available in therapy, exclusive restaurants, and select shops. In addition, the display of consumer goods commands attention, if they meet dominant standards of good taste and sophistication and bear a hefty price tag. On the less privileged side, some sectors of the population—welfare recipients, for example—face kinds and amounts of attention that reinforce their positions of relative helplessness and dependence. Some attention giving, then, represents an exercise of power and privilege (p. 73).

Derber's study offers many concrete illustrations of the pursuit and distribution of attention in American society. His concluding chapter forecasts the future of attention-getting practices in a society that remains respectful of narcissistic individualism and protective of sex- and class-based privileges. That chapter resonates with the recent works of Christopher Lasch and Edwin Schur as well as earlier contributions by Philip Rieff and Philip Slater. Nonetheless, the chapter is no regurgitation of others' ideas. Derber couples his incisive projections with specifications of the social practices and structural changes necessary for a reversal of

narcissistic, inegalitarian trends. And he accomplishes that task while masterfully controlling, even subduing, the rhetoric inherent in such undertakings.

In only one major respect does Derber's study limp. A number of points in the text obfuscate the connections between everyday social interactions and the larger social structure they cumulatively constitute. In grappling with those connections Derber uses verbs that confuse the relation of micro- and macrolevel aspects of society. He says, for example, that the two levels are "intricately meshed" (p. 4) and that he favors Erich Fromm's position emphasizing that norms "affect" interaction and character (p. 5). Similarly, interactions are "embedded" in a larger system (p. 6) and "express" macrolevel realities (p. 8). Worse yet, "inequalities of attention *grow out of* the most fundamental forms of social inequality" (p. 64; my italics); they are "shaped by" larger-scale phenomena (p. 88). Recurrently, too, Derber uses a mirror metaphor to describe micro-macro connections. Microlevel phenomena "mirror" or "reflect" macrolevel ones (pp. 5, 41, 88). In the most extreme instance Derber argues that "stratification systems *create* distinctions of social worth . . ." (p. 42; my italics). A more tenable formulation, though, precedes that statement: "The inequalities of face are not simply interesting derivatives of economic and social power, but an integral dimension of their very structure" (pp. 41–42). Yet the difficult problem, central to Derber's undertaking, escapes his most careful attention. In too many instances he nearly implies that an autonomous social structure shapes interactions independently of antecedent as well as current interactions repeated by innumerable individuals throughout society. To that extent Derber's analysis weakens and one of his stated goals remains unfulfilled.

Nonetheless, *The Pursuit of Attention* represents a compendium of theoretically credible, empirically grounded generalizations useful to students of everyday life. Because of its compactness and overall merit, the work also invites attention as a supplementary text in courses on inequality in American society, contemporary theory, and social psychology. Together with the lucidity of Derber's writing, timeliness renders the book likely to attract as well as benefit undergraduate students.

Dispute and Settlement in Rural Turkey. By June Starr. Leiden: E. J. Brill, 1978. Pp. xvi+304. f72.

Sally Falk Moore
University of California, Los Angeles

The core of this book is a description of what June Starr calls "conflict interactions," 104 in number, most of them taking place in one rural Turkish village (p. 260). The incidents described are varied and colorful, but most involve petty arguments ranging from a fistfight between two

young men to a dispute over a small amount of borrowed money. Of these, 26 became full-blown cases of dispute. Starr is concerned with showing the processes that defused some "conflict interactions" and brought some others to a head. This theme is further and more systematically developed in *Dispute and Settlement in Rural Turkey* than in any earlier legal ethnographies. The data were collected in the period 1966–68 when the author did her fieldwork. The village in question was tiny, consisting of 247 households with a total population of 1,014 persons. Mandalinci's farmers engaged in three basic forms of food production: (1) animal husbandry (milch cows, sheep, goats, camels, and donkeys); (2) the cultivation, in unirrigated fields, of vegetables, olives, and figs; and (3) the cultivation of citrus fruit (primarily tangerines) in irrigated orchards. The tangerines were almost exclusively produced as a cash crop, while the beasts and garden produce were used in the domestic economy as well as being sold in the market. Many of the farmers had secondary occupations, and some men, particularly youths, were involved in temporary labor migration.

Starr introduces the reader to the Mandalinci scene through a brief summary of the recent history of the village, a sketch of the local economic structure, and a discussion of the notions of rank and prestige of the village residents. An account of local social and political organization follows. The introductory material serves as the backdrop for the presentation of the "conflict interactions" and "disputes." In each case Starr makes a special note of the place of the disputants in terms of their economic and social situations. She is as much interested in "conflict-managing," "conflict-provoking," and "conflict-avoiding" as she is in the playing out of dispute itself (p. 110). This book is further evidence of the trend in legal anthropology away from a focus on formal organizations and normative rules in the direction of case studies outside formal systems, and on informal techniques of resolution. Legal rules are introduced into Starr's discussion only insofar as they shed light on particular cases. Her focal interest is the social relations of the villagers, and the way in which those relations determine the strategies they adopt either in seeking redress or, in some cases, in preventing the maturation of grievances into full-fledged conflicts.

Although Starr is much more concerned with this intravillage process than with the Turkish state and lawyers' law, she provides many glimpses of the enormous effect of state power. While her book is largely about village social structure and small mundane disputes between individuals, she readily acknowledges that when important property is at stake, "villagers use the formal legal system of the Turkish state" (p. 265). Important property in this sense includes not only fields and orchards and lots, but boats, cars, and houses as well. The force at the command of the bureaucracy outside the village—the inspectors, the gendarmes, and the courts—actually appears in several of Starr's cases. And from her account of the village as a social system, it is clear that the two most prominent men left the village frequently to go to town in order to carry on business as well as to negotiate transactions with government officials.

Clearly the major internal affairs of the village were substantially shaped by the larger society in which it was imbedded. The village was not simply a segment of a larger whole, separate and autonomous within it, but instead deeply penetrated by its political and economic environment. Mindful of this dimension, Starr follows a few cases from the village into the Turkish court system and notes the repeated episodes of litigation that frequently ensued. She contrasts the expectations of villagers with their experience in the courts. "The clearest goal of the judges in each case is to discover legal issues in the dispute. . . . Judges do not probe the total social relations between litigants to find underlying sources of conflict in the ways that have been described for some African and Mexican courts" (p. 274). In Turkey, as in many other countries with a professional bar, judges are concerned with professional norms, so that their concerns inevitably do not entirely coincide with those of litigants.

Starr does not explore the particular implications of the use of the Swiss Civil Code. Like most anthropologists, her heart is in the village. She finds village mechanisms for the resolution of conflict preferable in every way to the courts. "The study of Mandalinci shows . . . that villagers have ways of resolving conflicts that are faster, cheaper, and frequently more effective than recourse to the District Court. . . . National systems of law enforcement may be more professional, but they also are more expensive in terms of time, money, and number of employees, and they frequently give less attention to the individual, his family, and his problems" (pp. 277–78).

By presenting the Turkish villagers' situation and perspective Starr has made a valuable addition to a growing body of literature in legal ethnography. The material on villages will soon be extensive enough so that it will be possible to attempt some comparative generalizations and, one hopes, to add to them the perspective of the various national and provincial administrative centers. The giving of sensitive attention to the interpersonal disputes of individuals tends to be low on the priorities of most modern governments. Despite ideological statements to the contrary, such objectives may not be the primary focus of any formal system of courts, even though villagers and some anthropologists may think they should be. Starr's local study is more than a good example of an established genre; it has raised standards for all who would follow.

American Journal of Sociology

Economic Regulation and the Public Interest: The Federal Trade Commission in Theory and Practice. By Alan Stone. Ithaca, N.Y.: Cornell University Press, 1977. Pp. 314. $19.50.

Donald R. Winkler
University of Southern California

Alan Stone's book is a delight to read. *Economic Regulation and the Public Interest* is a well-written and thoroughly documented analysis and evaluation of the Federal Trade Commission, which many critics, including Stone, view as ineffective in accomplishing its objectives. The volume can be viewed as an attempt to answer three principal questions: (1) Under what legislative guidelines and constraints has the FTC operated? (2) How have those guidelines influenced the activities and perceived effectiveness of the FTC? (3) How does the FTC enforce the legislation under which it operates? In the final chapter, Stone asks what alternatives might replace the existing regulatory framework. In general, the book provides detailed, objective answers to the three major questions posed. Only the final chapter can be criticized as not providing convincing evidence for the author's conclusions.

Unlike some critics of the FTC, Stone does not attribute perceived ineffectiveness to presumed incompetence of personnel. Nor does he conclude that the FTC serves primarily the interests of business, either as a result of the original legislation or through the enforcement methods adopted by the agency. Instead, he argues, the effectiveness of the FTC has been hampered by contradiction in its aims, originated in the 1914 Federal Trade Commission Act. That act established the goal of simultaneously promoting competition and stability in the marketplace. Subsequent legislation, with the notable exception of the Robinson-Patman Act, often provided similarly incompatible goals. The Robinson-Patman Act clearly did not promote competition. Even when the goals are not incompatible or contradictory, the consequences of FTC legislation may be so. A recent example is consumer legislation which often calls for high and uniform product standards at the probable cost of reduced competition.

In addition to contradictory goals, the FTC has suffered from a very broad range of responsibilities and limited enforcement resources. Furthermore, Stone alleges the critics often confuse effectiveness with output, and, he argues, the FTC's output has been generally good in view of the legislative and fiscal constraints under which it operates. In particular, he defends the FTC against several of the charges made by its critics and demonstrates that the personnel are generally competent, the FTC has been surprisingly innovative in developing enforcement tools and techniques, and the agency has not unduly focused its investigative efforts on small firms.

Stone's distinction between output and effectiveness begs the question of how to measure effectiveness of a regulatory agency. In the concluding

chapter appears the statement, "Short of totalitarian measures, regulation tends to be ineffective as long as strong incentives exist for violating the law" (p. 259). This statement contributes to the impression that Stone defines effectiveness as the absence of violations. Surely a more reasonable definition is violations prevented or corrected owing to the existence and activities of the FTC. Furthermore, conceptually there is some optimal amount of regulatory activity (a level at which the benefits from additional regulation equal the costs of additional regulation), and almost certainly violations will continue to exist at that level of activity. In general, Stone allocates very little discussion to the costs of regulation to both taxpayers and firms. In the absence of knowledge about costs, one cannot know whether or not the existing regulation is optimal, excessive, or deficient.

The book does not directly address either the deterrent effect or the effectiveness of the FTC in any rigorous fashion. On the other hand, the analysis presented does have clear implications for both. The author demonstrates, for example, that the aims and enforcement tools provided by legislation hinder the FTC in efficiently attaining any one objective. Also, Stone shows convincingly that both major determinants of deterrence—the probability of apprehension and the penalty if captured—are small.

Stone's analysis has direct policy implications for improving the performance and effectiveness of the FTC. Yet in the final chapter he appears to reject the notion of legislative changes and concludes that regulation cannot effectively promote public values. He then offers a tantalizing discussion of the alternatives to regulation, including public ownership of firms. Unfortunately, exploration of these alternatives is beyond the scope of the book, and what exploration is undertaken avoids assiduously any comparisons with other countries. In the absence of a thorough exploration of the alternatives, it is interesting that Stone rejects regulation as ineffective. Assuming his analysis is correct and the FTC, and perhaps most regulation, is ineffective, regulation may still be the most cost-effective method of promoting the public values Stone discusses.

Barbarians, Gentlemen and Players: A Sociological Study of the Development of Rugby Football. By Eric Dunning and Kenneth Sheard. New York: New York University Press, 1979. Pp. xiii+321. $18.50.

Thomas S. Henricks
Elon College

As the subtitle indicates, this book is a sociological history of Rugby football. The emphasis is noteworthy, for *Barbarians, Gentlemen and Players* departs from more conventional social histories both in the nature of its descriptions and in the theoretical concerns which animate the work. As a descriptive project, the book is marked by an occasionally cumbersome

sociological terminology (which the authors take some pains to justify in the preface) and by the incorporation of quantitative data in the argument. Beyond this—and more importantly—the sociological cast is provided by the selection of the independent variable. We are to understand the development of Rugby through "the development of the British class structure and of related institutions such as the public schools" (p. 6).

Such a project might satisfy the social historian, but Eric Dunning and Kenneth Sheard wish their book to do more. Specifically, Rugby football is seen as a " 'decisive' or 'crucial' case" (p. 18) through which one may evaluate three broader issues: (1) the reasons why Britain was the first nation to develop modern forms of sport; (2) the theories of Norbert Elias (a previous collaborator with Dunning in sport studies) regarding the "civilizing process" (p. 6) in Western Europe; and (3) the worldwide trend toward increasing seriousness and competitiveness in sport. As the reader will quickly learn, this is an ambitious book; and the authors are to be admired for the energy and discipline which they bring to their task.

The analysis of Rugby is divided into five historical stages. Each stage is "characterized by more elaborate behaviour and more complex, more formal rules and organization than its predecessor" (p. 1) and is seen as the precondition for later development. Appropriately, the level of sporting development is seen as an expression of British class relations of the time.

Stage 1 features the mass ball games played by lower status groups and youth from the 1300s (when records begin) until the current century. These events are pictured as an equilibrating mechanism rooted in the corporate divisions of a predominantly stable, rural society. Furthermore, the authors emphasize that the very stability and assurance of the elite during this period allowed them to sponsor and even participate in such licentious folk play. This is an attractive argument, but as they admit (p. 38), there is not much evidence to support the notion of democratic participation in these games. Furthermore, the linkage between this stage and specific later patterns is never quite clear. Instead, the early period seems to be a kind of backdrop against which the later events are staged.

The finest chapters of the book are devoted to stages 2 and 3, the development of football in the public schools. Around 1750, the youth at various public schools began to incorporate mass ball games into their house rivalries and hazing practices. Central to an understanding of this process is the changing class composition of these schools. "Fee-paying" or elite boys rise to power, challenge their middle-class masters, and erect a tyrannical system of self-rule of which football is a part. Stage 3 begins around 1830 (sometimes seen as the dawn of modern sport) when the public school game became subject to more stringent and formal regulation. Important now to the argument is increasing industrialization and embourgeoisement and the effects of these on public school reform. New compromises in student-staff power relations (symbolized by Thomas Arnold's success at Rugby) permitted students their ball games but in-

fused the games with stronger notions of gentility or self-regulation. It is at this point that the various school football rules become articulated.

Stage 4 (which lasted from about 1850 to 1900) is characterized by the spread of public school football to the wider society. The "games cult" of the public schools becomes established not only in the universities but also in numerous independent clubs sponsored largely by public school "old boys." It is also at this point that association football or soccer (representing a modification of the Eton-Harrow games) and Rugby become differentiated explicitly and developed on a national level. Although originally more popular than soccer, Rugby is soon outpaced by its rival in the quest for popular support. In response to this problem, Rugby enthusiasts follow soccer in setting up their own national association, Rugby Football Union, in 1871. The meeting held then is significant for, in eliminating some of the more vicious forms of kicking and tripping, the members try to adjust their game to the more "civilized" morality of the 19th-century middle class.

Toward the end of this period, participation is broadened by inclusion of working-class players. Or, more accurately, this occurs in the North alone, where the debate over status exclusivity and the propriety of "broken-time" players results in the splitting off of the more professional Rugby League in 1895. Dunning and Sheard contend that the interdependence of working class and bourgeoisie in the factory system of the North led to attempts by the middle class to organize their social inferiors on the field of play as well. In the South, there was supposedly less contact between these groups. There was also the status insecurity of Southern Rugby players who found themselves competing with the more aristocratic soccer players for recognition.

The last stage extends from the amateur-professional split to the present. Rugby League remains confined to a few Northern counties, while the more amateur form has experienced wider support. However, there has been no simple triumph of the amateur ethos. From the 1960s on, there have been increasing pressures to professionalize Rugby Union (or at least to recognize the kinds of professionalism that occur covertly). As Dunning and Sheard note (p. 268), it may be that the "amateur ideal" can no longer be maintained amid the broader transitions of rationalization and commercialization.

If one accepts this account of Rugby's development (I accept most of it), how does it illuminate the three broader issues mentioned above?

1. Why was Britain the first modern sporting nation? The authors argue that the cause was the relative social independence of a dispersed elite and bourgeoisie from a royal court and the resulting mutual accommodation between the two former groups. Beyond this, the public schools expressed this relative social freedom by allowing their students to become innovators in this area. While this process of embourgeoisement is surely important, there are numerous other factors which the authors neglect. If one concentrates only on the 18th century, these include the new administrative centrality of the gentleman in the agricultural world,

the democratic intermingling at commercial spectator events, and the response of English social customs to the European political revolutions. In other words, the authors' emphasis on the new industrial values and class mixing is not entirely adequate to the task at hand.

2. How does the history of Rugby illustrate Elias's theory (see Norbert Elias, *The Civilizing Process: The History of Manners* [New York: Urizen, 1978], vol. 1) of the civilizing process? According to the authors, it is Elias's position that there has been a broad movement toward increasingly complex controls on human behavior. These are both external (in the form of societal sanctions) and internal (in the form of conscience). This pattern is purportedly exemplified by Rugby's increasingly complex rules and its violence control. I remain unconvinced by this portion of the argument. Is there less societal violence now or is it simply channeled into new forms? Furthermore, while manners may be somewhat softened in industrial societies, does that possibility mean there is more "self-regulation" than before? Might Weber's notion of a "legalistic" attitude toward norms and authority (in sport as elsewhere) be a better way of interpreting the change?

3. What are the sources of the cultural centrality and the increasing rationalization of sport in complex societies? The authors argue (following Elias) that the division of labor itself breeds long chains of functional dependence, status equalization and insecurity, and what they call "multipolar controls" (p. 280), that is, performance pressures on individuals from a variety of groups at once. Sportsmen can no longer be expected to play for fun when so many persons (the controllers and consumers of sport) depend on their performance. Dunning and Sheard then attribute the new centrality of sport to its ability to articulate individual and corporate identity in an exciting fashion. In a secular and leisure-oriented society, it is almost inevitable that sport should emerge as the new drama of status relations. All this seems sound enough, though in such a discussion of sport the reader may be inclined to forget that there are still levels at which games are played informally and for fun. The development of Big-Time Sport may be only one trend among others. Such concerns notwithstanding, *Barbarians, Gentlemen and Players* is a thoughtful and challenging book.

Rockefeller Medicine Men: Medicine and Capitalism in America. By E. Richard Brown. Berkeley and Los Angeles: University of California Press, 1979. Pp. xii+283. $12.95.

Naomi Aronson
Massachusetts Institute of Technology

The title suggests the major weakness of this work: the reader is promised too much and given too little. Two interesting but undeveloped treatises

are imbedded in a single volume. Is this book *Rockefeller Medicine Men,* a social-historical study of the influence of the Rockefeller foundation on medical practice and ideology? Or is it *Medicine and Capitalism in America,* an introductory text summarizing current Marxist analysis of the health care system and its relationship to the economic order? Given the recent upsurge of the latter type of publication (e.g., Vicente Navarro, ed., *Health and Medical Care in the U.S.: A Critical Analysis* [Farmingdale, N.Y.: Baywood, 1977] and John Ehrenreich, ed., *The Cultural Crisis of Modern Medicine* [New York: Monthly Review Press, 1978]), as well as the inclusion of the Marxist perspective in some mainstream medical sociology readers, the first is clearly the more worthwhile project.

E. Richard Brown has obtained some fascinating material from the archives of the Rockefeller and Carnegie foundations, both of which profoundly affected the development of medical education in this country. Sociologists would do well to make greater use of archival collections; too often these rich sources of data have been regarded as the exclusive domain of the historian. The documents show an interrelationship between medicine and capital in America at the turn of the century. Corporate philanthropists saw scientific medicine as a means of expanding both their profits and their ideological hegemony. The first objective was based on the premise that healthy workers were more productive, the second on the promulgation of the idea that disease, not social injustice, was the root of human misery. In the words of Frederick T. Gates, first director of the Rockefeller Foundation, "*Disease* is the supreme ill of human life and it is the *main source of almost all other human ills,* poverty, crime, ignorance, vice" (p. 129). Physicians, in turn, welcomed philanthropic aid for the establishment of modern research and training institutions; the corporately endowed system of medical education drove physicians' competitors, lay-healers and "irregular" doctors, from practice. But the interests of physicians and the foundation's "corporate rationalizers" soon diverged; the overall needs of capital were best served by weakening the monopoly which private practitioners had established over the health care system. Brown concludes, unfortunately with only cursory attention to the intervening decades, that the private foundations, joined by the federal government, continue to shape the American health care system according to the needs of corporate capital.

The historical analysis is encumbered by didactic excursusses into the fundamentals of the political economy of health care. Three points in particular are not only needlessly reiterated but conceptually undermined by the manner of presentation. The first is that corporate philanthropists and physicians were motivated, not by humanitarian goals, but by their material interests. The existence of class and professional ideologies, even among philanthropists and healers, will come as no surprise to most sociologists and historians. But Brown seems to define ideology as hypocrisy; his preoccupation with subjective motives obscures the fact that the authenticity of a particular individual's humanitarian impulses is irrelevant to the objective political functions of his ideas and actions. The second

point is that, although the writings and careers of several individuals are discussed in detail, their effect on the historical development of medical education was a product of the total social context. However, the inclusion of irrelevant bits of biographical detail (e.g., that Dr. William H. Welch, first dean of the Johns Hopkins medical school, was blessed with "gregariousness and wit" which, despite his "rejection of intimate relationships," kept him from being a "recluse" [p. 103]) fosters the very "great man" theory of history which the author rejects. The third point is that the book is not a conspiratorial view of the events but a structuralist analysis of political interests. The necessity for such a reminder arises from the overemphasis on individual characters and motivations which does give the book a conspiratorial tone. This is unfortunate because the main thrust of Brown's argument is indeed a substantive political analysis. Repeating that "this is not a conspiracy theory" gives the impression of protesting too much, an apologetic tone which detracts from the legitimacy of Marxist scholarship.

Finally, the book suffers from careless editing, most notably in redundancy of content and of style. Entire sections of text are duplicated almost verbatim. For example, the discussion of the relationship among the Rockefeller Foundation directors appears on both pages 42 and 111, including the description of Gates as "the brilliant dreamer and creator" and that of John D. Rockefeller, Jr., as "the salesman, the go-between with father." The style is similarly repetitious (e.g., Abraham Flexner "got his bachelors degree from Johns Hopkins in two years of diligent and hard work" [p. 144]).

In sum, sociologists concerned with the historical development of science and medicine in the United States will find *Rockefeller Medicine Men* of interest. But they will no doubt share my disappointment that such a promising venture has been obscured by defects of execution.

Culture and Curing: Anthropological Perspectives on Traditional Medical Beliefs and Practices. Edited by Peter Morley and Roy Wallis. Pittsburgh: University of Pittsburgh Press, 1979. Pp. 190. $14.95.

Judith Farquhar
Pacific/Asian American Mental Health Research Center

Medical anthropology has been plagued from its inception with a kind of "paramedical" thinking, in which an "anthropological" understanding of the culture of patients or the simple addition of foreign techniques to our own medical armamentarium has been taken to be a means of improving standard medical services. Almost without exception, the authors of new books in the field orient themselves first to medicine, perhaps declaring their special contribution to be indispensable to "health care delivery," or perhaps, like a doctor cornered at a cocktail party, pointedly disclaim-

ing any connection with professional medicine. This tendency has as yet been of no great service to medicine, and it has, moreover, failed to live up to the comparative promise offered by anthropology. A truly anthropological perspective would make the relativistic assumption that all forms of medical knowledge and all forms of healing, including Western biomedicine, are equally derived from human experience and social intercourse and can be productively analyzed as such. Without such an assumption in the anthropological study of medicine, the cultural components of healing processes are certain to appear epiphenomenal, and the study of them effete, while professional medicine retains its pride of place as an unquestioned social good, in the service of which anthropologists and other paramedics labor.

The editors of *Culture and Curing* intend it to be a contribution to the recent movement of medical anthropology away from paramedicine. Peter Morley states in his introductory chapter that "medicine in both traditional and modern societies is . . . a social phenomenon and can only be fully understood as such." In his view, sickness and death and the collective responses they engender are best understood as specialized expressions of a wider set of socially constructed meanings. The subject under investigation is thus an anthropological one: culture and meaningful action in sociocultural context. Morley discusses the contributions of writers like Richter, Gluckman, and Horton to the "primitive rationality" debate, developing an argument that traditional medical systems share many important features with modern scientific medicine. Although such an approach suggests the possibility of examining the cultural features of our own medical system and its bioscientific matrix, Morely and Roy Wallis have chosen to make a qualitative distinction, having to do with social differentiation, between traditional and modern medical systems.

This decision is reflected in the papers that have been selected for inclusion; all report research in "traditional medical systems," but only one of them can be viewed as a good illustration of the method that Morley advocates. This paper is R. G. Willis's "Magic and 'Medicine' in Ufipa." Willis explores the place of illness events and healing procedures in Fipa cosmology and social structure, showing how concepts of the body relate to a contrasting notion of social interconnectedness as it affects well-being. "Serious" ailments are those that arise from the patient's social situation, and in treatment they are symbolically manipulated in an effort to put all the elements of individual and social selfhood back in the proper relationship to one another. This kind of medical anthropology makes it clear, not only that good ethnography is required if illness and curing are to be understood, but also that the study of illness and curing can be a powerful tool in doing good ethnography.

Unfortunately, the rest of the papers in the volume are less successful in terms of the goals set out in the introduction. Michael Kearney's study of spiritualist healing in Mexico devotes scanty attention to the social and cultural context in which spiritualism flourishes, and Donn V. Hart's "Disease Etiologies of Samaran Filipino Peasants" is a routine exercise

in description and classification. Neither brings us any closer to an appreciation of the meanings of health, illness, and curing as they are experienced by the people studied. Richard Lieban's study of sex differences in the utilization of Cebuano healers, though more alive to the relations between context, belief, and behavior, is handicapped by an inappropriate attribution of causal status to biological factors that he has not investigated; the important role that "the biological base" plays is thus largely an unexamined assumption.

Una MacLean's survey of attitudes to traditional medicine among the Yoruba arrives at the not-very-startling conclusion that traditional medicine is not likely to be completely replaced by modern medical resources, but this conclusion is based on a strictly descriptive approach that treats the relationship between traditional cosmology and "choices of treatment" only in passing. Paul Atkinson's "Lévi-Strauss in Vermont" works from a popular book about rural American folk medicine and finds significant (though to me counterintuitive) structures that are coherent and elegant. Although MacLean and Atkinson display almost polar differences here, both papers suffer from the limitations of their methods, failing to move beyond survey research on the one hand and structural text analysis on the other. Neither approach gives much insight into what Morley in his introductory chapter calls "the phenomenological and social meanings of traditional medical beliefs and practices."

Barbara K. Halpern's and John M. Foley's analysis of the text of a Serbian healing charm benefits from having more modest goals than Atkinson's text analysis, and it is more successful at linking the material at hand to its social and cultural context of use. Lola Romanucci-Ross's "Melanesian Medicine: Beyond Culture to Method" is a charming paper that nevertheless offers no help in separating the "method" referred to in the title from the madness of the prose. If it was the author's intention to mystify completely Melanesian medicine, she has succeeded admirably.

Despite the differing orientations and uneven quality of the papers included, Peter Morley's thoughtful introduction and R. G. Willis's thorough ethnography elevate this book well above the general run of new books in medical anthropology.

Women and Men Midwives: Medicine, Morality and Misogyny in Early America. By Jane B. Donegan. Westport, Conn.: Greenwood Press, 1978. Pp. viii+316. $17.95.

Barbara Katz Rothman
Baruch College, City University of New York

Along with the revival of midwifery in the United States as nurse-midwifery, and the interest in women's history created by the women's movement, there has come a new look at the history of midwifery. In a field

about which only a few scattered articles existed 10 years ago, we are now blessed with numerous studies and several fine books. Jane B. Donegan's *Women and Men Midwives* is an important contribution to this growing literature.

Donegan's book is a history of American midwifery and obstetrics. It is primarily, as she herself describes it in the introduction, an examination of the development of male-dominated obstetrics against the background of middle-class concepts of morality, reform movements, and emerging feminism. As is true of most American history, the story begins elsewhere. Donegan begins by describing the history of English midwifery and the entrance of surgeons, as men midwives, into the lying-in chamber.

The surgeon's interest lay predominantly in the abnormal case, in which the use of instruments was indicated. Midwives retained their long-standing interest in the normal birth. Interest in abnormal cases increased in the 18th century; Donegan notes that the increase may have been due to rapid urbanization and the resultant increase in pelvic deformities caused by rickets. The history of male midwifery began with the barber-surgeon called in to perform an embryotomy, crushing the fetal skull and removing the body piecemeal, and only gradually came to include attendance at normal births. Indeed, the attendance of men at "normal" births marks the end of the perception of birth itself as normal.

Spurred on by the development of basic anatomical knowledge and increased understanding of the processes of reproduction, surgeons of the 1700s began to develop formal training programs in midwifery. Women midwives were systematically excluded from such programs. Donegan attributes the skepticism that most of the men felt about the abilities and potential of women in midwifery to the prevailing sexist attitudes of the age. Women were not trained because they were believed to be inherently incompetent. The situation was far from simple, however, as Donegan makes clear, and some surgeons did try to provide training for the midwives, sharing with them the advances in medical knowledge. Such attempts failed in the face of opposition from within medicine, supported by the prevailing beliefs about women's inability to perform in a professional capacity. The result was a widening disparity between midwives and surgeons. As new and sophisticated techniques were developed by the men, they were kept from the women.

Donegan does point out, however, that the surgeons were not necessarily providing better services, in spite of the surgical and instrumental techniques. Not all physicians who practiced the new obstetrics were trained in its use. "Meddlesome midwifery" is the forerunner of what in today's age of rising cesarean section rates is often called "interventionist obstetrics." Donegan reviews the arguments of the time that the iatrogenic results of the new obstetrics may have outweighed its benefits, particularly for normal and healthy births. Donegan, confining her scope to early America, does not examine these arguments in the context of the similar disagreements raging today between proponents of obstetrics and of midwifery.

The more compelling argument for the late 1700s to the mid-1800s however was that of modesty. A man in the lying-in chamber posed an affront to the modesty and delicacy, the sensibilities, of the ladies of the time and to their husbands' ideas of propriety. By the late 1840s this attitude, combined with the rise of feminism, began to open the door for women as "doctresses." Women like Elizabeth Blackwell, the first American woman physician, entered the formerly all-male world of medicine. This new departure was supported by feminists; for example, the Syracuse Women's Rights Convention in 1852 urged all sympathizers to employ a woman physician rather than a man. Standards of the day still limited women in medicine to obstetrics and the care of the diseases of women and children. The most radical and the most reactionary thus found themselves unwitting allies, as Donegan points out, in the sense that they supported the idea of training women as physicians: the one on grounds of equality; the other to maintain modesty and traditional ideas of propriety. Again the student of the contemporary childbirth movement is drawn to note the parallel with today's home birth and midwifery movement. The contemporary movement is supported both by feminists, as an issue of women's control over their bodies, and by the most traditional, as a restatement of the centrality of motherhood and the family.

Donegan does not attempt to draw these comparisons with the contemporary situation. Her book ends, with much of the story still untold, in the mid-1800s. Her goal, at which she succeeds admirably, is to fill in part of the history of American midwifery. In so doing she adds to our understanding of the history of medicine and the history of sexism.

The Politics of Health Care. By J. H. U. Brown. Cambridge, Mass.: Ballinger Publishing Co., 1978. Pp. xviii+153. $15.00.

Lola Jean Kozak
University of Chicago

The Politics of Health Care purports to be an examination of the factors that affect health policy and of the way policy decisions are made. The topic is enormous and important; the book, unfortunately, is not. J. H. U. Brown has written a slim volume based on his review of an array of published materials and on his own experiences, which include several years as a high-level official in the former Department of Health, Education, and Welfare. He reaffirms that politics is an integral part of the health care system, but he adds little else to the existing literature in the area.

Brown defines as political any forces "that attempt to maintain the status quo or to force change on a system" (p. xiii). For the health care system he provides a long list of groups with influence on policy: consumers, hospital boards, professional associations, insurers, drug and other private companies, professional schools, local, state, and federal govern-

ment agencies, and so on. He then reviews various parts of the health care system, pointing out which groups have influence on each part. In most cases the influence is regarded as an impediment to the development of more logical or cost-effective health policies.

For example, he discusses health maintenance organizations as a possible solution to problems in the provision of primary health care, but he finds that their development has been restricted by the opposition of state insurance commissions and medical societies. One of the major problems he sees with regard to hospitals is the high cost and uneven quality of laboratory tests, but a bill designed to regulate the laboratories was strongly opposed by the American Hospital Association and the various professional associations of laboratory personnel. He says that emergency medical services are not effectively organized because of jurisdictional disputes among a number of government agencies and private organizations.

Little in Brown's inventory of problems and influence groups is new. He does, however, avoid the feverish prose and monocausal explanations that often have marred the previous discussions of health politics. He also covers an unusually wide range of health topics, but this breadth results in some very superficial discussions of important issues. For example, a section on the medical industrial complex is allocated less than two pages. Brown suggests in the section that while the American Medical Association may be losing power, the industrial complex is becoming more and more involved in the health care system. Rather than stop to analyze the possible implications of such an important development, Brown says little more than, "Many think . . . an influx of hard-headed businessmen might be good for the system" (p. 60) before moving to the next topic.

Another difficulty caused by Brown's wide range of topics is that he has apparently included some about which he knows very little. There are a number of factual errors, particularly in the discussions of international issues. In a section on foreign medical graduates, Brown says that Sweden had an oversupply of physicians in the 1960s; in fact the physician-to-population ratio was lower in Sweden than in the United States until the mid-1970s. He also says that many European countries have fewer than 100 physicians per 100,000 population, but almost all European countries that report statistics to the World Health Organization have well over 100 physicians per 100,000 population, and many approach 200.

The chapter on national health insurance contains a series of errors. Brown talks about the national health insurance system in the United Kingdom, for instance, but the country has a national health service, not an insurance system. He says New Zealand is the only country to provide health care funds from general revenues, but many countries finance their health care systems, at least in part, from general revenues. He reports in a chart that 60% of the reimbursement for health care in France is paid by the government; instead, the major share comes from insurance.

The solution Brown offers for the problems of the health care system is first of all more health services research. Throughout *The Politics of Health Care* he maintains a faith in the power of more and better informa-

tion to cut through the tangles of influence group pressures and lead to the adoption of more sensible policies. He also calls for a "central point for decisions backed by authority and knowledge" (p. 133). He does not say that the government should take on this responsibility, but he does recognize that a variety of questions about health care, such as the development and application of high technology, are legitimately political issues. More important, he does not suggest how any one organization, governmental or otherwise, can go about obtaining the authority needed to coordinate health policy. The information he presents, as he is well aware, makes it seem quite unlikely that such a coordinating organization will emerge soon.

Aging and Income: Programs and Prospects for the Elderly. Edited by Barbara R. Herzog. New York: Human Sciences Press, 1978. Pp. 352. $15.95.

Social Security: Today and Tomorrow. By Robert M. Ball. New York: Columbia University Press, 1978. Pp. xxiii+528. $14.95.

George L. Maddox
Duke University

The demographic transition experienced by industrial societies is reflected in dramatic changes in the age structure of populations. For example, the percentage of persons over the age of 65 in the United States (11%) has more than doubled in this century and is projected to be substantially higher (17%) in the near future. Demographers, epidemiologists, and economists have displayed considerable interest in the implications of population aging, and their interest is illustrated reasonably well in the two volumes reviewed here.

Aging and Income, a special publication of the Gerontological Society edited by Barbara R. Herzog, reports the proceedings of a conference on programs and prospects for older persons in societies at or near zero population growth. The chapter on dependency costs by Robert Clark and Joseph Spengler is an excellent illustration of the convergence of demography and econometrics in analyzing the economic implications of ZPG. Other chapters provide useful material on the implications of a no-growth economy for an aging population, changing patterns of work and retirement; the problems and prospects of providing adequate income for a population that retires early and expects to live many years in retirement; and the importance for older persons of income from sources other than wages. A chapter by Alicia Munnell, "Social Security in a Changing Environment," warrants particular mention. She argues forcefully that the national economy can ill afford to continue uncoordinated, multiple public social security programs and calls attention to the proretirement and anti-

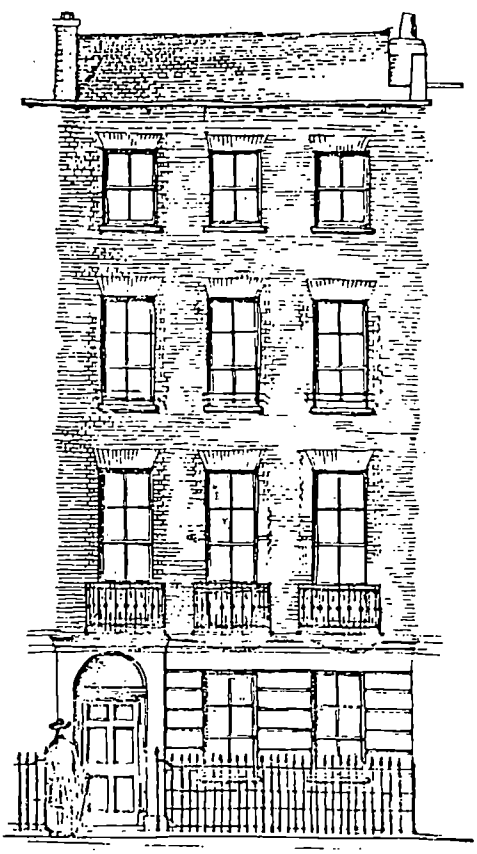
savings aspects of existing programs. *Aging and Income* is a competently written and reasonable introduction to an important set of issues; its high price may be justified by the two excellent chapters specifically noted above.

Social Security is less a book than an encyclopedia about what has been called, with some justification, the most successful program of social reform in the history of this country. Robert M. Ball literally grew up with and was a major architect in the building of the social security system. The question and answer format of the presentation will dissuade most persons from attempting to read it like a book, but everyone will profit from consulting it. *Social Security* provides almost everything anyone ever wanted to know about attempts to provide income security for the nation's retired, widowed, disabled, and poor, and probably more than most people ever intended to ask. Ball's view of the social security system is essentially administrative and atheoretical. He sees the system, for all its peculiarities and specific inequities, as a politically viable intergenerational contract in the American context. Ball ends his encyclopedia with 31 propositions which affirm his belief in the basic soundness of the existing system and specify the kind of fine tuning which will improve the effectiveness of the system in maintaining adequate income for all citizens over the life course. Of special note are his recommendations for taking into account the changing status of women and for retaining a system which continues to emphasize and require self-help in maintaining income for those outside the workforce. While social security appropriately has some welfare functions, it is only part of the answer for securing income over the life course. *Social Security* is a volume that belongs on everyone's bookshelf for personal if not professional reasons. The great majority of citizens are very dependent on the social security system. And because social security is built on the intergenerational transfer of societal resources, one does not have to be old, widowed, disabled, or poor to be involved and interested.

Until recently, most sociologists have managed to neglect the research opportunities associated with population aging. Except for a small number of sociologists identified as gerontologists, studies of the social and societal implications of aging have been left to economists, political scientists, and historians. This neglect is not likely to survive the belated realization of sociologists that population aging provides an unparalleled opportunity to study social change, intergenerational relationships, and societal allocation of resources over the life course. Perhaps these two books will provide a gentle reminder that sociology is missing an important part of the action.

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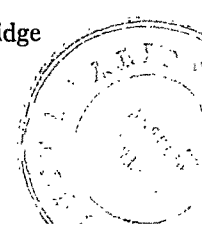
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IN THIS ISSUE

GIORGIO GAGLIANI has studied in Rome and at the University of Chicago (M.A. in Economics, 1969). He has been an assistant professor of economic policy at the University of Rome and has recently been named professor of labor economics at the University of Calabria. He is interested in income distribution, wage differentials, and the structure of employment.

THOMAS A. DIPRETE is assistant professor of sociology at the University of Chicago and a research associate at the National Opinion Research Center. He has recently completed a study of discipline and order in American high schools and is currently conducting research pertaining to institutional and community attachment.

JON MILLER, associate professor of sociology at the University of Southern California, is pursuing further explorations of the limits of rationalistic conceptions of organizational activity and is particularly interested in relating strategies of organizing collective activities to larger political-economic and historical circumstances.

JAMES R. LINCOLN is associate professor of sociology at Indiana University and director of the Institute of Social Research. His present research pertains to interorganizational networks of youth service agencies. He also has a continuing interest in cross-cultural research on organizations.

JON OLSON is assistant professor of sociology at the University of Akron. His research interests include stratification in organizations, comparative organizational structure, and the ideology of industrial health.

THEODORE D. KEMPER is associate professor of sociology at St. John's University in New York. His major interest continues to be the development of the power-status model of social relations and its implications, especially in the area of emotions. His current work focuses on the division of labor, on emotions, and on love—as emotion and as social relationship.

MARK DAVIES is a graduate student in the Division of Biostatistics, Columbia University, and the Department of Child Psychiatry, New York State Psychiatric Institute. His principal research interest is reliability in multi-informant studies.

DENISE B. KANDEL is professor of Public Health in the Department of Psychiatry at Columbia University and head of the Division of Youth and Family Research in the Department of Child Psychiatry, New York State Psychiatric Institute. Her research interests include adolescent socialization and development, processes of interpersonal influence, psychiatric epidemiology, and social strains and coping. She is currently engaged in a follow-up study in young adulthood of a subsample of former adolescents included in the analyses presented in this issue. She is also completing cross-cultural studies of the pychosocial adjustment of adolescents in France and in Israel.

CHARLES HIRSCHMAN is associate professor of sociology at Cornell University. His current research projects are (1) a study of the timing of family formation events in Southeast and East Asia and (2) the process of stratification among Asian-Americans. His long-term interests are social and demographic change in Southeast Asia and structural inequality in the United States.

KIM BLANKENSHIP is a doctoral candidate in the Department of Sociology at Duke University. Her major areas of research interest are issues of political economy, Third World development, women and the family in class society, social change theories, and philosophy of science.

KENNETH A. BOLLEN is a Senior Research Scientist at General Motors Research Laboratories. He is collaborating with David P. Phillips in further research on imitative suicides. He also continues to do research on international development and statistical methodology.

DAVID P. PHILLIPS, associate professor of sociology at the University of California at San Diego, continues to focus his research on two major issues in public policy: (1) the effect of the mass media on violence and (2) the effect of capital punishment on homicide. More generally, he is concerned with elucidating the social and psychological factors affecting mortality.

ALLEN E. LISKA is associate professor of sociology at the State University of New York at Albany. His research areas are social psychology and deviance/social control. He is currently studying the capacity of models generated from the conflict perspective to explain variation in crime control across major U.S. cities.

JOSEPH J. LAWRENCE is associate professor of sociology at Central Michigan University. His research interests are deviance and social control, social indicators, and issues in teaching sociology. His current research projects include studies on the fear of crime and additional analysis of urban police forces.

MICHAEL BENSON is a doctoral candidate in sociology at the University of Illinois. He is currently doing research on white-collar offenders in Chicago.

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Format of References in Text: All references to monographs, articles, and statistical sources are to be identified at the appropriate point in the text by last name of author, year of publication, and pagination where appropriate, all within parentheses. Footnotes are to be used only for substantive observations. Specify subsequent citations of the same source in the same way as the first one; do not use "ibid.," "op. cit.," or "loc. cit."

1. When author's name is in the text: Duncan (1959). When author's name is not in text: (Gouldner 1963).

2. Pagination follows year of publication: (Lipset 1964, pp. 61–65).

3. For more than three authors, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation: (U.S. Bureau of Census 1963, p. 117).

4. With more than one reference to an author in the same year, distinguish them by use of letters (*a*, *b*) attached to the year of publication: (1965*a*).

5. Enclose a series of references with a single pair of parentheses, separated by semicolons.

Format of References: List all items alphabetically by author (providing the full list of multiple authors) and, within author(s), by year of publication, in an appendix titled "References." Examples follow:

Davis, K. 1963*a*. "Social Demography." Pp. 124–37 in *The Behavioral Sciences Today*, edited by Bernard Berelson. New York: Basic.

———. 1963*b*. "The Theory of Change and Response in Modern Demographic History." *Population Index* 29: 345–66.

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How Many Working Classes?[1]

Giorgio Gagliani
University of Rome, and University of Calabria

This article concerns the contradictions between the segmentationist approach to the analysis of the labor market and the orthodox radical contention that all or most wage and salary earners belong to the same social class. Labor market segmentation implies privileges, and these, in turn, provide for class alliances aimed at perpetuating segmentation. Here the crucial class division within the labor market is seen as located along the manual-nonmanual occupational divide. The concepts of equalizing pay differentials, noncompeting groups, and barriers to education are used to show that the nonmanual working class has a strong incentive to form alliances with capital, at least as long as benefits which might derive from different alliances remain unspecified.

> [Distribute] the earth as you will, the principal question remains inexorable: who is to dig it?
> [JOHN RUSKIN, *Sesame and Lilies*]

This is a study in class analysis written by a labor economist. Sociologists and economists, when they happen to be interested in the same problems, resemble explorers belonging to two different expeditions, facing each other across a tumultuous river and discovering with the help of binoculars that one group has the canoes and the other the paddles.

It is also a critique of what I call the "orthodox radical" approach to class analysis, orthodox inasmuch as it tries to apply conventional Marxist theory to the modern world. Among writers of different creeds there exists a tendency toward reciprocal allegations of neglect, somewhat reminiscent of passengers peeping at each other's papers in the bus, professing indifference when caught.

From this double point of view this article represents an attempt to

[1] I wrote this article while on sabbatical at the Institute of Industrial Relations, University of California, at Berkeley. I thank its director, Lloyd Ulman, and all the staff for their kindness and cooperation. I also thank W. Kornhauser, P. Adler, M. Burawoy, G. Carbonaro, C. Downs, M. Franzini, T. Green, S. Jacoby, E. Jones, C. Lloyd, J. Lopreato, P. Palazzi, P. Rappoport, S. Rosenberg, F. Sanna Randaccio, and L. Ulman, who offered helpful comments on a first draft, and the Consiglio Nazionale delle Ricerche for financial help. Responsibility for errors remains, of course, mine. Requests for reprints should be sent to Giorgio Gagliani, Dipartimento di Economia Politica, Università della Calabria, Arcavacata di Rende (CS), 87030 Italy.

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establish contact. Readers should note that there are communication problems and that the waters are unsafe.

Since the mid-1950s economists have been writing on "balkanized," "internal," "stratified," or "segmented" labor markets (Kerr 1954; Doeringer and Piore 1971). It has become coventional among them to draw a dichotomous distinction between a primary market, consisting of jobs which offer high wages, good working conditions, employment stability, and opportunities for advancement; and a separate secondary market, in which the overall conditions are far less desirable. The persistence of this dual structure is normally explained by referring to a combination of factors related to technology, the market power of firms, and the interaction between job characteristics and worker traits (Piore [1970] 1977).

Being economists, these authors have normally tended to shy away from the implications of their analyses in terms of class distinctions. However, radical economists who have recently tackled the problem have offered their own rationale for segmentation along with a class-oriented interpretation (Edwards, Reich, and Gordon 1975; and, especially, Edwards, Reich, and Weisskopf 1978; Edwards 1979). Capitalists, the argument goes, have always attempted to divide the working class the better to conquer it. In the 19th century they were confronted with a dangerous trend toward the homogenization of the work force which might lead to their defeat. By the end of World War I oligopolistic ("core") employers, thanks to their increasing market power and high profits, managed to reverse that trend, introducing a rigorous "internal market" framework of limited ports of entry, seniority rules, firmly articulated career ladders, stable employment, and high wages. In so doing, they bribed part of the working class into accepting capitalist rule. This method of controlling the workers, which may be termed "bureaucratic control" (Edwards 1979) coexists today with two older and less refined ones, namely, *technical control,* or machine-paced processes such as the assembly line, and *simple control,* or arbitrary rule, prevailing in the surviving peripheral economy, for example, in smaller competitive firms.

Accordingly the labor market can be more realistically subdivided into three segments and the working class into three fractions (Edwards 1979). The independent section of the primary market (the domain of pure bureaucratic control) is composed of the middle layers, which include "craft workers, technical and professional employees and supervisors, and middle level administration staff; public employment constitutes a significant share" (p. 192). The subordinate primary market which, being primary, also offers job security, relatively high wages, and some career prospects, is a strongly unionized market in which the "traditional proletariat" dwells. Present here are elements of both technical and bureaucratic control, thanks to a "bargain between core firms and industrial unions that

leaves the management of the business in employers' hands but guarantees to workers primary job rights" (p. 181). At the lower end of the ladder is the realm of the "working poor." Here labor power comes closest to being treated simply as a commodity "unfettered and unencumbered by any job structure, union or other institutional constraint" (p. 167).

These developments would seem to cast promising doubt on the monolithic image of the working class that radicals have normally tended to present. Not so. The working class may well be divided into "fractions" but it is said to remain, nevertheless, *una et trina*—an argument reminiscent of other articles of faith. Differences in pay and working conditions among fractions are stressed, but at the same time it is reaffirmed that (1) the true common enemy of all three fractions is the capitalist employer and (2) what may appear to be privileges are in reality no such thing.

Against this it may be argued that proposition 1 above may be correct but is rather simplistic. Sharing an enemy does not automatically imply being friends (Wallerstein 1975, p. 368). The end result will depend, instead, on which alliance is more convenient for whom. As to 2, while it is somewhat aristocratic to assume that workers are unable to recognize a privilege when they see one, it is certainly contradictory to ascribe the success of a divisive plot to the concession of unreal advantages. Now, granted that these privileges exist within a given framework, the privileged must be somewhat reluctant to enter a different framework, unless they can be persuaded that their losses, if any, will be minimal. This applies, of course, to all privileges, not just to those deriving from differential ownership of the means of production.

The main thesis of this article is that a middle class of employees, or a "new petite bourgeoisie," does exist, and that its borders with the traditional working class can be approximately situated along the manual-nonmanual labor divide. The preservation of mechanisms accounting for the creation and reproduction of this class has been, I believe, one of the fundamental aims of political alliances within Western democracies. I will contend that this new petite bourgeoisie *is* class conscious; that its interests are best defended through an alliance with the old petite bourgeoisie and capital; and that, as these three classes together now enjoy a numerical majority in all advanced capitalist countries, they can defend their interests using constitutional means compatible with a free electoral process.

At the heart of the problem lies the division of labor in society. If the proletarianization thesis, advanced by orthodox radicals, were correct, the scope and importance of class-determined mechanisms of job allocation would be diminishing. In other words, when all jobs are more or less comparable as to pay and working conditions, the allocative process is not crucial for class alliances, while the division between the propertied and

the propertyless remains crucial. My first target (Secs. I—III) will thus be the alleged proletarianization of the work force. In particular, Section I will examine the orthodox radical approach to the problem. Section II will address itself to the proletarianization of the old petite bourgeoisie. Section III will concentrate on the manual-nonmanual distinction and on the proletarianization of nonmanual employees. I will try to show that the available evidence does not support the belief in proletarianization.

Section IV will then apply the classical theory of pay differentials to the relationship between the work conditions of manual and nonmanual labor. It will be argued that the dividing line between the two types of labor is determined by the presence of noncompensating pay differentials. As these are, in turn, due to barriers to entry into nonmanual occupations, Section V presents an analysis of such barriers seen as a reproduction mechanism for a class-determined, biased division of labor. I will then draw the sociological implications of the analysis in terms of both class boundaries (Sec. VI) and class alliances (Sec. VII).

I. THE RADICAL APPROACH TO PROLETARIANIZATION

The word "proletarianization" tends to be assigned two different meanings. Technically, in Marxian terms, a self-employed worker who turns into an employee—irrespective of whether he becomes a miner or a lawyer—has turned into a proletarian. The other meaning relates to conditions of work and pay in some nonmanual or self-employment occupations. If these conditions worsen to such an extent that they come to resemble those typical of manual wage labor, the occupations are said to have been proletarianized.

Words are not used by chance. If many members of the labor force are turning into proletarians in either sense of the term, the hope that they will realize their strength and unite to overthrow capitalism may some day come true. And it is on this hopeful note that radical studies on labor market segmentation generally end. Labor may be segmented, but past and present trends leading to the proletarianization of all the segments create the preconditions for unitarian action, which will be undertaken as those strata acquire class consciousness and form a "class for itself." But let us see some examples of the way this two-edged proletarianization thesis is formulated.

Drawing on historical occupational statistics, Reich (1978, p. 179) remarks that in the United States of 1776 the majority of the working population consisted of "wealthy merchants" and "independent commodity producers." Very few people had to hire themselves out as workers. To be sure, there were "significant class inequalities. . . . *But about four fifths* of the nonslave labor force . . . derived their income from their own

property and labor" (my italics). Changes since then have been dramatic: "The United States has become a nation of wage and salary employees, who have virtually no access to income from property or control over the production process, and whose economic welfare is determined by the vicissitudes of the labor market. The process by which capitalist development progressively reduces more adults to the status of seller of labor power has taken place in all capitalist countries" (Reich 1978, p. 181).

The "middle layers"—according to Edwards (1979, p. 193)—have lost their onetime advantages. The old middle class was made up of independent risk takers, determining "their immediate conditions of work," making decisions based on their own interests. Its members were "shrewd, petty, calculating, opportunistic, independent-minded, politically conservative but personally bold small entrepreneurs." In contrast, the new middle layers that have allegedly taken their place are limited in their horizon, "organized and governed by the highly structured apparatus of bureaucratic control." They have become "organization persons."

The working conditions of the middle layers—argues Braverman (1974, chap. 15)—are constantly deteriorating because of automation and rationalization. The old nonmanual employee worked close to management and enjoyed a relatively high salary for duties entailing a substantial degree of responsibility. In contrast, the introduction of office machinery and the pressure toward an increasing division of labor has degraded most middle-class jobs, depriving them of all those desirable characteristics.

In the following sections I will argue that the radical practice of counting the proletariat by subtracting the self-employed from the total labor force, if correct within the radicals' internal taxonomy, is of little use in understanding historical changes in the class structure of capitalist societies. More specifically, I will maintain that, although many self-employed workers should be considered some kind of proletarians, many employees should not, if overall conditions of life and work are not to be ignored by an operational approach to the analysis of class structure and alliances.

II. SELF-EMPLOYMENT AND THE OLD PETITE BOURGEOISIE

If theories are to be tested, analytical categories must be matched with available statistical classifications. And it must be very tempting for orthodox radicals to use the "self-employed" category of occupational statistics as a good proxy for the ensemble of employers and petits bourgeois. Reich's passage (1978, p. 179) quoted above is but one instance of how difficult it is to resist that temptation. The self-employed—it is argued—must have at least some capital to work with; no one extracts any surplus from them; and, if they are not themselves employers, they

work their own hours, unvexed by any supervisor. These characteristics *must* be enough to exclude them from the proletariat and identify them with some fraction of the bourgeoisie.

Indeed, they *are* enough. The problem is, however, that not all those registered as self-employed correspond to that description. This is especially true in underdeveloped countries, where the capital owned, let alone the amount of independence enjoyed, by the self-employed shoeshiner, street hawker, or porter is negligible. And it is even more true in the agricultural sectors of those countries, where many of the registered "self-employed" are only croppers, renters, tenants, and their relatives, groups that own no capital or land. Even among those who *do* own some land there are many to whom the description would not apply. If your plot is very small, it does not matter much whether you have to punch a clock or not. And if you are not a mere seller of labor power, you may be a mere seller of corn, whose only "free" choice is that between earning a low and unstable income in long hours of hard work and starving to death. Technically, in all these cases, you cannot be called a proletarian. But, surely, you are not much of a petit bourgeois either.

Underdevelopment is, after all, precisely this: too many people are employed on the farms. Their being normally *self*-employed, owing to the way the land is divided, is largely irrelevant to their socioeconomic conditions or, more precisely, is relevant mainly because they constitute a subsistence-income fraction of the labor force which stands very little chance of becoming unionized.

Capitalist development has typically required a pool of unemployed and underemployed people to stand ready to leave the farms and work as cheap labor in the factories. The soldiers of this reserve army, the source of this "unlimited supply of labor" (Lewis 1954, 1958) were, mainly, self-employed workers or their unpaid relatives, who were "pushed" and "pulled" into low-paying industrial jobs by the conditions of life and work they were leaving and those they were hoping to find. The disruption was immense, their suffering extreme. Compassion, however, should not obscure the fact that their transfer was a prerequisite for development and for an improvement, via productivity growth and unionization, in the long-term prospects of the migrants themselves. It was, as Edwards (1978, p. 109) has put it on a different occasion, a "sad but progressive" event. Naked exploitation replaced veiled exploitation (Marx and Engels [1848] 1959, p. 49; Marx [1889] 1946, p. 739) but exploitation it was, even before the transfer. *Mutato nomine*—writes Marx after Horace—*de te fabula narratur*.

However, it may be argued that this applies only to underdeveloped countries. The story must be different for Western industrialized areas and, especially, for the United States, where capital was abundant and

land ownership more common. Here the statistical disappearance of the self-employed from the farms can be rightly interpreted as a proletarianization of a vast number of unwilling petits bourgeois, compelled to cede their properties to the capitalist bird of prey and to start working for wages.

This claim, as applied to the United States, stems from a curious combination of orthodox radicalism with the American dream, ready to see in every "farmer" a self-assertive settler, as eager to leave the country as were the Cartwrights to leave Ponderosa. In the United States of 1776, the "golden age" of the self-employed, total population was less than that of today's Maryland, and 95% of that population inhabited rural areas. Developed countries were once underdeveloped.[2] And it would be interesting to learn what alternative model of economic development radical authors have in mind, which would have ensured growth to an economy of 200 million people without destroying a world of self-employed farmers.

In 1900, the first year for which a consistent set of data is available (Lebergott 1964), the process was far from completed. The structure of U.S. employment by broad industrial classification (agriculture, industry, services) was roughly the same as in today's Mexico. Self-employment still accounted for 47% of the total "occupied" (paid and unpaid) population. By 1960 this percentage had come down to 16, and it is less than 10 today. Table 1 shows, however, that the largest growth-generated outflows from self-employment did not occur among Reich's "wealthy merchants" or among Edwards's "small entrepreneurs." The exodus was, above all, a flight from the farms and especially—as might have been expected—from those farm jobs which entailed no land ownership. For every job lost in 1900–74 by full and part owners of land, almost two

TABLE 1

U.S. PERCENTAGE SELF-EMPLOYED AND UNPAID FAMILY WORKERS, 1900–1974

	1900	1960	1974
Farm	68	36	26
Full and part owners	30	21*	19
Nonowners and unpaid family workers	38	15*	7
Nonfarm	32	64	74
Total	100 (12,690)	100 (10,838)	100 (8,262)
Total as a percentage of total occupied population	47	16	10

SOURCES.—1900 and 1960: Lebergott 1964, tables A-3 and A-4; U.S. Bureau of the Census 1975, Series K, pp. 109–13; 1974: ibid., 1977, tables 653 and 1136.

NOTE.—*N*'s in thousands shown in parentheses.

* 1959.

[2] For Lewis's (1954) model at work in more developed contexts, see Kindleberger (1967).

were lost by tenants and unpaid family workers in agriculture. Similar developments were registered in all other Western countries for which data are available.[3] Recent research (Lecaillon and Germidis 1975; Gagliani 1979) has also shown that, in a process of economic development à la Lewis, the movement of self-employed workers into factory employment greatly increases the average income of the self-employed group and reduces the average income of the old and new employees combined, relative to average income per occupied person.[4] This is due, at least in part, to a statistical composition effect: the lumpenproletarians, the wretched of the earth, the working poor, or whatever else one may want to call them, keep average incomes down wherever they happen to be.

On the other hand, for every job lost by landowners in 1900–1974, almost one was gained in nonfarm self-employment, the category most likely to contain the petits bourgeois of various types. Of course, this category lost ground in relative terms, owing to the enormous increase in wage and salary employment. But, at least in the United States, it grew in absolute terms at a compounded annual rate of .6% in the first three quarters of this century. Even taking into account the U.S. manufacturing sector alone, in which the number of self-employed workers decreased by almost 50% between this century's high (1910) and low (1933), one finds (Lebergott 1964, table A-7) that subsequent rises brought that number in 1960 to 90% of what it was in 1900. In all other sectors, except agriculture, this group has increased in absolute terms. And the number of "individually-owned businesses and farms" increased by 550% from 1939 to 1950 and by 58% from 1950 to 1974.[5]

Some of the self-employed may have experienced an income reduction relative to the wage earners but, on the whole, although the evidence is

[3] For every 10 jobs lost by farm owners and tenants, 26 were lost by unpaid family workers in West Germany (1950–78) and 16 in Italy (1954–76) (*Statistisches Jahrbuch* 1960, p. 142; 1979, p. 97; *Annuario Statistico* 1955, p. 308; 1977, p. 322). Similar information may be found for France and other OECD countries (OECD 1965) and for Japan (Minami 1973, pp. 33–34).

[4] Let N be total employed population, y its average income (obtained by dividing national income Y by N); L the number of wage and salary earners, with average income w; and $(N - L)$ the self-employed, with average income i. Note that if W is total wages and salaries, w equals W/L and i is $(Y - W)/(N - L)$. The ratio w/y is, in underdeveloped countries, typically larger than unity, meaning that many of the workers in the very sizable $(N - L)$ group are much poorer than many of the few L's. Development leads to a drastic increase in i/y and a reduction in w/y (the two things do not necessarily go together) as the $(N - L)$ group is freed of its poorest members and the L-workers acquire low-income colleagues.

[5] Data from U.S. Bureau of the Census (1977, table 892, and 1975, Series V 4). In some countries, such as West Germany, the absolute number of nonfarm self-employed workers went down. Still, 91 of every 100 jobs lost by the self-employed between 1950 and 1978 had been in agriculture. In other countries, such as Italy, the absolute number increased: for every 100 jobs lost by the agricultural self-employed, 48 were gained in nonfarm self-employment (1954–76, same sources as for n. 3).

scanty[6] they have suffered from missed opportunities at most—certainly not from economic disaster. The old middle class stood by, alive, often combative, although shrinking in relative size, while economic development was presenting it with a rapidly growing potential ally. To this point I now turn.

III. MANUAL AND NONMANUAL LABOR

The distinction between manual and nonmanual labor is constantly played down both by conservatives—who seem to believe that economic progress will bring manual conditions up to nonmanual standards—and by orthodox radicals, who are apparently convinced that, under capitalism, manual conditions have extended, or will extend, to most nonmanual employees.[7]

Yet differences between the two groups have always been and continue to be very wide. Manual workers are exposed to fatigue, dirt, smoke and other harmful substances, noise, and heat, and to risks of disease, injury, and death unknown to nonmanuals. Data taken from published statistics for several countries show that the rate of absenteeism for illness and accidents is much higher for manual workers; job satisfaction is much lower; so is their life expectancy. These data pertain mainly to physical conditions of work and to their effects. But nonphysical conditions too are much worse for manual labor. Their unemployment rate is higher; they are much more subject to discipline and work measurement practices; their career patterns are more limited. This list is far from exhaustive. To be sure, no distinction is clear-cut. There are many border areas, both in definitions and in conditions of work. Instances lending themselves to subtle distinctions will always be there. Still, would one deny the drop in living conditions just south of the California border by referring to San Diego ghettos or some rich villas around Tijuana? The manual condition defines a second-class life, penetrating into each and every experience of men and women: even the probability of stillbirths is higher in manual families, and the average weight of their children at birth is lower (Central Statistical Office 1975, p. 26, table 7.3; see also Office of Population Censuses and Surveys 1978, appendix 2; Desplanques 1974).

[6] The lack of data in this field is discouraging. Information on some professions is available in the United States only until the 1950s. Doctors seem to have improved their relative position relative to manual workers, while lawyers and dentists have lost some points. Shopkeepers' incomes are something of a mystery everywhere. It is to be noted, however, that relative losses vis-à-vis the wage earners do not entail economic debacle. Top nonmanual employees have also lost some relative points. While this may be the result of educational advancements, and is a desirable event, it would be arbitrary to conclude that it represents a signal of their proletarianization (why not, instead, a signal of the embourgeoisement of manuals?).

[7] For contrasting opinions see, on one side, the references quoted in Goldthorpe et al. (1969, p. 9) and, on the other, Aronowitz (1973).

Many radical authors, Braverman (1974) among them, approach the question by pointing to the large number of *new* alienating low-level nonmanual jobs created by capitalist development; conditions in such jobs, they believe, resemble those in manual occupations. There is a triple source of confusion here.

First, the alleged trend toward an increase in the number of low-level nonmanual occupations relative to middle- and high-level ones is not confirmed by the available data. What scanty data exist show that higher-level nonmanual jobs grow everywhere at least at the same rate as low-level ones, and always more rapidly when only males are considered (see table 2). This biased distribution by sex leads to my second point.

Here, again, there is confusion between the importance of absolute and

TABLE 2

WORKERS IN CLERICAL AND SALES OCCUPATIONS
(United States, Britain, and France)

	SALARIED EMPLOYEES[a] IN CLERICAL AND SALES OCCUPATIONS (%)				
	1900	1950	1960	1972	1977
Males and females:					
United States	64	69[b]	66	63	61[c]
United Kingdom	...	65	62	...	...
Great Britain	...	...	72[d]	67[e]	...
France	...	57[f]	...	...	49
Males:					
United States	69	64[b]	58	49	46[c]
United Kingdom	...	58	50	...	...
Great Britain	...	...	65[d]	56[e]	...
France	...	47[f]	...	...	36
Females:					
United States	50	75[b]	75	74	72[c]
United Kingdom	...	72	72	...	...
Great Britain	...	...	78[d]	75[e]	...
France	...	70[f]	...	...	61
Female labor force participation rate:					
United States	20	30	37	...	47[g]
United Kingdom	31[h]	...	51	...	57
France	...	...	45[i]	...	53

SOURCES.—U.S. Bureau of the Census 1975, Ser. D 182–232 (1900–1960) and D 36 (1900–1950); 1977, tables 660 (1972–77) and 626 (1960–76). UK-GB: Ministry of Labour 1967, tables Ia and IVa; General Register Office 1966, table 19; Office of Population Censuses and Surveys 1975, table 29; Department of Employment 1971, table 109; *OECD Labour Force Statistics*, 1967–68 (Paris: OECD, 1979). France: Thevenot 1977, table 1; *OECD Labour Force Statistics*, 1956–66 and 1967–78.

[a] Excluding managers.

[b] 1960 classification.

[c] April.

[d] Socioeconomic group 6 over SEG 4–6, England and Wales, 1961.

[e] SEG 6 over SEG 4–6, 1971.

[f] 1954.

[g] 1976.

[h] 1901, ages 20–24.

[i] 1962, ages 15–64.

relative figures. Even if it could be proved that the fraction of low-level nonmanual jobs increases over time, the increase would not necessarily mean that a proletarianization of the nonmanual middle class was under way. This class might be reproducing itself in the higher nonmanual occupations, with low-level nonmanual jobs increasing more rapidly via an influx of upwardly mobile ex-manual workers or of previously inactive persons. The former category, however, does not move into nonmanual jobs as frequently as the latter. What normally happens is that women and other disadvantaged groups such as youth (but relatively few blacks and few immigrants, as these are to be kept in the manual domain) have increasingly been hired to fill clerical and sales posts[8] at salaries lower than the highest manual wages, which mainly accrue to males. This process has simultaneously lowered the gap between *average* manual and nonmanual pay and allowed more women into the labor force. Countries such as Italy, where for various reasons (regulations against pay discrimination, union opposition to part-time jobs, etc.) this was not possible, have a much lower female activity rate and a much higher manual-nonmanual average pay differential.

However, and this is my third point, even the most degraded nonmanual job is normally performed in an office environment, so that the physical conditions of work are far superior to most manual ones. This is an empirical issue, which should be dealt with by more accurate studies than now available. For present purposes table 3 shows, for selected U.S. indicators, the differences between *lower* nonmanual and other occupations. The table shows that operatives have an accident rate more than four times as high as that of clerical workers. Work days lost because of illness and chronic illness rates are also considerably higher. Moreover, operatives' earnings are lower than clerical earnings when sex is controlled for.

Although the evidence of table 3 is impressive, I do not claim it to be conclusive. However, I have never encountered evidence giving different indications.[9] All authors agree—and it would be difficult not to—that some

[8] The feminization of lower-level nonmanual jobs can be inferred from the data and sources of table 2. If one calculates an index a/b of unequal female representation in those jobs, with a being the ratio between women in clerical and sales occupations and total salaried women and b the same ratio for men, this index is on the rise everywhere (by 1/3 in the United States between 1950 and 1977).

[9] For better results one should really be able to penetrate the occupational classification cells to see whether, within each of them, the actual content of jobs is being degraded. It must be remembered, however, that the degrading of an existing job is one thing, and the creation of a new job, less skilled than existing ones, is another. Even if automation has made some jobs more repetitive and, in a sense, degraded, many of the new jobs would simply not have been created without it. Thus, first, one should see how many new skilled, high-level jobs were originated by the need to devise, introduce and organize automated processes. And, second, one should keep in mind that, whatever the content of the new lower-level jobs, they are nevertheless nonmanual and thus represent an improvement in average conditions of work.

lower nonmanual jobs are just as boring and repetitive as assembly-line occupations. But this is only one dimension of the overall conditions of work. As Lockwood (1958, p. 81) put it, "Because of the rigid division between the 'office' and the 'works' it is no exaggeration to say that 'management' from the point of view of the manual worker, ends with the lowest grade of routine clerk" (see also Townsend 1979, table 12.2). At any rate, I will assume here that workers would assign, on average, a higher mark to nonmanual occupations, on a preference scale ranking according to *physical* conditions of work.[10] What would this imply for a theory of class interests and class alliances?

TABLE 3

AN OVERALL COMPARISON BETWEEN U.S. CLERICAL AND OTHER OCCUPATIONS: SELECTED INDICES (Clerical Workers = 100)

	Professional, Technical	Clerical	Operatives	Nonfarm Laborers
Physical conditions of work:				
a) Accident rate	121	100	435	714
b) Work days lost	80	100	127	129
c) Chronic illness	100	100	130	155
Unemployment:				
d) White males, rate	55	100	203	306
e) Black males, rate (white clerical = 100)	106	197	326	403
Education:				
f) 12 or more years (%)	118	100	68	43
Occupational segregation:				
g) Differential representation of whites (females)	87	100	51	65
h) Representation of foreign-born persons	129	100	203	149
Earnings:				
i) Median annual, males, 1956	137	100	96 (112*)	77
j) Median annual, females, 1956 (male clerical = 100)	83	71	59	...
k) Median annual, males, 1976	129	100	91 (106*)	79
l) Median annual, females, 1976 (male clerical = 100)	86	63	52	59

SOURCES.—For *a* and *c*, U.S. Department of Health, Education, and Welfare, National Center for Health Statistics, Ser. 10, no. 21, *Selected Health Characteristics by Occupation*, July 1961–July 1963; for *b*, Department of Health, Education, and Welfare, *Disability Days*, 1975; for *d* and *e*, U.S. Department of Commerce, *Social Indicators*, 1976; for *g*, U.S. Department of Labor, Bureau of Labor Statistics, *Handbook of Labor Statistics*, 1978; for *i* and *j*, U.S. Bureau of the Census, *Statistical Abstract of the United States*, 1958; for *k* and *l*, ibid., 1977, all published by the Government Printing Office. For *f* and *h*, see Wool 1976.

NOTE.—*a*, average *N* persons injured while at work aged 17 and over, per 1,000 in labor force, June 1961–June 1963; *b*, days of work lost owing to disability, per 1,000 in labor force, 1975; *c*, persons with one or more chronic conditions, with limitations of activity, per 1,000 in labor force, July 1961–July 1963; *d* and *e*, 1974; *f*, percentage of experienced civilian labor force, 16 years and over, with 12 or more years of education, 1970; *g*, ratio between (employed whites in each subgroup/all employed whites) and (employed blacks in each subgroup/all employed blacks), females, 1977; *h*, percentage of experienced civilian labor force, 16 years and over, born outside the United States, 1970.

* Craftsmen, foremen, and kindred workers.

[10] Some would argue that what matters in class analysis is not what people think, or the *subjective* dimension, but what the relations of production really are, or the *objective* dimension. To this it has been answered that ideological relations are, also, objective (Przeworski 1977, p. 368). And, as I will argue later, relations of production should be

IV. THE THEORY OF COMPENSATING PAY DIFFERENTIALS

Nothing can be said, of course, unless the other element of job differentiation is brought into the analysis: pay. At least in theory, a structure of Smithian compensating wage differentials can be envisaged so as to render workers perfectly indifferent to the job they hold. This is a very intricate matter. I will now try to explore its theoretical underpinnings in some detail.

Assume, first, that workers have identical abilities and tastes. Assume, moreover, that jobs differ only as to physical conditions of work, and that workers are willing to trade working conditions for pay. In such an abstract world, if workers were free to move in search for the best occupational deal, *relative* pay would be determined, in the long run, by relative physical conditions of work,[11] and people would be totally indifferent to the jobs they perform. This is because jobs offering better pay relative to their physical working conditions would be flooded with applicants and vice versa, relative wages adjusting so as to eliminate differentials not compensating for different conditions.

Let us now gradually relax the assumptions. First, physical conditions may be so disagreeable in some jobs that the associated high level of pay would eliminate profits. As, with prices constant, profits depend not on wages, but on unit labor cost (the ratio between labor cost and productivity), some margin is offered by the possibility of reducing the length of the working day. In other words, a job which would not find applicants *and* remain profitable at $10 an hour for eight hours a day, may find applicants and remain profitable at $8.00 an hour for four hours a day, depending on the relative productivity of the two shifts. If this is not possible, either those jobs would disappear or product prices would have to change, subject to limits set by market power or international competition. The end result would thus depend on whether people, as consumers, were ready to pay high prices for products requiring high wages for them as producers.

Second, jobs may differ in nonphysical conditions of work, such as career opportunities or prestige. The results in this case are the same: pay and *all* job features would be taken into account by workers in their choices, and compensating differentials would hold.

Third, jobs may differ as to the level of skill they require. Under the assumption of equal abilities, many specific skills would still be costly for

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seen as something more complex than those between capital on one side and a homogeneous labor force on the other.

[11] Absolute real wages, or command over commodities, may be thought to be determined in such a (capitalist) world by the reproduction needs of workers receiving the lowest money wage. The wage structure would then be anchored to that subsistence level of pay.

the workers to acquire. Compensating differentials would then have to incorporate those costs. Disagreeable jobs requiring costly worker-financed training would be paid more than equally unpleasant but less skilled ones. A job which was pleasant and required a lot of training, relative to one both unskilled and unpleasant, would be paid less on the first count and more on the second, the net result depending on whether the higher cost of training for the first job was larger or smaller than the monetary value attached by all workers to the greater unpleasantness of the second.

Fourth, workers may differ according to their native ability to perform certain tasks or to acquire the necessary training for "proving" that they can perform those tasks. Here pay differentials need not be compensating. If, for instance, the number of people able to perform a certain job were naturally limited, these people would be paid a more than compensating wage.

Fifth, workers may have different tastes. If—in Smith's example—you can find enough people who enjoy the hangman's job, you will not have to pay too dearly to fill the available positions. Here the concept of equalizing differentials loses much of its applicability.

Bourgeois ideology tends to use these concepts in a characteristically apologetic way. Differences in working conditions are understated. When they are acknowledged, the tendency is to say, for example, that bad working conditions not compensated by higher pay are associated with substantially less training;[12] or that some workers enjoy risky or dirty jobs which do not make them "think"; or that the long-run tendency is toward compensating differentials and that one should just wait long enough to see compensation work; or that differentials reflect the existing distribution of abilities (which implies that people deserve what they get). All this is hardly surprising. What is surprising is the lack of an articulate radical position on this subject. I believe the lack derives from two main causes. The first is the already mentioned emphasis radical writers place on the concept of proletarianization. The second is the position of intellectuals and academics within the class structure. I will come back to this point later. Let us now see what implications may be drawn from the analysis presented above.

The existence of noncompensating differentials in the real world depends less on differences in abilities and tastes than on the presence of obstacles to the movement of workers between occupations, both intra- and intergenerational. The economic literature has long recognized the existence of these barriers and has formulated, but not sufficiently developed, the con-

[12] This is the direction in which the so-called functionalist theory of stratification seems to move (Davis and Moore 1945). The functionalists stress differences in training and ability. See also Cullen and Novick (1979) whose study, incidentally, bears close resemblance to that by Lucas (1977), indicating some degree of unacknowledged convergence between conventional economic and sociological approaches to the problem.

cepts of barriers to entry and noncompeting groups (Mill [1868] 1929, p. 393; Cairnes 1874, pp. 67–68; Taussig [1911] 1939, p. 228). Equalizing differences may exist within very specific job clusters. For instance, manual workers who dig underground tend to be paid more than those who work in the open air for the same mining firm. On the whole, however, barriers to entry are very strong when broader groups are taken into account.[13] My arguments in what follows are based on the assumption that barriers to entry prevent the actual structure of pay differentials between manual and nonmanual occupations from compensating for their different conditions of work. I will assume, in other words, that—given the present structure of pay—nonmanual jobs are preferred to manual ones by the great majority of the working population.

This does not mean, for instance, that a relatively highly paid manual male would choose to leave his job for lower-paying clerical work. Such comparisons would be inappropriate owing to sexual discrimination and other considerations (among them the fact that the need to rationalize one's own destiny can lead to declaring undesirable what is perceived as impossible). It means, instead, that on average both manual and nonmanual males prefer their sons to perform nonmanual jobs, given present disparities and the existence of sexual discrimination.[14] Their hopes represent, respectively, advancement in or preservation of status, deriving from the combination of higher pay and better conditions of work, and the social judgment those two factors tend to bring with them.

V. REPRODUCTION MECHANISMS IN THE SOCIAL DIVISION OF LABOR

To speak of noncompensating differentials is to speak of the social division of labor. Without them, the division of labor would reduce to the narrowly technical problem of how many tasks are performed in each occupational slot. Dangerous, repetitive, or "de-skilled" jobs would either be paid adequately or disappear. This might not eliminate differences in prestige, but would seriously undermine them.

On the other hand, *with* noncompensating differentials, the job-allocation mechanisms, or the social division of labor, become crucial to the creation of interests and the formation of alliances. To start with, non-

[13] For a recent opinion see Fogel (1979, p. 35). Lucas (1977), using the "hedonic wage" approach, finds that wages tend, ceteris paribus, to be higher when the job "involves mostly sitting down and never lifting more than ten pounds."

[14] Only 23% of Goldthorpe's "affluent workers" who answered the question on the preferred type of occupation for their children expressed a preference for manual jobs. For white-collar workers the percentage was less than 11. And the perception of opportunities must have played some role in the results, as it would be difficult to explain why more answers were not concentrated on "higher professional and managerial careers" (Goldthorpe et al. 1969, p. 131, table 16; p. 219, no. 6[a]).

compensating differentials imply that "good" and "bad" occupational slots exist. This, in turn, implies that some workers, who would be willing to retain their "good" slots even after a reduction in pay, have a vested interest in preventing that reduction from taking place. If the pay structure is seen as permanent, those workers will also have a vested interest in their own reproduction, that is, in reserving those privileged jobs for their children. And if the biased allocative mechanism is operated successfully, the associated pay structure *will* become permanent. This is no small loot to plunder. And all it takes is the erection and defense of barriers to entry into those occupations.

These barriers predate capitalism and can be found under varying labels everywhere. They represent indeed the most widely used and universally successful type of manpower policy. The problem is how to force some groups of workers to take up the worst jobs and bequeath them to their children. A caste or a slave society does this very effectively. Western democracies, however, must use other methods.[15] The most efficient of these is, of course, the control of education, a control which has gradually lost its crude prewar features (see Hogben 1938, chaps. 8–10) and has become increasingly sophisticated.

An analysis based on orthodox economic theory would easily explain why low-income families may be expected to purchase, ceteris paribus, less education than the more well-to-do (Becker 1967). This is due, among other reasons, to the existence of an "imperfect" capital market which makes borrowing more difficult and expensive for the poor and to the effect of family environment on educational achievement. The discrepancies ensure that jobs "requiring" little education are allotted to offspring of low-income families. But when economic development increases average income and, at the same time, social pressure from below keeps the cost of education down through subsidies of various kinds, the income-barrier method of job allocation is threatened (although by no means abolished; see Bowles [1971] 1977; Sewell 1971).

At this point other "corrective measures" may be introduced, or ancient mechanisms reinforced, to supplement it. For instance, educational requirements may move up irrespective of technological needs. More people acquire basic education? Why not increase the fraction of jobs requiring higher education? This method presents the added advantage of being legitimized as the inevitable fruit of technical progress (Miller and Reissman 1969; Berg 1970). Or "an educational sorting process which appears neutral but in reality favors children from relatively privileged families"

[15] These methods do, indeed, work. Hauser et al. (1975, p. 585) remark that "in this century . . . there has been essentially no trend in the relative mobility chances of American men whose fathers held differing occupations" and that "changes in the occupational structure are the only sources of systematic variation in intergenerational occupational mobility."

may be relied upon; such a process "tends to perpetuate existing status differences" (Karabel and Astin 1975, p. 383). Educational quality may remain differentiated by region or area, so as to retain better facilities for the privileged group (Bowles [1971] 1977, p. 244; see also Barkin 1975). If public education is made more accessible, the qualitative gap between it and more expensive private schooling may be allowed to widen. Or the cognitive aspects of education may be made more abstract, more divorced from production needs. This would have some positive effect in a world of equals (Dore 1976), but in the present context makes for an increase in the consumption element of education, an element which, again, only higher incomes can afford.

All this, however, may still not be sufficient to keep manual families from sending their children to compete in the market for nonmanual jobs. But other methods can be found. The job allocation process can be partly transferred outside the schools, being made to rely on class loyalty and values, or even on accent and physical appearance. This is often "justified" by employers, who cite the worsening quality of education provided by a system in which public expenditure either fails to keep up with the rise in student population or is allocated to keep an increasing army of teachers in inadequate facilities. Also, the loss of surplus labor in the manual group can be compensated for by accelerated reliance on immigrants or minorities. But even this may not be enough. It may be necessary to turn to a second line of defense, which is the subject of what follows.

The privileged position of nonmanuals is justified, in the present system, by their relative scarcity. The most effective method to ensure the persistence of this scarcity is that of limiting supply but, if this is not enough, an increase in demand will do. Of course, the effects of such an increase may be devastating to privileges, unless the whole process is carefully controlled. Higher-level nonmanual jobs must remain sheltered. But an expansion of the "upper middle" layer of occupations, those just within the nonmanual side of the division, can be accommodated by the system. One of the most interesting phenomena related to occupational censuses is the continual change in classifications, only in small part due to statisticians' perfectionism (or sadism). In the nonmanual domain 20 years are sufficient for a census occupational classification to become obsolete. New occupations are mushrooming, one of their main features being that their salaries are only analogically comparable to existing ones. In part this is the result of changing technology but, of course, even technology is not neutral.

Thus both upper- and lower-level nonmanual occupations increase. But while the former continue to provide much more than compensating salaries, the latter both (i) entail a reduction of privileges relative to manual jobs and (ii) represent an outlet for nonblessed entrants into the labor

force, such as women, and for upwardly mobile children of manual families. If one compares conditions of work, the new occupations in the latter group are still far superior to most manual ones: but their pay has come to reflect, if only in part, that superiority.

The new petite bourgeoisie has conflicting interests regarding those jobs. Their creation prevents unwanted competition from exercising its effects in the sheltered domain and, for this reason, they are welcomed. But, at the same time, wealthier workers feel, sometimes wrongly, that owing to progressive income taxes many of these jobs, being in the public sector, are paid mostly out of their pockets, while their inferior overall status makes them attractive only to upwardly mobile persons. When economic and social conditions make the middle class believe that competition can be averted in other ways as, for instance, during a slump, that class turns against the increase in job-creating public expenditure.

I will now turn to another way in which the beneficial effect of increasing educational opportunities on discrepancies in pay and working conditions can be vitiated. An accelerated inflow of educated youths would reduce relative nonmanual pay in the ports of entry into the nonmanual career. However, on-the-job training and seniority privileges would still shelter mature nonmanuals from direct competition from manual ranks and from the youths. Thus another strategy may be applied, by which income distribution by age is made to shift in favor of the middle and senior positions in nonmanual occupations. If career advancements were independent of class, this would amount to an intergenerational conflict within the nonmanual domain. But if internal promotions can be made to rely on class origin, the middle class would have managed to retain its privileges simply via control of careers and an adjustment of the pattern of lifetime income distribution. This seems to be one of the possible explanations of the much steeper age-income profiles found in nonmanual occupations.

In the real world all these methods are applied simultaneously. The most successful capitalist economies have been those in which higher-level nonmanual jobs have increased most rapidly. Low-level jobs in that group, however, have also increased. This has allowed for some upward mobility of manuals and for an increase in the labor force participation of women. Most of these workers are channeled, via educational or other types of discrimination, into low-level posts which, however, still represent an advancement in terms of compensating pay relative to manual jobs, to which the majority of "manual" children are still confined. "Nonmanual" children, especially if male, are instead "tracked" via education and other methods, among them the control of careers, toward the middle and higher ranks of the nonmanual group, where income rises much more rapidly with age.

VI. NONMANUAL WORKERS AS A CLASS

This brings us to the sociological implications of this analysis. Nonmanual workers are, on the whole, a privileged group, and their privileges can be, and are, defended in several ways. But why should they constitute a class? After all, it has been argued (Wright 1976, p. 20), sexual and racial differences also create "important divisions within the proletariat." But unless these divisions "correspond to different actual relations of production, they do not constitute criteria for class boundaries in their own right."

Even within Marxist categories, as Ossowski (1963, pp. 76–81) has shown, the definition of class as a group determined by its relation to the means of production does not give rise to a unique scheme of social structure. And one could argue that the existence of a privileged group of jobs *is* part of a society's relations of production inasmuch as employers' decisions determine, via technology, the distribution of available jobs by characteristic, and joint employer and nonmanual control over the educational process and requirements determines the allocation of jobs among workers. Moreover, one of the few contributions of the economics of education[16] has been the notion of a productive resource, knowledge, or human capital, embodied in educated workers. If control over the acquisition of this very special form of capital is also class determined, can Marxist analysis afford to keep ignoring the effect this has on class formation and alliances?

However, Marxist method is one thing and Marxist categories are another (Lukács 1971). It is reasonable to argue that the latter are inadequate for dealing with something Marx himself never encountered. Nonmanual, propertyless labor is mainly a product of this century (see Soares 1966). England, the only country where the self-employed had already reached their "mature economy" level of 10% in 1900, came out of World War II with less than one-sixth of her manufacturing workers engaged in nonmanual jobs. At the turn of the century nonmanual workers probably accounted for less than 5% of the manufacturing sector and no more than 15% of the entire economy. In all other countries there were even fewer. They are now a near-majority in most advanced capitalist countries. "The true character of capitalism has to be rediscovered by each new generation" (Blackburn 1972, p. 164).

This is the message delivered in recent work by, among others, Poulantzas (1975) and Ehrenreich and Ehrenreich (1979). They correctly recognize the existence of some class "in the middle," which cannot be unambiguously assimilated to the two polarized ones, the owners of capital and the propertyless nonmanual workers.

My points so far can be synthesized into a threefold argument. First,

[16] For recent criticism by an early advocate, see Blaug (1976).

where the dividing line lies is an empirical concept, to be explored on the basis of a theoretical argument: the criterion of equalizing pay differentials. Jobs paying more than compensating wages are likely to generate in their holders interests distinct from those which pay less than compensating wages.

Second, this does not mean that each occupation can form a class, defined as a group of individuals who relate to the production process in similar ways (Bowles and Gintis 1975, p. 67) and thus share the same economic interests. The most convenient, though necessarily rough, way to trace a divide on this ground is to use the manual-nonmanual distinction. The reason is that the presence of noncompensating differentials between these two groups of occupations is related to the occupational structure of production and the social mechanisms deriving from it and leads to different sets of interests and different "global" experiences of work and leisure. No other distinction is, on this count, so clear-cut. If blacks perform, for instance, nonmanual professional jobs, they are acknowledged members of the new petite bourgeoisie. Racial discrimination, however, causes this to be a rather rare event, as blacks are normally forced into the manual domain. An educated black youth is still black. It is still possible, and easy, to discriminate against him. On the contrary, a white educated son of manual workers has few differentiating characteristics which can be used to prevent him from competing for nonmanual jobs. Hence the need for privileged groups to enter into class alliances aimed at sheltering nonmanuals from unwanted competition. Racial discrimination is not much more than a handy support of class barriers. If blacks did not exist the bourgeoisie would have to invent them.

Third, the way in which the interests of the new nonmanual petite bourgeoisie are pursued is through an alliance with the bourgeoisie, which "pays" for the political support accorded to the system by privileged workers by making sure that institutional mechanisms are biased toward the continuation of those privileges. This price is not a high one, as the same mechanisms are used by all fractions of the bourgeoisie to support their own class-reproduction mechanisms.

None of this, it must be noted, implies that there is something inherently just or desirable in a world of compensating differentials. But when a system is allegedly based on the working of the market mechanism, compensating differentials, ensured by the maximum amount of interoccupational mobility compatible with differences in ability, represent one acceptable long-term solution to the problem of the social division of labor. Other, more desirable, solutions might be found in different social contexts in which, for instance, job characteristics and pay differentials were socially determined and the nature of the association between each individual

and his or her job was less that of the lifetime "marriage" it represents today. This example brings us to the next point.

VII. NONMANUAL WORKERS AND CLASS ALLIANCES

Common exploitation by capital might suggest, instead, an alliance of all workers against the exploiter. But this would necessarily entail a reshuffling of the existing division of labor and the disappearance of privileged conditions of work and pay. Those potentially damaged by the change should then be persuaded that the new alliance could bring about benefits of various kinds which would more than compensate for their losses. Persuasion would require the specification of a theoretical model of (*a*) the way in which the economic and noneconomic benefits and losses deriving from the expropriation of capital would be allocated among today's wage and salary earners and (*b*) future job contents and allocation mechanisms implied by the new alliance as an alternative to present, biased ones.

This point is almost totally neglected by the Left, even in its debate on the transition to socialism,[17] and yet it is crucial to all discussions of alliances. When, for instance, Aronowitz (1979, p. 221) criticizes Ehrenreich and Ehrenreich by pointing out the lack of political self-representation and of a common "vision" or program in the so-called professional-managerial class (PMC), he comes close to the issues, but misses both points. The PMC and, indeed, the whole nonmanual group *has* found its political representation in those center or left-center parties or groups which have always rejected an alliance with the traditional Left and have entered government coalitions in all Western countries. They are, normally but not always, "liberal," reformist parties, geared toward the rationalization of the capitalist system, supporting state intervention, within limits, as a means of broadening consensus on a form of society they fundamentally accept (for data relative to several countries see Lipset [1966]). Without entering into the intricacies of the "class vote" (Alford 1963; Przeworski and Sprague 1977; also Bonham 1954), I will only remark that if "the white collar workers . . . are much more likely to vote for the more conservative parties than are manual workers" (Lipset 1966, p. 416), even without accounting for the numerous and omnipresent "working class Tories" (Nordlinger 1967), an alliance among nonmanual labor, the old petite bourgeoisie, and capital would enjoy a theoretical majority in all advanced capitalist countries and has been in this position for something like three

[17] The failure to go beyond a few lines on the necessity of overcoming "the objective conditions of the social division of labor between managers and managed" by "among other things" "a radical shortening of the working day" is apparent, for instance, in Mandel (1974).

decades. For a biased social division of labor, authoritarian rule is no longer a must in those countries.

And it may be precisely the lack of a clear "alternative vision" which prevents nonmanual workers from considering different alliances for different solutions. But who is to offer such a vision, if not radical intellectuals, who generally happen to belong—from the economic if not the ideological standpoint—to the same class as the nonmanual workers? Can the existence of objective contradictions between economic and ideological class locations present an obstacle to the full formulation of the vision? These are old questions, which means, of course, that they have never been answered. At any rate, the middle class *has* something to lose besides its velvety chains.

And yet it might accept the loss, for something of value. Many "mental" workers might be intellectually attracted by a society where they could hunt in the morning, fish in the afternoon, and criticize after dinner. Many might think that a more just society is more inherently stable, and thus worth some losses. Many might, however, simply refuse to consider a change because—as Edwards (1979, p. 213) has it—"no left party has constructed a satisfactory transition strategy that both defends (and utilizes) democratic government and provides for a transition to socialism." They wonder how prescriptions would change in societies where hunters and fishermen are not important fractions of the labor force. And they see the experience of self-styled socialist countries where those who criticize, before or after dinner, are jailed by omnipotent bureaucrats who purchase assembly lines from the West with one hand, while reserving the best schools for their own children with the other. In the West, slogans on the dictatorship of the proletariat—a patent anathema for the middle class—die hard. Socialist utopians are forgotten or ridiculed by the "scientific" Left. The thirst for an alternative model to struggle for is met with internal ideological purges.

Thus as long as the creation, design, and allocation of jobs remain in the hands of the ruling alliance, the propertyless but privileged nonmanuals accept and support the system granting them privileges and legitimize it with the undeniable absolute advancements brought about by capitalism, claiming that these advancements will continue until they reach the lower strata of society. They can wait, and do.

VIII. SUMMARY AND CONCLUSION

In this study I have argued that the "orthodox radical" account of the proletarianization of the middle class is misleading. In particular, it is misleading to claim that "workers" have become "an overwhelming fraction of the labor force" (Edwards 1979, p. 208). If the empirical coun-

terpart of the old petite bourgeoisie is cleared of all those who owned no capital or, when they did, had no real "command over their immediate conditions of work," one might say that the "workers" have always been a majority, even when hidden under self-employed status.

It is also misleading to say that "for middle-layer workers today . . . the loss of control over the labor process has been as complete as for other fractions of the working class" (Edwards 1979, p. 193). Today's middle layers had no rosy yesterdays. In fact, they had no yesterday at all as most of them, simply, did not exist. They were not proletarianized, they were created. Furthermore, to say that they now have the same interests as manual workers is to neglect the effects of relative conditions of work and pay on class consciousness and behavior.

Along with an impressive increase in high-level nonmanual jobs, normally assigned to males, capitalism has created a vast number of entirely new occupations in the lower nonmanual group, whose pay and working conditions are unfavorable relative to previous nonmanual standards but which, being increasingly assigned to women just entering the labor force, do not lend themselves to meaningful comparisons with the past. Two-income families have also mushroomed, and only careful study of the occupational patterns prevailing in such families and of the political orientation of nonmanual females[18] can clarify the implications of these changes.

The most significant change to occur in this century was not a shift from a society of independent producers to one of dispossessed and alienated workers. It was, instead, a change from a society in which most manual workers were "self-employed" to one in which most of them are wage earners; from a society in which most of the jobs were manual to one in which most are nonmanual; and from a society in which most women "stayed at home" to one in which a majority of them work outside the home.[19] It is certainly legitimate to ask whether a system other than capitalism would have created better conditions of work for everyone. It is not legitimate to imply that, within capitalism, conditions of life and work have worsened.

Using the classical concepts of compensating pay differentials and noncompeting groups, coupled with an analysis of barriers to occupational mobility, I have shown how nonmanual workers have at least one, fundamental incentive to ally themselves politically with capital and the old

[18] It may be interesting to note that, in March 1977, more than half of the working wives in the United States had husbands whose income fell in the upper and upper-middle quartile, and only 19% had husbands in the lower income quartile (Ryscavage 1979).

[19] Braverman (1974, p. 362) seems to look back somewhat nostalgically to a society where "beds were made, floors were scrubbed, food prepared and served, children minded . . . long before people were hired to do all these things." It remains to be shown that the condition of women has worsened because of the change from such a society.

petite bourgeoisie. The incentive is given by the opportunity to preserve the existing, biased, social division of labor which guarantees most nonmanual workers a more-than-compensating wage. An alliance between the manual and nonmanual groups would necessarily entail an agreement over a new, and more fair, division of labor in society.

If a class is defined as an ensemble of agents who, owing to their specific position in the production process, share interests aimed at the defense of or the attack on recognized advantages and who find in the organization of society the elements for their own reproduction, the new petite bourgeoisie may be said to constitute a class.

One of the most intriguing puzzles for the orthodox Left is the outcome of elections in Western democracies, where their theoretical working class should constitute much more than the majority of the voters, but where the parties claiming to represent working-class interests get a small fraction of the votes. That this is due to the workers' misinterpretation of their true interests is not easy to swallow, especially in the long run. The quandary is best exemplified by Wright's (1976, p. 23) reply to Poulantzas's (1975) escape from orthodoxy. On the basis of Poulantzas's definition, the U.S. working class would constitute less than 20% of the employed population. And "it is hard to imagine a viable socialist movement developing in an advanced capitalist country in which less than one in five people are workers." Hardly a sound argument with which to refute Poulantzas's work, at least as long as wishful thinking is not enough to make a theory.[20]

As we have seen, the middle class knows better than that. Its political behavior *does* derive from its class consciousness. What is at stake is nothing less than the social division of labor. Radical thought should abandon the old tools, which are based on the vain and somewhat consolatory hope that the middle class will some day acknowledge its unity with the proletariat before knowing what this unity might mean.

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[20] This does not mean that Wright's (1976) analysis on contradictory class locations is uninteresting. See, however, Przeworski (1977, p. 365).

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Unemployment over the Life Cycle: Racial Differences and the Effect of Changing Economic Conditions[1]

Thomas A. DiPrete
University of Chicago and National Opinion Research Center

An analysis of the unemployment experience of white and nonwhite male heads of household was conducted using methods of partial likelihood. The results show job tenure to be the most important determinant of unemployment experience but point to the complications involved in interpreting its effects when the sample studied has mixed levels of age and job quality. Nonwhites differ in unemployment experience from whites mostly at low tenure levels, but they tend to be more sensitive to recessions at higher levels of tenure as well.

With the increased unemployment rates of the 1970s and the present, the study of unemployment has assumed a new saliency. Demographic studies have shown how the incidence and duration of unemployment vary across major age, sex, and racial groups in the population. But although much has been learned about the constituents of the most important and visible statistic in this area, namely, the unemployment rate, a detailed understanding of the reasons for unemployment, and hence of workable policies with which to combat it, has not yet been fully obtained.

The purpose of this paper is to examine how the risk of unemployment of male heads of household changes over the work career, and how the patterns of change differ for whites and nonwhites. Specifically, it will explore the relationship between unemployment experience and age and analyze the extent to which this association is due to job tenure. Second, it will analyze the role of other job and household characteristics with regard to the level of risk. Third, it will show how the introduction of information about past work history can be used to reveal otherwise unmeasured differences in risk among workers, as well as to qualify the relationship between job tenure and unemployment risk. Finally, it will

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explore how aggregate differences in unemployment experience across racial groups can be analyzed to suggest systematic variation in the distribution of risk both with age and with the state of the economy. Because analyses of changing risk—whether due to individual or global circumstances—are most appropriately longitudinal, this investigation will be accomplished through the development of models which can make use of all of the information about a worker's career available in longitudinal data sets.

AGE, JOB TENURE, AND POPULATION HETEROGENEITY

In recent years there has been a great deal of interest in the problems that workers, especially disadvantaged ones, have in establishing stable careers. Studies of status attainment (Blau and Duncan 1967; Duncan, Featherman, and Duncan 1972; Sewell and Hauser 1975) and of earnings growth over the career (Mincer 1974) have established the basic tendency for workers to improve their occupational positions as they grow older. A general consensus seems to have been reached in the literature on the causes of this pattern. Workers are said to be rewarded for their level of resources (Sørenson 1977), a concept referred to in the economic literature as human capital (Becker 1975). Since resources tend to increase over the career through accumulation of experience, rewards do also. In addition, it has been argued that over the course of the career, the relationship between rewards and resources is strengthened through job shifts that improve the match between worker and job (see Becker, Landes, and Michael [1977] for an application to marriage, or Tuma [1978] for an application to job mobility).

Parallel findings have been obtained when another important indicator of the status of a worker's career, namely, unemployment experience, is examined. The negative relationship between unemployment and age has been well documented by a number of studies both in economics (Hall 1972; Gramlich 1974; Marston 1976; Leighton and Mincer 1979) and in sociology (Sullivan 1978; Clogg 1979). In the economics literature, this gross tendency has been explained in terms of the two basic types of unemployment: voluntary and involuntary terminations (the third source of unemployment spells, movement into the labor force, is of only minor importance in a study of household heads). Voluntary terminations decrease because workers learn more about the job market and find less need to "shop" for jobs. At the same time, family responsibilities increase the need for continuous income. Third and most important, as workers acquire job tenure and are rewarded for their specific skills by higher wages, they find it harder to locate other jobs which would pay as well. Conversely, the probability of layoff decreases with job tenure, both be-

cause of seniority clauses in collective-bargaining agreements and because employers usually value their more experienced employees more highly.

However, this crude relationship between unemployment and age masks the problems that low-skill and minority workers have in establishing rewarding careers. Black and Hispanic workers tend to have lower income and lower occupational status at the start of their careers, and their rate of growth in earnings is on the average lower than that of white men. This is true both because nonwhites have lower average education, and because those who do not go to college have lower rates of return for the schooling they do get (see Hoffman 1978; Rosenfeld 1980). Sullivan (1978) has shown how rates of underutilization, a concept which includes unemployment as one of its components, are lower for nonwhites than for whites at all age levels. The interaction between race and age in her data was weak, however, and in fact showed a slight tendency for black-white differences to decrease at older ages; this tends to support other observations that the difference between black and white unemployment is greatest at the younger age levels (Leon and Rones 1980).

Explanations for the different experiences of blacks as compared with whites have been based on two factors: the inadequate number and quality of jobs available in the local labor markets within which minority workers live, and discrimination. While human-capital theory predicts that workers attain stability of employment by acquiring skills so that employers gain a stake in holding on to workers, the relevance of this argument to minorities is diminished by two phenomena. The first is that, according to segmented labor market theories (e.g., Piore 1975), competent performance is attainable in low-skill jobs after such a brief period of training that employers essentially have no incentive to hold on to their workers; conversely, workers have little incentive to try to hold on to their jobs. Second, minority workers find it harder to obtain high-quality jobs because of discrimination. These difficulties can multiply over the career. Frequent unemployment caused by unstable jobs can increase the discouragement of black workers and decrease their attractiveness by giving them an employment history which makes them appear less interested in continuous work than they really are. But even though job instability can multiply future difficulties, it is apparent from the crude patterns that most blacks and whites do in fact attain greater continuity of employment as they grow older, while a small but significant fraction continues to experience work instability into the middle years of life.

Some of the variation in the experience of black workers has been explained through the inclusion of more detailed information about their educational and training resources, and about that part of their attachment to the work force that can be explained in terms of their current family situation, and by measures of occupational position for those working at

the time of a specific survey. But even after such important elements have been included in an analysis, much of the unemployment experience remains unexplained. Some of this variance is due to the kinds of chance phenomena that would not be of strong theoretical interest. But some is due to unmeasured characteristics of individual workers and local labor markets. The existence of this unmeasured heterogeneity can be seen by the association between unemployment events over the work careers of individuals in longitudinal data sets.

This unmeasured heterogeneity is of sociological interest for two basic reasons. First, a better understanding of the continuity of employment problems is needed in order to identify the magnitude of the problems that some workers must face and to determine the likelihood of workers being able to escape the disorganization that might affect part of their careers. Second, unmeasured heterogeneity can affect the relationships between more basic elements used in the explanation of career events, such as age and job tenure, because of the variation in its magnitude over the life cycle. These complications have not been fully dealt with in existing empirical treatments of the subject.

Heterogeneity affects the measurement of job tenure principally by upsetting the simple relationship among job tenure, job quality, and unemployment risk that exists on the individual level. Both human-capital and segmented labor market theories predict that the change which tenure produces in unemployment risk is greater for an individual in a better job. However, it does not necessarily follow that if one compares groups having different average levels of job quality tenure differences are more important for the group with the higher average level of job quality.

Unmeasured heterogeneity causes a relationship to exist between past and future spells of unemployment. While many social and economic factors related to unemployment experience can be measured and statistically controlled for, they certainly cannot provide a complete elaboration of the risk of unemployment. Therefore, one would expect a continuing net relationship between the existence of previous unemployment and job separation experience and the present risk of a spell of unemployment. (This relationship has been analyzed, using longitudinal data in an econometric context in the work of Heckman [1978] and Chamberlain [1979]; Ellwood [1978] has used econometric techniques to estimate the effect of past unemployment when controlling for the effects of heterogeneity, i.e., "pure state dependence." Clogg [1979] has analyzed unmeasured heterogeneity in unemployment, using repeated cross-sectional data.)

Furthermore, this persisting association will affect attempts to measure the influence of other variables, particularly job tenure, on unemployment risk. Obviously those who have been at their present jobs for a long time have not experienced unemployment (temporary layoffs excepted) in the

recent past; those who have only recently taken their present jobs may or may not have a history of frequent job changes and unemployment. Past turnover and unemployment, in other words, will be negatively correlated with tenure and positively correlated with further unemployment experience. Therefore, information about a worker's past experience with unemployment is needed in order to eliminate this bias in the size of the influence of job tenure.

First, let us consider how unmeasured heterogeneity affects this relationship. For illustrative purposes, let us suppose that the analysis was being done on two populations of workers, that the jobs of workers in each group were of uniform quality, and that the quality levels of the two groups differed. It is easy to see that there would still be sociological reasons (which might include personal attachment between employer and employee, or noneconomic satisfaction gained from the job) for expecting that some workers in the poor jobs would nonetheless enjoy considerable stability of employment. Over time, these workers—the "stable" group—would constitute an increasing proportion of the class of workers who had not changed jobs for some specified period and would therefore be a cause of its increasing aggregate stability. For this reason, workers who had accumulated tenure would on the average have greater stability in the future than would workers with less tenure. Of course, the same phenomenon would be expected for workers in the better jobs, but because of their overall lower rate of movement, the "stable" group here—again defined as those workers who have lower rates of separation than would be predicted simply from the quality of their jobs—would not as quickly increase its proportionate representation among the workers who had not changed jobs in a comparable period of time. Because of the more rapid dominance of the "stable" workers among those in poor jobs who had accumulated tenure, the difference in aggregate stability between the two classes could conceivably be greater than the comparable difference for workers in good jobs, even though one would argue that, ceteris paribus, the greater spread in job security should be found among workers in good jobs. If the latter group also tended to have lower turnover rates at early stages of tenure than do workers in poor jobs, the argument would hold with even greater force.

The real situation is obviously more complicated, because of differences in job quality within the populations of white and nonwhite workers. We can see, however, that even though the average quality of the jobs of nonwhite workers is lower, differences in tenure among them might be more significant indicators of unemployment risk. This would be the case if nonwhite workers with little job tenure were in situations (in which characteristics of the person, the job, and the local economy might all be salient) which gave rise to a significantly higher risk of unemployment

on the average than comparable white workers experienced. Then, if the risk for nonwhite workers with accumulated tenure was the same or even slightly higher than that for a comparable group of whites, the aforementioned result would follow.

To summarize, comparisons between white and nonwhite risks of unemployment will depend on the age distribution of the groups being compared. It seems likely that in a comparison of young workers both groups will tend to have a larger number of workers with high risk. The difference in risk between the two groups is likely to show itself across a range of tenure levels. As both samples age, it seems probable that high-tenured nonwhite workers will gradually behave more like high-tenured white workers, while the low-tenured group will increasingly be composed of workers who have long-term employment problems. Explanations for the continuing differences in unemployment experience of the aggregate populations could then take various forms. The difference might be seen as an otherwise unexplained racial effect that appears at all levels of tenure. Alternatively, it might be due to the fact that the distributions of job tenure become more dissimilar as the populations age. Finally, the bulk of the difference might be accounted for by the possibility that low-tenured nonwhite workers are actually more vulnerable to unemployment than are comparable groups of white workers. When attention is focused on sensitivity to economic downturns, the same possible explanations for greater nonwhite sensitivity (Gilman 1963; Gilroy 1974) arise again.

METHODS AND DATA

For the present study, I used data from the Panel Study of Income Dynamics for the years 1968–76 for male heads of household between 25 and 54 years of age in 1972. Because the use of career records with missing information involves a further order of complication which seemed inadvisable at present, I further restricted the sample to men who had reported at least five years of experience in the labor force and who had some employment experience between 1973 and 1976. The definition of unemployment was also limited by the exclusion from consideration of all instances of temporary layoff. Justification for such an exclusion is based on the work of Feldstein (1976) and others who have argued that many temporary layoffs should be and are considered by workers (many of whom take a long-term view of their income and employment) to be part of the job. Consequently, it has been argued, temporary layoffs do not carry with them the same uncertainties and consequences as unemployment that follows the termination of a job (whether voluntary or involuntary). This exclusion is made with some reservations, however, since the distinction between the two types is not always clear, let alone im-

portant. On balance, it is hoped, the use of the distinction will increase the interpretability of the results.

The analysis was carried out by methods which can be described generically as the estimation of hazard functions. Such methods have become familiar to sociologists largely through the work of Singer and Spilerman (1976) and Tuma, Hannan, and Groeneveld (1979; see also Hannan, Tuma, and Groeneveld 1977). In their analyses likelihood functions describing the probability process were proposed and estimated using records that contained complete data (specifically number and timing) about the events being studied.

In general, complete information is not available. Yet, in the past 15 years, data sets—notably the National Longitudinal Studies and the Panel Study of Income Dynamics—have become available which contain far more information than any preceding cross-sectional survey without, however, reaching the level of detail in a data set such as that being generated by the Seattle-Denver Income Maintenance Experiments. In order to analyze such data, a method which makes fewer demands on them is desirable.

Models based on the partial likelihood framework suggested by Cox (1972) are particularly useful for answering questions such as those with which we are concerned in this paper because they are capable of meeting a number of the needs of this sort of analysis. They are called partial likelihood methods because, as their name suggests, estimates based on them are independent of some of the information which would be present in the full likelihood function. Essentially, the likelihood function is factored into pieces—one that depends only on the order of events and one that depends on their exact timing. Maximization is done only on the former piece. Efron (1977) has shown that the Fisher information matrix of the estimates converges asymptotically to that based on the full likelihood.

Important and useful features of the methods can be expressed as follows. (1) They can allow the researcher to incorporate longitudinal information about the individuals studied in the model. (2) They can allow for dependence on independent variables that change with time by actually incorporating the changing values of each independent variable in the estimation procedure. (3) They can allow for a time-changing "baseline" representing unmeasured changes in the economic environment. (4) Because of the inclusion of past dependence, they can include in the estimating procedure the possibility that any individual has had multiple unemployment events in the data without having to treat the events as independent. In Cox's suggested use of these models in life-table research, the event of interest—death—can of course occur only once for each individual. It is not difficult, however, to transform the model slightly to

deal with cases in which an event can occur more than once. (5) And finally, they can allow for the fact that the data might be censored, that is, that the data records might not be complete. In fact, they can handle situations in which analysis necessitates the possibility of a number of censoring times. In the study of unemployment, for instance, an individual who has become unemployed cannot become unemployed again until he has first left the unemployment state. He is in effect censored during his spell of unemployment, and the method of analysis used here can take account of that fact.

Given the employment histories, the model would express the probability of a move into unemployment as a function of time. The probability that an individual would survive past a time "t" without unemployment could be written as $S(t) = \Pr(T > t)$, and the hazard, or time-specific failure rate, could then be denoted as $\lambda(t)$, where T is the time of the event, and

$$\lambda(t) = \lim_{\Delta t \to 0+} \frac{\Pr(t \leq T \leq t + \Delta t | t \leq T)}{\Delta t}.$$

The key to the method lies in the classification of the population according to whether each member could (because he was at that time employed) experience unemployment in that week. The group which could have an unemployment experience is considered to be "at risk" for that time. The group which is not at risk is defined as those workers who were already unemployed, were out of the labor force, or had left the study. The risk set, $R(t)$, is clearly a function of time.

Following Efron (1977), the full likelihood for the data could be expressed as follows

$$f(\text{data}) = \exp\left\{ - \int_0^\infty [\Sigma_{i \in R(t)} \lambda_i(t)] dt \right\} \cdot \prod_{j=1}^{J} \prod_{k=1}^{n_j} \lambda_{i_{jk}}(t_j), \qquad (1)$$

where λ_i is the hazard of the ith individual, t_j is the jth ordered event time, and n_j is the number of events occurring at t_j. As I said previously, the calculations are made tractable by factoring this likelihood function into two parts: that which specifies the timing of the unemployment events for the sample, and that which expresses the probability of any particular member of the risk set experiencing unemployment, given that an event has occurred. The components—also factors—of the partial likelihood factor can be understood as conditional probabilities that one or more members of the risk set experienced unemployment at a particular time, given the composition of the risk set. These factors can be seen to be of the form

$$\prod_{k=1}^{n_j} \frac{\lambda_{i_{jk}}(t_j)}{\Sigma_{l \in R(t_j)} \lambda_l(t_j)}, \qquad (2)$$

where n_j is the number of events occurring at t_j. The following functional form of the hazard rate is convenient and will be used in the analysis:

$$\lambda(t) = h(t)\, e^{\beta' Z(t)}, \tag{3}$$

where $h(t)$ expresses direct time dependence. To generate the partial likelihood, one would then take the product of all factors, where the number of factors equals the number of distinct times that someone in the sample experienced unemployment. The logarithm of the result can then be maximized by conventional procedures. In this analysis, the Davidon-Fletcher-Powell (Fletcher and Powell 1963) method was used to generate maximum-likelihood estimates of the beta coefficients. The factor $h(t)$, which expresses relationships between time and unemployment risk, was estimated by an extension of Breslow's (1974) procedure.

The use of partial likelihood methods requires that the data contain information about the correct ordering of all events, if not their exact timing. Unfortunately, this need was not fulfilled by the Panel Study of Income Dynamics. However, increasingly complete summaries about the events of each year covered by the study were available. Simulated work histories were therefore created for each individual which, when summarized, would always agree with the summary information contained in the data for each year. It was on these simulated work histories that the analysis was conducted.

EMPIRICAL RESULTS

Probability models were then fitted to the employment histories for white and nonwhite male heads of household, aged 25–54 in 1972. Each model was fitted to all employment experience during the four years from 1973 through 1976. The independent variables, selected to provide a minimal description of the job and family situation of the individual, were the log of age, marital status (coded as a dichotomous variable, "1" indicating married, zero not married), whether the worker had a disability that interfered with his work ("1" indicated yes), whether the worker was self-employed, whether he was employed by the government, the number of dependents, the log of the hourly wage (measured in cents and defined as the annual labor income divided by the annual number of hours worked), the amount of education (measured in years of schooling), the local unemployment rate (measured as the percentage unemployed), the log of the annual earnings of others in the family (measured in dollars), the log of the annual number of hours worked, and whether the worker had already experienced a spell of unemployment in that calendar year. All variables were lagged by one year, to reduce problems in interpreting the direction of causality. All were treated as functions of time.

Job duration was measured through a set of five dichotomous variables ($J_{-1}, \ldots J_{-5}$) for the five years prior to the current year. If the worker had had no recent job separations, all five variables were coded zero. Otherwise a "1" was coded for the year of the most recent separation. Hence a worker with no job separation in the previous five years had a tenure of at least 60 months in his present job at the time of the salient interview.

The results are presented in table 1. The left column in each set shows the size of the coefficients. In the right column of each set these coefficients are illustratively interpreted. The numbers indicate how the hazard would change if the appropriate variable were to increase in size by one unit. A 1.1 indicates that the hazard would increase by 10%, 1.0 indicates that no change would occur, .9 indicates a 10% decrease, and so forth.

The results reveal that, as expected, the frequency of unemployment spells diminishes with age and with increased job tenure. Furthermore, it can be seen that the tenure effect ($J_{-1} \ldots J_{-5}$) is clearly the largest in the model. All other things being equal, nothing explains the likelihood of a spell of unemployment as well as the tenure of employment in the present job. For white males a job separation in the previous year implies over six times as high an unemployment probability as that for a worker with over five years of tenure. Even a worker who changed jobs five years ago (J_{-4}) has 2.6 times the weekly probability of unemployment of the

TABLE 1

MODEL I

Independent Variables	White ($N=1,054$)			Nonwhite ($N=434$)		
	B	SE*	Exp(B)	B	SE*	Exp(B)
Log age	−.91	.05	.40	−.094	.004	.91
Marital status	.054	.04	1.1	−.38	.09	.68
Health	.33	.03	1.4	−.55	.10	.58
Self-employed	−.36	.04	.70	−.19†	.13	.83
Government-employed	−.81	.06	.44	−1.50	.20	.22
Dependents	.067	.007	1.1	.12	.01	1.1
Log wage	−.35	.01	.70	−.61	.05	.54
Education	−.10	.003	.90	.03	.007	1.0
Unemployment rate	.11	.001	1.12	−.016†	.013	.98
Log others' income	.033	.003	1.0	−.022	.008	.98
Hours worked	−.57	.02	.57	−.38	.03	.68
J_{-1}	1.87	.02	6.5	2.95	.08	19.0
J_{-2}	1.31	.035	3.7	1.83	.13	6.2
J_{-3}	1.19	.04	3.3	1.42	.04	4.1
J_{-4}	.95	.04	2.6	1.57	.13	4.8
J_{-5}	.99	.04	2.7	1.26	.08	3.5
U_0	1.91	.001	6.8	.63	.07	1.9

* Asymptotic estimate of the standard error.

† Less than twice the estimate of the standard error.

longer-tenured worker.[2] By comparision, a separation in the previous year for a nonwhite worker implies a chance for a spell of unemployment 19 times that of a long-tenured worker. To see the effect in another way, a worker with a 5% yearly probability of unemployment who has worked at his present job for more than five years would, if his job tenure were reduced to one year, experience an increase in the probability to .28 if he were white, but .62 if he were nonwhite, assuming all other factors were held constant.

The effect of age per se is much less important, but still significant and in the expected direction. An increase in age from 25 to 30 implies a decrease in risk for a white worker by a factor of .15 (that is, a 30-year-old will have a risk that is .85 times the risk of a person who is 25 years old with the same values on all other factors in the model), while a change from 30 to 35 causes a decrease of .13. To take an extreme example, white workers at age 50 have .5 of the risk of white 25-year-olds of having a spell of unemployment in a given week. For nonwhites, however, the decrease is much smaller. To illustrate, a 50-year-old nonwhite male has .94 the weekly risk of a nonwhite 25-year-old—certainly a small reduction. Naturally, since job tenure tends to increase with age, the zero-order effect of age would be much larger for both groups of workers. The decreased risk, though, is principally due to increased job tenure.

It was hypothesized earlier that there should be significant heterogeneity in the size of the effect of job tenure because its implications depend on the quality of the job. Thus, while we would expect the relationship between tenure and unemployment to be flatter for workers with poorer jobs, the effects caused by population heterogeneity would run in the contrary direction, leaving the net result unclear. From table 1 it seems that the heterogeneity effects are stronger than the pure tenure effects when we compare populations of white and nonwhite male household heads. In the aggregate, then, whites and nonwhites apparently differ in their risk of unemployment most strongly at the lowest levels of tenure.

Earlier we noted that a model which lacked measures of multiple job shifts and past unemployment experience might overstate the importance of job tenure. The results of estimating a model which included measures of the number of job separations in the past five years, and whether or not each was accompanied by a spell of unemployment, are presented in table 2. In this augmented model, U_0 is coded 1 if there was any previous

[2] Technically, λ is a rate that must be converted to a probability to be easily interpreted. The probability of an event occurring in a week, then, would be $1-e^{-\lambda}$, while the yearly probability would be $1-e^{-52\lambda}$. However, for small values of λ, $1-e^{-\lambda} \approx \lambda$. If the yearly probability of unemployment is .1, for example, this implies that λ is .00203, which is very close to the weekly probability of .00202. Therefore, as a good approximation, the incidence parameters can be thought of as weekly probabilities and changes in their value can be thought of as changes in weekly probabilities.

TABLE 2

MODEL II

Independent Variables	White ($N=1,054$)			Nonwhite ($N=434$)		
	B	SE*	Exp(B)	B	SE*	Exp(B)
Log age	−.82	.05	.44	−.086	.017	.92
Marital status	.014	.039	1.0	−.077†	.10	.93
Health	.26	.034	1.3	−.63	.09	.53
Self-employed	−.32	.04	.73	−.013†	.13	.99
Government-employed	−.77	.058	.46	−1.60	.20	.20
Dependents	−.065	.007	.94	.065	.014	1.1
Log wage	−.33	.014	.72	−.51	.05	.60
Education	−.093	.003	.91	.03	.007	1.0
Unemployment rate	.10	.005	1.1	−.019†	.013	.98
Log others' income	.030	.003	1.0	−.026	.008	.97
Hours worked	−.45	.023	.64	−.39	.01	.68
J_{-1}	1.65	.04	5.2	2.45	.12	11.6
J_{-2}	1.06	.04	2.9	1.49	.13	4.4
J_{-3}	.92	.04	2.5	1.27	.12	3.6
J_{-4}	.67	.05	2.0	1.49	.14	4.4
J_{-5}	.85	.08	2.3	1.18	.057	3.3
U_0	1.94	.02	7.0	.60	.07	1.8
U_1	.44	.03	1.6	.20	.09	1.2
S_2	.28	.03	1.3	−.11†	.06	0.9
U_2	.018†	.03	1.0	.21	.09	1.2
S_3	−.31	.04	.73	.37	.09	1.4
U_3	.047†	.04	1.0	.70	.09	2.0

* Asymptotic estimate of the standard error.

† Less than twice the estimate of the standard error.

unemployment in the current calendar year; U_1 is coded 1 if there was a spell of unemployment corresponding to the most recent job separation in an earlier calendar year (i.e., corresponding to a J variable). If there was a second job separation during the past five years, S_2 is coded 1; U_2 measures a spell of unemployment at that time. Similarly, S_3 measures a third job separation in the past five years, and U_3 indicates a spell of unemployment either at the time of that job separation or at some earlier point in the five-year interval. So, to find, for example, the effect of three job separations, one would add the coefficient for the time of the last separation to the sum of S_2 and S_3.

The findings support the expectations indicated above but reinforce the argument that job tenure is the most important determinant of the risk of unemployment. For whites, the increase in the effect of a separation one year back (compared with more than five years in the past) falls from 6.5 to 5.2. For nonwhite workers, the drop is much greater, from 19 to 11.6. The recomputed yearly probabilities would then be .45 for nonwhites and .23 for whites, a smaller though still wide gap. Other changes in the model can be noted. As expected, there is some flattening of the slope implied by the tenure variables. Furthermore, there is clearly a positive association between the occurrence of a spell of unemployment

and previous unemployment events; the existence of spells of job-separation unemployment in one, two, or three of the past five years implies increases of 22%, 31% and 200%, respectively, in the probability of unemployment for nonwhite workers. For whites, the effects are all concentrated in the first spell of prior unemployment, but unemployment at that time implies a 55% increase in the chances of another spell. While these effects are significant in comparison with, for instance, four years of schooling or a doubling of the wage rate, they are still quite small in comparison with the tenure effects. The status of the present job is clearly the most important determinant.[3]

The models also reveal other important indicators of unemployment risk and differences in their strength across the two groups. For whites, four additional years of schooling imply a one-third reduction, but for nonwhites they actually increase slightly the chances of unemployment (the coefficient is, however, very small). For whites, increased education is often an indicator that a worker has a white-collar job, with greater security, corresponding to college attendance. Education does of course allow nonwhites to gain better jobs, but its effect seems to be almost completely accounted for through its association with job tenure and wages. Self-employment tends to be more risky for nonwhites than for whites, while government employment provides the former group with comparatively greater security. Also, marital status is clearly a more important indicator of unemployment experience for nonwhites than for whites. However, its effect is reduced to insignificance when a greater elaboration of past history is introduced into the model. The same is true for self-employment. The number of dependents is also revealed to be an identifier rather than a cause of unemployment; it is attenuated for nonwhites and shows a reversal of sign for whites. Finally, one might note that the addition of the more detailed information about the past reduces the size of the age coefficient.

[3] It is perhaps somewhat surprising that the trend from the variables measuring multiple job changes is less clear than expected. Part of the problem arises from the implicit assumption on which their calculation is based, namely, that the pattern, if not the magnitude, of the tenure protection is the same regardless of the number of shifts workers have made in the previous five years. This assumption is inadequate, as can be seen through an examination of the proportion of workers who experienced unemployment as a function of tenure and the number of past shifts. If one fits a curve to these figures (the data are available from me upon request), it is easy to see that the slope as well as the intercept tends to change and the disaggregation introduces some fluctuation in the figures. Thus, for nonwhites one can see that those with one previous separation tend to have slightly greater protection with low seniority than do workers with no previous separations, though the expected pattern reasserts itself for those with two or three separations. This wobble is reflected in the model by the negative coefficients. Similarly, white workers with three separations are somewhat more stable than are those with two. As the sample is further disaggregated, the increasing fluctuation makes these coefficients less interpretable, though by taking account of the shifts in the tenure pattern, they may increase the utility of the unemployment coefficients.

In contrast to the education and age effects, wages tend to be a stronger indicator of job security for nonwhites than for whites, though the effect is not dominant for either group. A change in the effective hourly wage from \$5.00 to \$8.00 implies a decrease of 25% for nonwhites and 15% for whites. The nonwhite effect is stronger probably because unemployment experience is most likely at low wage levels, where nonwhite workers are more concentrated. Therefore, the same proportionate increase in wages implies a greater security return for nonwhites than it does for whites.

In order to gain a more detailed picture of the relationship between age and unemployment experience, I subdivided each race group into two cohorts, those who were 25–34 years old and those who were 35–54 years old in 1972. The fact that the samples are consequently smaller increased the instability of the coefficients in the models, especially for the young nonwhite workers. Some care should accordingly be taken in the interpretation of the results.

Because of the difficulty in interpreting the age coefficient when detailed past effects are included, these models have an additional variable, namely, the log of age multiplied by "1" if the worker had neither unemployment nor multiple job separations in the past five years, and "0" otherwise. I argued above that job tenure could have different meanings at different stages of the life cycle, primarily because of the heterogeneity of the population. At older ages a person with low tenure might be more likely to experience a spell of unemployment than at younger ages, because an older worker with little job tenure is more likely to have long term employment problems than is a comparable young worker, all other things equal. An interaction term between job tenure and age therefore seems justified.

The results of the disaggregation are presented in table 3. Several interesting contrasts across the four groups become visible, and the hypotheses presented earlier in the paper have been partially supported. If we look first at the size and pattern of the job-tenure coefficients, we can see that the disaggregation has caused a significant degree of fluctuation in their size. Nonetheless, the general trend for all groups is as expected, namely, a decreasing likelihood of unemployment with increasing tenure. The older white workers have a greater risk of unemployment with low tenure than the younger men, a pattern consistent with the hypothesis that a process of concentration has changed the significance of low tenure for the two populations. The relation is reversed for the nonwhite workers, however. This anomaly may be due to different responses of the two groups to the 1975 downturn in the economy, a topic discussed below. Third, as in the earlier results, nonwhite workers clearly have much less

TABLE 3

MODEL III

Independent Variables	B	SE*	Exp(B)	B	SE*	Exp(B)
	White, 25–34 ($N=389$)			White, 35–54 ($N=665$)		
Log age	.55	.14	1.7	−.157	.16	.21
Log age × stable	−.20	.02	.82	−.007†	.02	.99
Marital status	.22	.06	1.2	.046†	.07	1.0
Health	−.32	.08	.73	.36	.05	1.4
Self-employed	−.36	.07	.70	−.17	.06	.84
Government-employed	−.68	.10	.51	−.66	.08	.52
Dependents	−.21	.01	.81	−.039	.01	.96
Log wage	−.41	.02	.66	−.19	.02	.83
Education	−.14	.005	.87	−.058	.005	.94
Unemployment rate	.037	.007	1.04	.17	.008	1.19
Log others' income	.039	.005	1.0	.0049†	.005	1.0
Hours worked	−.39	.03	.68	−.36	.04	.71
J_{-1}	1.29	.07	3.6	1.73	.06	5.6
J_{-2}	.93	.07	2.5	.89	.07	2.4
J_{-3}	−.61	.10	.54	1.32	.07	3.7
J_{-4}	−.011†	.10	.99	.97	.07	2.6
J_{-5}	.75	.07	2.1	.51	.07	1.7
U_0	1.18	.04	3.3	2.49	.05	12.0
U_1	.19	.05	1.2	.33	.05	1.4
S_2	−.19	.07	.83	.38	.08	1.5
U_2	.14	.05	1.2	−.13	.08	.88
S_3	−.30	.05	.74	−.30	.06	.74
U_3	.035†	.06	1.4	.37	.07	1.4
	Nonwhite, 25–34 ($N=133$)			Nonwhite, 35–54 ($N=300$)		
Log age	−.24†	.46	.79	−1.01	.30	.36
Log age × stable	.52	.08	1.7	.023†	.04	1.0
Marital status	−1.02	.15	.36	.76	.13	2.1
Health	−.61	.12	.54	−.70	.09	.50
Self-employed	1.31	.05	3.7	−1.05	.22	.35
Government-employed	−.81	.27	.44	−1.75	.27	.17
Dependents	.33	.02	1.4	−.033†	.018	.97
Log wage	−.65	.10	.52	−.25	.05	.78
Education	.068	.011	1.1	.010†	.009	1.0
Unemployment rate	−.16	.017	.85	.022†	.02	1.02
Log others' income	.044	.01	1.0	−.065	.009	.94
Hours worked	−.07†	.07	.93	−.38	.04	.68
J_{-1}	3.20	.26	25.0	2.67	.13	14.0
J_{-2}	3.20	.28	25.0	.24†	.18	1.3
J_{-3}	2.8	.28	16.0	.31†	.20	1.4
J_{-4}	−.56†	.53	.57	2.20	.12	9.0
J_{-5}	3.56	.23	35.0	.22†	.24	1.2
U_0	−.43	.08	.65	.67	.05	.20
U_1	.33	.10	1.4	.29	1.0	1.3
S_2	3.42	.30	31.0	−2.26	.20	.10
U_2	−.01†	.30	.99	.47	.19	1.6
S_3	−.25†	.14	.78	2.44	.19	11.0
U_3	−1.10	.12	.33	.94	.11	2.6

* Asymptotic estimate of the standard error.

† Less than twice the estimate of the standard error.

security from low levels of tenure, because of the different mix of jobs they work in.

Compared with the effect of job tenure, the effect of past unemployment tends to be small, but in the expected direction. Here the predictions from before are completely correct; for both racial groups the older workers tend to have a greater continuity of unemployment experience—a stronger association between present and past unemployment—than do the younger men.

The disaggregation has caused the expected changes in the pattern of age dependence and these may plausibly be attributed to the heterogeneity effects described earlier, though some qualifications are needed. For younger workers, the probability of unemployment tends to increase with age.[4] The concentration for younger white workers takes place in both the "stable" and the "unstable" groups, though more strongly in the latter. The increase for the young nonwhite workers, in contrast, is limited to the stable group, the other workers exhibiting no age-related trend in either direction. If the stable and unstable groups had been effectively distinguished by the interaction terms, and if the age dependence specific to the unstable workers was primarily a result of the changing influence of low tenure because of their increasing concentration in that part of the distribution, then we would have expected the age $\times$ stable coefficient in model III to be negative for both racial groups. Yet, in neither group did the stable workers tend to experience decreasing probabilities of unemployment with age.

Part of the problem may lie in the conjecture that previous unemployment experience or multiple job changes are the most useful way of distinguishing between the two groups. Five years may be too short a time for this procedure to be effective. In fact, the relative size of the job-tenure and unemployment coefficients suggests that the amount of turnover, rather than unemployment, should be the criterion for distinguishing between the two groups. Within each cohort, older workers with no unemployment but short tenure in their present jobs might still have a tendency to experience more unemployment than younger ones, though the former have not engaged in multiple job shifts in the past five years. This explanation makes even more sense in the light of the recession that took place in the last two calendar years analyzed in this paper. An attempt was therefore made to rerun the model with the category of stable workers limited to those with more than five years since their last job separation. The results tended to confirm the hypothesis that stable workers under the new definition had decreasing probabilities of unemployment

[4] Because of the weaker relationship between age and labor-market experience in the younger cohort, a weaker age effect might have been expected. But the sign should have remained negative if population heterogeneity were not a factor.

with age, but statistical instabilities between the job-tenure and age variables prevented convergence of the maximum-likelihood procedure and hence prevented a strong assessment of their effects.

The older cohort, in contrast, showed no concentration effects. This finding conforms to the expectation that most job sorting and mobility have taken place by age 35. The protective aspects of age dominate from then on. Again, as in the earlier results, the coefficients are larger for whites than for nonwhites. In both cases they are stronger than in table 2, as one would expect. The fact that the decrease is the same size for those who had past unemployment or multiple job separations as for those who did not leads to the suggestion that, over time, even workers in poor jobs might be locating more stable (if still low-paying) jobs through a process of trial and error. This possibility, if true, would have the interesting implication that secondary labor markets, for all their other properties, are not characterized by truly constant instability over the life cycle. Older workers with poor jobs might find ways of stabilizing their work careers, or drop out of the labor force permanently. This suggestion, of course, does not deny that over the entire age range workers in poor jobs tend to experience more unemployment than other workers. The age effect, after all, is hardly dominant; the reduction of 40% for whites and 30% for nonwhites for a 15-year age difference is much smaller than the effects of job tenure. It should also be noted that even if such an effect were operable, it would still tend to benefit white workers more than the nonwhites.

CYCLICAL SENSITIVITY AND GOODNESS OF FIT

In order to determine the ability of the models to predict who is actually going to experience unemployment in a given year, and to examine the residuals in order to get some sense of the differential cyclical sensitivity of the population groups used here to changes in the economy, I compared predicted probabilities of an unemployment event and the actual history of each member of the sample for the four calendar years covered by this analysis. These calculations are based on the coefficients of model III and the values of the direct time-dependence factor $h(t)$ of equation (3). A summary of the results is presented in tables 4 and 5.[5]

Table 4 reveals, first, that for all groups the large majority of the population is assigned a very small probability of unemployment in any given year. The older cohorts tend to be distinguished from the younger ones in two ways. More older workers—approximately 90% as compared with

[5] Because of the procedure used to generate these figures, I was not able to obtain strictly comparable cutting points in tables 4 and 5. A more complete table of comparisons, of which these are only brief summaries, can be obtained from me upon request.

TABLE 4

PROPORTION OF WORKERS WITH LOW OR MODERATE RISK OF UNEMPLOYMENT, BY AGE AND ETHNIC CATEGORY, 1973–76

	1973	1974	1975	1976
Low risk:				
Young nonwhites	.76 (.10)	.71 (.05)	.76 (.12)	.83 (.10)
Older nonwhites	.89 (.05)	.87 (.04)	.91 (.10)	.90 (.10)
Young whites	.80 (.06)	.64 (.04)	.84 (.10)	.77 (.08)
Older whites	.87 (.04)	.89 (.06)	.91 (.10)	.88 (.08)
Low or moderate risk:				
Young nonwhites	.92 (.20)	.86 (.12)	.82 (.24)	.96 (.30)
Older nonwhites	.93 (.08)	.96 (.12)	.92 (.20)	.91 (.20)
Young whites	.94 (.12)	.89 (.12)	.96 (.20)	.98 (.24)
Older whites	.98 (.08)	.96 (.12)	.97 (.20)	.99 (.20)

NOTE.—Entries indicate the approximate proportion of workers with a predicted probability of unemployment (temporary layoff excepted) no higher than the quantity in parentheses.

TABLE 5

COMPARISON OF PREDICTED AND ACTUAL AMOUNTS OF UNEMPLOYMENT, BY AGE AND ETHNIC CATEGORY, 1973–76

	1973	1974	1975	1976
Low risk:*				
Young nonwhites	.020 (.05)	.0034 (.03)	.0039 (.06)	.030 (.05)
Older nonwhites	.0049 (.03)	.0022 (.02)	.037 (.05)	.020 (.05)
Young whites	.010 (.03)	.016 (.02)	.033 (.03)	.025 (.04)
Older whites	.0083 (.02)	.012 (.03)	.027 (.05)	.021 (.03)
All higher than low risk:†				
Young nonwhites	.24 (.26)	.13 (.15)	.51 (.42)	.28 (.25)
Older nonwhites	.30 (.23)	.23 (.12)	.70 (.54)	.52 (.40)
Young whites	.14 (.12)	.10 (.11)	.17 (.18)	.23 (.17)
Older whites	.10 (.09)	.16 (.11)	.20 (.19)	.18 (.12)
Low or moderate risk:‡				
Young nonwhites	.017 (.07)	.035 (.04)	.013 (.07)	.060 (.07)
Older nonwhites	.0064 (.03)	.018 (.02)	.042 (.05)	.022 (.05)
Young whites	.022 (.04)	.03 (.04)	.052 (.05)	.065 (.06)
Older whites	.013 (.02)	.023 (.03)	.036 (.06)	.030 (.04)
All higher than low or moderate risk:§				
Young nonwhites	.69 (.46)	.08 (.23)	.63 (.50)	.35 (.43)
Older nonwhites	.29 (.24)	.36 (.22)	.79 (.63)	.58 (.44)
Young whites	.25 (.20)	.18 (.20)	.14 (.30)	.23 (.38)
Older whites	.16 (.14)	.16 (.20)	.27 (.27)	.67 (.25)

NOTE.—Entries are the proportions of workers in each group who experienced a spell of unemployment (temporary layoff excepted) in the indicated year. Quantities in parentheses are the predicted probabilities for unemployment as generated by the model described in table 3.

* These groups correspond to the "low-risk" groups of table 4.

† These groups are the complement of the "low-risk" groups.

‡ These groups correspond to the "low- or moderate-risk" groups of table 4.

§ These groups are the complement of the "low- or moderate-risk" groups.

70%–80% of the younger workers—are classified in the most stable group. Second, the most stable older workers have a lower risk than the most stable younger ones. There is some tendency for the most stable white workers in each age group to have lower probabilities than the most stable nonwhite workers; this tendency implies that the model predicts a greater across-the-board instability for nonwhites than for whites. If one looks at the proportion of the most stable group which actually experienced unemployment, however, one can see that the estimates are in general too large; in fact, there is not very much difference between nonwhite and white stable workers. The roughly similar experience of the most stable workers from each cohort tends to support the comparisons of the size of the tenure coefficients that were made across the four cohorts. Though the size of the coefficients implies, strictly speaking, internal differentiation of the four groups, the similar bases support the across-group comparisons.

The major difference between the white and nonwhite workers lies in the extent of risk for the workers in the higher-risk groups. It can be seen in table 5 that the nonwhite high-risk workers both are assigned higher probabilities of unemployment by the model and in fact experience higher unemployment than do the white workers. This is particularly true for the older nonwhite workers. It is for this cohort that the model can most clearly distinguish between low- and high-risk workers. The concentration effect for nonwhites causes both a reduction in the size of the group which experiences unemployment and an intensification of the risk for this group. For whites, on the other hand, the concentration effect is shown only through the reduction in the size of the group with a higher risk. No visible increase in the size of the risk for that group is evident. In fact, the predicted probabilities for the high-risk older whites tend to be lower than the predicted probabilities for the high-risk younger whites. It may be that the better opportunities for older whites, even for those who have had recent employment problems, weaken somewhat the contrast one would have expected between older and younger workers.

By comparing the unemployment experience of older white and nonwhite workers for the recession years of 1975 and 1976 one can see that the stable nonwhite workers tend to be significantly more sensitive to recessions. This finding supports the argument that nonwhites tend to be concentrated in more cyclically sensitive industries than are whites. It also explains why the job-tenure coefficients are smaller for the older nonwhites than for the younger ones. The older nonwhites with longer job tenure still experienced unemployment in relatively high numbers in 1975 and 1976, while the young nonwhites with high tenure escaped unemployment at least in 1975, the year that accounted for most unemployment for that group as a whole. Again, fluctuation due to the small size

of the samples certainly plays a role here, but the suggestion that there is a substantive difference in the experience of the groups when the economy worsens remains credible. The findings indicate again that simple uses of job-tenure variables can distort basic structural tendencies. They imply that distinctions must be made between the effects of tenure during times of prosperity and recession, as well as across dimensions of job quality.

DISCUSSION

The analysis suggests that the variety of factors involved in determining or predicting unemployment experience make their presence felt differently over the life cycle and the business cycle. Quality of job is exceedingly important, not only directly, but also indirectly through its very strong interaction with job tenure. The protection and disincentives afforded by job tenure are clearly dominant; moreover, long tenure produces similar benefits for young and old workers regardless of their race. But major differences become apparent at low levels of tenure; the protection afforded by jobs of different quality varies enormously. The association between recent unemployment and future unemployment is in general positive for all four groups analyzed, and for some its effects can be reasonably strong. It is the fact of recent turnover, however, which produces the largest risk, and its magnitude is strikingly different for workers from the four groups.

The recession effect is also of interest. During downturns, the young workers with short tenure tend to absorb the burden of greater unemployment for their cohort. Those with long tenure also experience unemployment in greater numbers and make up proportionately more of the unemployed at these times. But because the number of more vulnerable workers is so great, the shift is only moderately large. In contrast, the shift in frequency toward the high-tenured workers is much greater for the older cohorts, and particularly for the nonwhite workers. During periods of prosperity, the higher levels of nonwhite unemployment can be traced largely to a core of workers whose job characteristics and past employment histories indicate work instability and low job quality, expressed both through low job tenure and previous unemployment experience. This group is comparatively well defined; its members tend to experience unemployment with greater certainty than the comparable group of white workers, whose experience tends to be less predictable. During the recession, the high-risk older nonwhites experienced unemployment with a much greater frequency. But the amount of unemployment experienced by previously secure nonwhite workers was so great that the proportion of unemployment accounted for by the high-risk workers declined considerably. Given the

pattern of response to recession found in the data, the estimates might even understate the relative risk of low tenure during times of prosperity.

The analysis reproduces the common finding that unemployment experience decreases with age. But the decrease is not uniform across the population. For those whose youthful unemployment is part of a process of establishing a career, the decline in risk with age is probably substantial. Workers with chronic problems would show a weaker association. Because the sorting process between people and jobs gradually draws to a close as a cohort ages, it is not surprising that the association between unemployment risk and instability in the recent employment history of workers is higher for older workers.

The fact that the age effect for older workers is relatively strong suggests that there may be effects of job tenure which cannot be captured by a tenure variable with a ceiling of six years. The fact that the age $\times$ stable coefficient tends to be insignificant for older workers implies that these hidden tenure effects are not the complete explanation for the strength of the coefficient. Evidently, workers who have experienced unemployment or multiple job shifts tend to experience age benefits of the same size as those for workers with high seniority. This result suggests that many workers find other ways of gaining protection at least against unemployment if not against turnover. It also raises the possibility that even workers who never leave behind the labor of unrewarding jobs manage to increase their job security as they age. As these workers pass through a number of jobs, they may reach positions from which, for whatever reason, they are unlikely to move and unlikely to be fired. Some instead may move out of the labor force indefinitely. Further analysis would be needed, however, before such a pattern could be better documented.

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Rationality and Equity in Professional Networks: Gender and Race as Factors in the Stratification of Interorganizational Systems[1]

Jon Miller
University of Southern California

James R. Lincoln
Indiana University

Jon Olson
University of Akron

The organizational principles of rationality and equity account for the bureaucratic leveling effect on social differences posited by Weber. An inference from this framework, that organizational systems will neither create nor reinforce inequality based on gender or race, was examined with data provided by the members of six multiagency social service delivery systems. The dependent variable was a measure of access to the networks of interorganizational exchange that tied together the agencies in these systems. This measure, called centrality, did not vary by race or gender. However, an analysis of first- and second-order interaction effects indicated that the combinations of investments and contributions that were predictive of centrality were very different for white men, white women, nonwhite men, and nonwhite women. A complicated process of negotiation for resources and advantages was indicated that is not easily reconciled with deductions from classical organizational theory.

How the work experiences of women and men and whites and nonwhites differ and whether the differences resolve into clear patterns of privilege and deprivation is a matter of increasing sociological concern. This study reports on the stratification by ascription and achievement that developed in six multiagency social service delivery systems created to provide community-based facilities for juvenile "status offenders" (youth whose of-

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fenses would not constitute legal violations for adults). Each system was characterized by a network of professional ties linking together the members of its separate constituent agencies. We charted these ties with sociometric measures of the participants' work contacts and the patterns of influence, respect, support, and professional assistance that had evolved among them. Our purpose here is to expose the mechanisms by which central positions in these interorganizational networks were gained and, in particular, to show how access to these mechanisms was affected by the race and gender of the participants.

As we measured it, network centrality represents access to the structures of interaction, communication, and professional exchange that were crucial to the participants' work-role performance. The pool of resources available for dealing with a client's problems was significantly expanded for the practitioner who not only was able to rely on his or her own skills and those of colleagues in the same agency but also was strategically placed in the interagency networks (Miller 1980). Centrality is not a "return on investment" of the same type as more familiar job rewards, such as income, rank, or occupational mobility, particularly since it does not share their more easily recorded quasi-contractual status. Nevertheless, there is a wide scholarly consensus that centrality in either intra- or interorganizational networks is a generalized resource that can readily be converted into an array of concrete advantages and rewards (Aldrich 1976; Benson 1975; Galaskiewicz 1979).[2] Furthermore, taking centrality as our dependent variable is consistent with a focus on organization-level processes, for while it implies advantages to individuals it is also a structural property that rationalist theories claim should be responsive only to the achievement-based attributes of organizational members. That is, directing communication channels through persons with high training and experience is ostensibly conducive to the achievement of organizational goals, whereas a rule for assigning persons to central network positions on the basis of ascriptive traits is likely to impair goal attainment. Our consideration of network centrality, then, has implications both for processes of resource distribution among individuals and for theories of organizational structure.

The strategy we have followed is to compare the effects on network

[2] The validity of this proposition has long been apparent in the organizational literature, though rarely stated in network terms. Blau's study of consultation patterns (Blau 1955) is a good example, as is Homans's (1961) discussion of interaction patterns in organizational surroundings. Recent examples include Rogers and Agarwala-Rogers (1976) and Miller (1980). The proposition is also supported by a wide range of other studies, including studies of job-finding networks (Granovetter 1974), networks that mediate access to mental health services (Horwitz 1977), and those that define the structure of community power (Perucci and Pilisuk 1970; Laumann and Pappi 1976).

position of gender and race, two ascribed attributes, with the effects of several indicators of professional achievement. Although theoretical considerations involving organizational and interorganizational relations are our major substantive concerns, the data we have generated also have important points of relevance for the more specialized literatures on internal labor markets and status attainment, and for the study of sexual and racial stratification in general. Studies of these topics are making increasing use of organizational variables in attempting to explain differences in income and mobility and to account for differential access to decision-making authority (Kluegel 1978; Wolf and Fligstein 1979; Halaby 1979; Grandjean 1981). Often, however, these studies are not actually conducted in organizations, and they are unable to specify in any detail the actual organizational practices that would account for their findings. What we have done is to take a more direct look at the mechanisms by which individual attributes and resources, or investments, are translated into the daily work experiences of respondents.

The programs we surveyed were complicated multiorganizational structures; however, they relied heavily on the expertise of their members and they had explicit system goals, an explicit interagency division of labor, and clear system boundaries that permitted a sharp demarcation between the agencies in a network and those that remained outside it. Over a two-year period, several thousand clients were processed by them. Therefore, they represented bureaucratic responses, in the Weberian sense, to a problem of large-scale performance and administration. Rationality and equity are important elements in the legitimating symbolism of such systems (cf. Meyer and Rowan 1977), especially given their strong public commitment to professionalism and social justice. By *rationality* we mean a principle according to which resources, advantages, and rewards gained are proportionate to participants' objective contributions or investments. *Equity* supposes that such a principle does not vary in its application to different categories of participants. Ideally, then, the ascribed characteristics of practitioners in these programs should not have been dominant factors in the way the interagency networks of professional exchange were structured; in fact, however, the actual implementation of these principles is not to be taken for granted. As the studies reviewed below show, the effects of ascription-based differentiation—racism and sexism—characteristic of the larger society can break through the boundaries of supposedly rational and equitable systems in a variety of ways.

There are three stages in the analysis. The first compares the effects of gender and race with the effects of formal training, professional experience, workplace location, and formal authority or rank. Stated in "ideal-typical" form, the expectation that provides our point of reference is straightforward: in a purely rational situation, the advantages of the work

setting will not be structured along the lines of gender and race but according to objective professional investments and contributions to the system's activities.

A simple regression of network location on race, sex, and various indicators of objective qualifications addresses this proposition but by itself is incomplete because it does not reveal whether different mechanisms are in operation for men and women, whites and nonwhites. To explore the latter possibility requires a determination of whether ascribed attributes interact with achieved characteristics in determining the advantages gained by individuals. Equivalent training or experience or authority producing access to more central network positions for men than for women, for example, would constitute evidence of inequity. Such possibilities are the subject of the second part of the investigation.

Finally, a third concern involves the interplay of the two ascribed characteristics, gender and race. Jeffries and Ransford (1980) have implied that the intersection of ethnicity and gender will create unique aggregates ("ethsexes") whose experiences assume patterns that cannot be anticipated simply by "adding" the effects of race to those of gender. With this in mind, we asked whether the patterns of effects associated with race differed for men and women, and whether the effects of gender differed for whites and nonwhites. Because of racial imbalance in the programs studied, our examination of these second-order interaction effects has to be somewhat tentative.

Before describing our research in more detail, we turn briefly to a consideration of previous work that bears on these substantive issues.

UNIVERSALISM AND ACHIEVEMENT IN BUREAUCRATIC SETTINGS

In one of several variations in which the concept comes to us from Weber, rationality encompasses the shift from ascription to achievement in the allocation of resources and from particularism to universalism in the evaluation of performance (cf. Parsons 1951; Kalberg 1980). In this sense the term is associated theoretically with increasing reliance on complex, bureaucratized forms of organization. For Weber (1947), rationality was apparent in the ways in which organizations linked access to formal positions and other resources to expertise and in the practice of gauging performance by the achievement of role obligations. A leveling of differences ascribed in the larger society would also accompany the attempt to eliminate irrelevant or counterproductive considerations from the search for or utilization of talent (Weber 1947, p. 340; Gerth and Mills 1946, p. 224–28).

The accuracy and the generalizability of this argument have been ques-

tioned,[3] but the claim that organizations are *in principle* universalistic and achievement oriented ("intendedly rational" in the language of March and Simon [1958], pp. 169–71) is included in most theories of organizations. Despite this consensus, however, little of the organizational and interorganizational literature has directly scrutinized the impact of ascribed characteristics, particularly gender and race, on the allocation of system resources, rewards, and advantages.[4] There is ample evidence that women and nonwhites are at a disadvantage in finding their way into organizations and are disproportionately found in lower-level occupations and positions that convey fewer rewards and less chance for individual progress (Siegel 1965; Rossi 1965; Epstein 1970; Treiman and Terrell 1975; Featherman and Hauser 1976; Kluegel 1978; Treas 1978; Wolf and Fligstein 1979). However, still unresolved is the question of whether *within a given position,* or *at a given level of skill* (in other words, once the entry level barriers are passed), the work experiences of women and nonwhites will parallel those of their male and white counterparts.[5] In a culture characterized by ingrained sexism and racism, discriminatory practices may well penetrate the boundaries of organizations; otherwise, there would be a marked disparity between the practices of organizations and the surrounding culture that supports them. Nevertheless, few studies have addressed this issue explicitly, and the direction of the evidence is still unclear.

Kanter's hypothesis (Kanter 1977*a*, 1977*b*) is that women who have to function alone or in small disconnected numbers surrounded by men will have experiences that are different from, and less favorable than, those of women who are able to form liaisons and alliances with other women or who can otherwise escape becoming highly visible tokens. Kanter devotes comparatively little attention to the relative power of men and women, independent of their proportions, or to related questions concerning competing interests and direct discrimination.

In a study of small, highly professionalized bureaucracies in which the numbers of men and women were fairly evenly balanced, Miller, Labovitz, and Fry (1975) reached substantially different conclusions. For men they

[3] The most direct recent challenges to the Weberian view are to be found in the loose coupling view of Weick (1976) and in the view that rationality and formality are legitimating myths for organizations (Meyer and Rowan 1977). In a similar vein, see Brown (1978) and Rothschild-Whitt (1979). Also relevant are Benson (1977) and Goldman and Van Houten (1977).

[4] This assessment applies only to the organizational literature. Concern for ascription is not lacking in other areas dealing with work and rewards (see Jeffries and Ransford 1980; Alves and Rossi 1978; Nock and Rossi 1978).

[5] A traditional interpretation is that women bring different expectations to their work and respond in fundamentally different ways from men to issues of influence and equity (Etzioni 1964, p. 89). For contrary evidence see Grandjean and Bernal (1979).

reported a rational pattern: the greater the investment or contribution, the greater the subjective and social relational rewards; for women, however, the greater the investment, the greater was the disparity between their rewards and those of men. This finding provided a strong if still essentially inferential argument for the operation of differential reward allocation mechanisms in the internal workings of organizations. Further evidence for this phenomenon is to be found in Halaby (1979) and Wolf and Fligstein (1979).

Very little research has dealt directly with the experiences of nonwhites in organizations, but it could be argued that the patterns of race relations will be basically similar to those for gender. Kanter's argument about relative numbers could be extended to cover race as well as sex (see, e.g., Epstein 1973). Alternatively, Blau and Duncan's (1967) study of differential achievement by race in the larger occupational structure (see also Parcel 1979) would not be inconsistent with an argument that organizationally bound occupational experiences will be directly affected by systematic racial differentiation. In the Blau and Duncan data, the deficits faced by nonwhites were greater the more they invested in education, a finding similar in form to the result referred to above in the Miller et al. (1975) investigation of gender differences. More recently, a study of the army by Butler (1976) has brought this question of racial inequity closer to the organizational sphere. Independent of ability and training (as measured by the army), race was shown to have a direct effect on the difficulty of promotion, and the delays experienced by nonwhites were more pronounced in the higher than the lower levels of the enlisted structure.

Except for Kanter's, each of these diverse studies of gender and race indicated that the *relative* deficits in rewards experienced by disadvantaged groups can persist and in some cases be intensified by access to the resources that are thought to be the keys to success in the workplace. It is possible to piece together from this pattern an impression of differentiation in organizations based on ascription "imported" from the larger society, even though many theoretically important details about the mechanisms involved remain unclear in these studies.[6] Our strategy for approaching this problem will be more apparent after the special circumstances of our research setting have been described.

[6] To illustrate the lack of clarity, three of the five organizations studied by Miller et al. (1975) were involved in scientific research, and highly qualified women with professional credentials or administrative positions were few in number compared with similarly qualified males. Their disadvantages might have been tied to this "skewness" as much as to active discrimination. Note also in Butler's (1976) army data that blacks were disadvantaged relative to whites, but the system actually worked "rationally" for neither category. This is in contrast to the Blau and Duncan finding (1967) that investments paid off for both whites and nonwhites, though at a higher rate for whites.

METHODS AND MEASURES

The six programs in this survey, all of which were federally sponsored, ranged in size from 20 to 90 members and from seven to 25 participating agencies. The pattern was for one agency (technically the grantee) to coordinate and oversee the services of the other agencies in a given program. In each case, a bounded network of agencies was established which could offer a wider range of services and treatment modalities for juveniles than any single agency could. This interorganizational network structure was thought to be a major strength of the programs.

Racially, the programs were staffed predominantly by whites (199/261, or 77%). In fact, in two programs all the respondents were white, and in the others the proportions of whites ranged upward from 60%. A similar imbalance did not occur for gender. Overall, 60% of the practitioners were female, with a range among the programs of 48%–70%. The most characteristic occupations were social worker and youth counselor, categories that are certainly not the privileged preserve of men. Nor was the frequent observation that most supervisors in these kinds of occupations are men (Montagna 1977, p. 280) borne out. Among the 261 respondents, 85 reported having some supervisory responsibilities. This proportion (about one third) did not vary greatly in the breakdowns for men and women, whites and nonwhites.[7]

Sociometric Measures of Network Position

The interpersonal connections that linked together the practitioners and agencies in each program were plotted from reports of closest work contacts and four other sociometric dimensions, including influence (who determined how the respondent's work on the project was done), respect (whose professional opinion was most highly regarded), informal support (who was considered supportive in times of contention), and professional assistance (who was a good source of professional advice).[8] Respondents were asked to base their reports on their relations with the overall inter-

[7] The Wolf and Fligstein (1979) and Kluegel (1978) findings that women and blacks are excluded from authority receives little support in this study.

[8] The respondents were asked for three nominations on each sociometric question. This approach is still used in important studies (Laumann and Pappi 1976; Burt 1977) but has been criticized on technical grounds (Holland and Leinhardt 1973). The forced-choice format confronts respondents with a standard stimulus, and it can be used to minimize the reporting of ties that might be unimportant. However, this method may be subject to error as a result of underreporting. In our study no participant in any of the programs was excluded (i.e., neither chose nor was chosen) by the method we used, and therefore meaningful centrality scores were available for all participants. Given our findings here and elsewhere (Miller 1980), we are confident that the basic structure of the networks was accurately captured. Nevertheless, we agree that the open-ended format is preferable for most applications.

agency program rather than confine their choices to the members of their own agency. The nominations that were in fact interagency as opposed to intraagency ranged from 36% to 60% on the work-contacts criterion, with similar proportions on the other four sociometric items.

"Centrality" is the graph-theoretic measure of network location proposed by Harary, Norman, and Cartwright (1965, p. 188).[9] Consider a matrix, P, any element of which, p_{ij}, is the path distance from actor i to actor j in the network of a given type of tie in a program. The centrality measure, C, is then defined as follows:

$$C = \frac{\sum_{i=1}^{N} \sum_{j=1}^{N} p_{ij}}{N}, \qquad \sum_{i=1} p_{ij}$$

where $i \neq j$ and N is the total number of actors in the network. In other words, all the distances in the graph are summed, then divided by the sum of distances to a particular node, j. Centrality in this sense is often interpreted as the access by an actor to information and resource flows from other actors, since high centrality implies short communication channels converging on j (Lin 1976, p. 345). This interpretation is appropriate in view of our conceptualization of centrality itself as a generalized network "resource." For the purpose of pooling the data from the six programs for the regression analysis, these centrality measures were standardized by calculating each person's score as a proportion of the highest score achieved in the program in which they were a participant (Individual Centrality / Maximum Centrality). Thus the unstandardized regression coefficients in the tables have a direct interpretation as increments in percentages of the top score.

Measures of Achieved Status

In this category of variables, education refers to the number of years of formal schooling, professional experience records the number of years the individual had worked at his or her occupation by the time of the survey, and formal rank (also referred to as authority) was measured by a dummy variable based on whether the respondent was in a position with official supervisory responsibility.[10] A measure called "workplace," also a dummy, was included as a control. It reflects whether the respondent's own employing agency was or was not the grantee agency, that is, the one with responsibility for the overall coordination of the program. Almost by

[9] Centrality scores were generated by Peter Marsden's DIGRAPH program.

[10] Simple hierarchies in the programs justified this dichotomy.

definition, members of these administrative centers were likely to have greater centrality in the networks of interaction,[11] and it was necessary to allow for this in the analysis.

In addition to social workers and counselors, these programs also included court personnel (primarily probation officers), staff and technical consultants (testing experts, psychologists, statisticians, etc.), and those with administrative but not client-contact responsibilities (called "administrators" in the tables). These five categories were entered into the regressions as dummy variables; "other occupations" (which included primarily secretarial and clerical personnel) served as the reference category.

Finally, a preliminary analysis indicated that professional experience (or more precisely, its inverse) should be interpreted as recency of acquiring professional skills, an important consideration given the emphasis in the programs on young practitioners collaborating to deal with youthful clients. This in turn suggested that age of practitioners also be included in the analyses as a control variable.

FINDINGS

Overall Regression Analyses

It is evident that the networks of professional exchange were not restricted to one or another race or sex category.[12] Mean centrality on the five network dimensions varied little from one category to another (table 1), and, with the exception of the dimension called "assistance," neither race nor gender was a significant factor in explaining the variance in centrality (table 2). Much of the variance was traceable, as expected, to workplace and to individual rank or authority. In addition, a pattern of isolation appeared for the "staff/consultant" category, which represents advisory personnel with little direct involvement in the programs' day-to-day operations. Relative isolation also characterized practitioners with greater experience, a finding that might seem paradoxical but, as was indicated above, is consistent with the fact that such programs rely on the collaboration of newly trained practitioners to carry out their client-related activities. The inverse relation between age and centrality in the work-contacts networks reinforces this observation.

[11] The proportion of all work contacts exchanged that were directed to the members of the administrative center averaged 36% for the six programs, with a range of 16%–83% (the latter figure representing the smallest program, one with only 20 participating members). Similar proportions appeared for the other four sociometric questions.

[12] Separate analyses for the programs produced results that were essentially the same. The overall variance explained varied, but the effects of gender and race were quite similar.

There is little here to indicate that race and gender dominated the way these programs were structured, and, in fact, if no other data were generated, it would seem that ascribed differentiation had been largely neutralized. However, the following sections will make it clear that this conclusion is premature.

Interaction Effects

Our analyses in this section address the question whether the relationship of network centrality to the achievement attributes of program participants depends on race and gender. An affirmative answer depends on evidence of statistically significant variation across race and gender categories in the coefficients of experience, education, age, rank, workplace, and occupation. Our approach was to perform an overall test of the equality of regression slopes across groups, and then, on the basis of that test, proceed to examine and interpret particular differences among coefficients. However, we have not gone beyond the initial omnibus test for interaction to discuss the significance of each of the large number of individual differences.

We will first present evidence on whether gender and race condition

TABLE 1

MEANS AND STANDARD DEVIATIONS FOR THE VARIABLES USED IN THE ANALYSIS

	ALL ($N=261$)		MEN ($N=100$)		WOMEN ($N=159$)		WHITES ($N=199$)		NONWHITES ($N=57$)	
	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD
Gender*	.39	.49	...	...	...	...	.36	.48	.49	.50
Race*	.78	.42	.72	.45	.82	.39	...	...	...	...
Experience	5.58	5.27	6.08	5.96	5.27	4.79	5.72	5.51	5.18	4.42
Education	16.49	2.29	16.99	2.47	16.16	2.14	16.67	2.09	15.88	2.90
Rank*	.33	.47	.37	.49	.31	.46	.34	.48	.30	.46
Age	31.57	8.81	31.81	7.42	31.41	9.59	31.66	8.90	31.11	8.49
Workplace*	.26	.44	.25	.44	.27	.45	.28	.45	.18	.38
Social work*	.33	.47	.29	.46	.35	.48	.30	.46	.46	.50
Counselor*	.18	.38	.21	.41	.16	.37	.19	.39	.14	.35
Court work*	.06	.24	.08	.27	.05	.22	.07	.25	.05	.23
Staff/consultants*	.05	.23	.07	.26	.04	.21	.06	.24	.04	.19
Administrators*	.19	.39	.19	.39	.19	.39	.21	.41	.11	.31
Other occupations*	.15	.36	.11	.31	.18	.38	.15	.35	.16	.37
Work contacts	.73	.15	.73	.15	.73	.15	.73	.14	.74	.16
Influence	.75	.14	.75	.14	.75	.13	.75	.13	.74	.16
Respect	.76	.14	.76	.14	.75	.14	.76	.13	.74	.16
Support	.78	.16	.78	.17	.77	.15	.78	.15	.78	.19
Assistance	.79	.13	.78	.14	.79	.12	.79	.12	.77	.16

NOTE.—Small discrepancies in N's are due to missing values on gender and color.

* These are dummy variables: male, white, supervisor, and member of the agency charged with overall coordination were coded 1; for the occupational variables, membership in the category was coded 1.

TABLE 2

REGRESSION OF NETWORK MEASURES ON TWELVE INDICATORS OF ASCRIPTION AND ACHIEVEMENT

INDEPENDENT VARIABLES	WORK CONTACTS		INFLUENCE		RESPECT		SUPPORT		ASSISTANCE	
	b	β	b	β	b	β	b	β	b	β
Color	−.013	−.04	.014	.04	.023	.07	.006	.02	.030**	.10
Gender	.005	.02	.018	.06	.017	.06	.022	.07	.008	.03
Experience	−.003*	−.10	−.004**	−.15	−.004**	−.13	−.008**	−.26	−.007**	−.30
Education	.000	.00	−.0004	−.01	−.002	−.04	−.002	−.03	.002	.03
Age	−.003**	−.19	−.002*	−.11	−.002*	−.10	−.002*	−.11	.0003	.02
Rank	.067**	.22	.060**	.21	.081**	.28	.069**	.21	.055**	.20
Workplace	.105**	.31	.077**	.25	.086**	.28	.064**	.18	.072**	.25
Social work	.003	.01	.008	.03	.045**	.15	−.002	−.005	.031*	.12
Counselor	−.025	−.07	−.036	−.10	−.012	−.03	−.058**	−.14	−.005	−.02
Court work	−.016	−.03	−.024	−.04	−.004	−.01	−.037	−.06	−.016	−.03
Staff/consultants	−.131**	−.20	−.109**	−.18	−.118**	−.19	−.152**	−.22	−.102**	−.18
Administrators	.007	.02	−.002	−.004	.003	.01	−.014	−.03	−.009	−.03
R^2 ($\bar{R}^2$)	.24	(.20)	.17	(.13)	.22	(.18)	.19	(.15)	.20	(.16)
Intercept	.820		.783		.783		.885		.722	

* $P < .05$.
** $P < .01$.

the effects on centrality of individual resource variables; below, we will extend this analysis to inquire whether particular combinations of gender and race (e.g., white men, nonwhite women) shape network access in a fashion not reducible to the sum of the separate influences of these factors. The hypotheses to be tested now are presented formally as numbers 1 and 2 in table 3. The appropriate test statistic in each case is an F-ratio of the following form:

$$F_{(q,N-p)} = \frac{ESS_c - ESS_u/q}{ESS_u/N - p},$$

where ESS_c is the error or residual sum of squares from the constrained regression of centrality on the ten achievement indicators. The constraint is that the vector of slopes is presumed constant across race and gender classes, although the intercepts of the equation may vary. The unconstrained ESS_u is the residual sum of squares from an identical equation except that both intercepts and slopes are allowed to vary. In the equation, q is the number of constraints, N is the number of observations, and p is the number of parameters estimated in the unconstrained regression.

The first two rows of table 3 show that all F-ratios are significant at conventional probability levels, indicating that for all five centrality dimensions, gender- and race-specific effects differ by more than sampling error. We are justified, then, in proceeding to examine the detailed within-group regression results which appear in table 4.

Comparisons of men and women.—In performing the partial analyses that are reported in table 4, we expected to find evidence indicating a superior capability on the part of men and whites to translate job-related qualifications into network advantages. Table 4 shows that different combinations of positive and negative factors did in fact go into the determination of network centrality for men (panel A) and women (panel B). For both sexes, experience (more correctly, inexperience, since the relation-

TABLE 3

F-TESTS FOR RACE AND GENDER INTERACTIONS

Hypothesis	df	Work Contacts	Influence	Respect	Support	Assistance
1. $\beta_m \neq \beta_f$	10,233	3.33***	2.95***	2.54**	2.56**	1.83*
2. $\beta_w \neq \beta_b$	10,233	2.99***	3.33***	3.33***	2.57**	2.67**
3. $\beta_{wm} \neq \beta_{wf} \neq \beta_{bm} \neq \beta_{bf}$	5,232	5.10***	5.55***	3.29**	5.45***	2.97*

NOTE.—Cell entries are F-statistics. In row 1, βm is the population vector of 10 slope parameters for males; βf is the analogous vector for females. In row 2, βw and βb are the same vectors for whites and blacks. In row 3 βwm, βwf, βbm, βbf, are vectors of five slope parameters for equations within race-gender subgroups.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

TABLE 4

REGRESSION OF NETWORK LOCATION MEASURES ON INDICATORS OF ACHIEVEMENT, PERFORMED SEPARATELY FOR MEN AND WOMEN, WHITES AND NONWHITES

	Work Contacts		Influence		Respect		Support		Assistance	
	b	β	b	β	b	β	b	β	b	β
A. Men Only										
Experience	−.005**	−.20	−.006**	−.26	−.006**	−.23	−.011**	−.37	−.010**	−.43
Education	−.018**	−.29	−.019**	−.33	−.021**	−.37	−.019**	−.28	−.012**	−.22
Age	.001	.07	.004**	.21	.004*	.19	.004	.16	.004*	.22
Rank	.101**	.32	.109**	.37	.097**	.33	.135**	.38	.084**	.29
Workplace	.107**	.31	.080**	.24	.089**	.27	.038	.10	.040	.12
Social work	.051	.15	.084**	.27	.134**	.42	.079*	.21	.072**	.24
Counselor	.082**	.22	.070*	.20	.128**	.36	.020	.05	.073*	.22
Court work	.067	.12	.076	.15	.135**	.26	.063	.10	.083	.16
Staff/consultants	−.176**	−.30	−.096	−.18	−.015	−.03	−.154**	−.23	−.066	−.12
Administrators	.049	.13	.061	.17	.096**	.26	.031	.07	.012	.04
R^2 ($\bar{R}^2$)	.42	(.35)	.36	(.28)	.36	(.29)	.33	(.25)	.27	(.19)
Intercept	.919		.879		.889		.965		.847	
B. Women Only										
Experience	−.003	−.08	−.040**	−.14	−.003*	−.12	−.008**	−.25	−.005**	−.19
Education	.012**	.18	.013**	.21	.011**	.17	.011**	.16	.015**	.27
Age	−.003**	−.22	−.002**	−.14	−.002**	−.13	−.002**	−.14	.0002	.02
Rank	.065**	.21	.034	.12	.075**	.26	.020	.06	.035**	.13
Workplace	.103**	.31	.077**	.26	.091**	.30	.073**	.22	.092**	.34
Social work	−.022	−.07	−.036	−.13	.001	.003	−.049*	−.16	.005	.02
Counselor	−.093**	−.23	−.088**	−.24	−.084**	−.23	−.090**	−.23	−.050**	−.15
Court work	−.064	−.10	−.063	−.10	−.078*	−.23	−.059	−.09	−.063*	−.12
Staff/consultants	−.050	−.07	−.076	−.12	−.141**	−.22	−.126**	−.18	−.107**	−.18
Administrators	−.040	−.11	−.044	−.13	−.049*	−.14	−.037	−.10	−.023	−.08
R^2 ($\bar{R}^2$)	.29	(.23)	.21	(.15)	.26	(.21)	.22	(.16)	.26	(.20)
Intercept	.638		.624		.642		.721		.550	

* $p < .05$.

TABLE 4—*Continued*

	Work Contacts		Influence		Respect		Support		Assistance	
	b	β	b	β	b	β	b	β	b	β
C. Whites Only										
Experience	−.001	−.05	−.001	−.06	−.0004	−.01	−.004**	−.17	−.006**	−.27
Education	−.003	−.05	−.002	−.03	−.005	−.08	−.005	−.07	−.001	−.01
Age	−.004**	−.27	−.002**	−.17	−.003**	−.18	−.003**	−.20	.0002	.02
Rank	.091**	.30	.074**	.28	.094**	.34	.079**	.25	.066**	.27
Workplace	.116**	.36	.083**	.29	.087**	.30	.059**	.18	.073**	.28
Social work	.016	.05	.019	.07	.063**	.22	.013	.04	.036*	.14
Counselor	−.007	−.02	−.013	−.04	.015	.04	−.030	−.08	.016	.05
Court work	.002	.004	−.011	−.02	.012	.02	−.011	−.02	−.007	−.02
Staff/consultants	−.081**	−.14	−.053	−.10	−.059	−.11	−.095**	−.15	−.040	−.08
Administrators	.012	.03	.007	.02	.009	.03	−.002	−.01	−.006	−.02
R^2 ($\bar{R}^2$)	.30	(.26)	.19	(.15)	.24	(.20)	.18	(.14)	.20	(.16)
Intercept	.863		.820		.847		.951		.776	
D. Nonwhites Only										
Experience	−.008	−.22	−.016**	−.43	−.015**	−.43	−.023**	−.53	−.011**	−.32
Education	.018**	.33	.013*	.24	.135*	.25	.013	.21	.018**	.33
Age	.0005	.02	.002	.11	.002	.09	.004	.17	.001	.07
Rank	.010	.03	.027	.08	.037	.11	.031	.08	.019	.06
Workplace	.099*	.23	.076	.18	−.091*	.22	.108*	.22	.068	.17
Social work	−.080	−.25	−.051	−.16	−.035	−.11	−.045	−.12	−.030	−.10
Counselor	−.092	−.20	−.080	−.17	−.072	−.16	−.084	−.16	−.079	−.18
Court work	−.132	−.18	−.085	−.12	−.069	−.10	−.134	−.16	−.073	−.11
Staff/consultants	−.545**	−.62	−.503**	−.57	−.510**	−.60	−.493**	−.49	−.549**	−.66
Administrators	−.052	−.10	−.009	−.02	−.008	−.02	.032	.05	−.039	−.08
R^2 ($\bar{R}^2$)	.36	(.21)	.42	(.28)	.47	(.33)	.45	(.31)	.44	(.30)
Intercept	.538		.582		.575		.595		.539	

* $P < .05$
** $P < .01$.

ships again were inverse) and workplace had an important influence. For education, however, an interesting interaction effect is apparent. For women, this variable contributed significantly and positively to centrality on all of the network dimensions, but for men the result was reversed: the effect of education was negative and significant across all five dimensions.

Less pronounced but nevertheless interesting contrasts appeared in the effects of rank. The "payoff" of rank was consistently positive for both men and women, but the increments to centrality were in general greater for men, particularly on the dimensions of influence and support. This is based on a comparison of unstandardized coefficients: .109 versus .034, and .135 versus .020. (Recall the interpretation of these slopes as increments toward the highest score attained in the respondent's program.)

Important contrasts by occupation are also apparent. For men, being a counselor, social worker, or court employee (compared with the reference category) was associated with greater centrality, patterns that were missing or reversed for women in the same occupations. While men were not numerically dominant in these categories, it appears clear that they fit into the professional exchange networks in different ways from their female colleagues.[13]

Finally, the effects of age were also variable by sex. For men, age was positively related to the influence and assistance dimensions, whereas for women the relationships were negative on all but one dimension.

Comparisons of whites and nonwhites.—Panels C and D of table 4 compare the experiences of whites with those of nonwhites. Specific differences are apparent for experience (the advantage of being professionally "new" was more pronounced for nonwhites), age (being young, net of experience, was an advantage for whites, not for nonwhites), and the dummy variable staff/consultant (which conveyed much greater handicaps for nonwhites than whites). However, viewed substantively, the most important differences between the races, as between the sexes, involved formal rank and education. Rank conveyed distinct and comparatively strong advantages for whites, a result that was conspicuously absent for nonwhites. The findings for education were just the reverse; this resource was generally an advantage for nonwhites but not at all for whites. These two mechanisms for gaining network access were clearly race linked, but in opposite ways.

In the diverse array of findings from the first two stages of the study, sex- and race-linked activities are clearly indicated, but the data do not resolve the question of systematic inequity in the patterns of reward and resource allocation. Each race and sex category had advantages and dis-

[13] Note that most of the coefficients for the occupational dummies were negative for women, indicating that only membership in the excluded category, predominantly clerical and secretarial personnel, was instrumental in providing network access.

advantages that were missing for its counterpart. In fact, two conflicting scenarios, equally plausible on the surface, can be constructed from the data in table 4. The first notes that women and nonwhites could command centrality in the networks of exchange on the basis of their educational credentials, a rational process that calls to mind Weber's discussion of certification (Weber 1947, p. 333). They could place less reliance on access to formal decision-making authority. In contrast, whites were unable to profit from their education in the same way as nonwhites, and men in comparison with women actually had to pay a price for the educational investments they had made. Any gains made by men came from the possession of official decision-making authority. The fact that authority had such a payoff for them could be seen as rational, but the anomalous finding for education certainly could not. This interpretation focuses on what may be inequities directed against whites and men and supports the idea that the social service delivery area is one that has in part reversed the general societal pattern of white and male domination.

However, an alternate scenario suggests a different conclusion. Leaving aside the question of education, the data show that men and whites with formal authority consistently found themselves close to the center of the exchange networks, a fact that indicates their ability to exploit this crucial organizational resource, the formal structure, to their advantage. The fact that superior training gave them no such advantage but in fact was a hindrance that could be overcome makes this finding even more impressive. In general, authority did women less good than it did men, and it is of particular importance that it carried with it less claim to actual day-to-day influence over co-workers. For nonwhites, rank carried hardly any advantage at all. A claim to access to the networks was a little more likely to be successful if backed up by superior technical credentials, specifically, recent training and more education, but the benefits were not great. This second scenario is not consistent with the usual interpretation of the leveling effect posited by Weber. To be sure, credentials opened up a path to important work-related resources for women and nonwhites, but another channel, that provided by authority, remained at least partially closed to them. Stated differently, the power of office was less effective for them.

Although we are drawn to the second scenario, these two arguments in fact undermine each other, illustrating the futility, in the absence of more information than we have presented so far, of searching for a pattern that unequivocally supports or unequivocally refutes the notion of domination by whites and males. The data simply do not offer this certainty but are suited to an argument that recognizes an underlying process of negotiation in which different categories of participants have different strong suits with which to advance their own positions and different liabilities for which they must compensate. To provide a better understanding of the terms on

which such a process might have unfolded, the final stage of the analysis adds information on the interaction of gender and race.

Interaction effects of race and gender combinations.—We turn now to a consideration of higher-order interactions between gender and race and the remaining practitioner resource variables in our model. Our results have thus far indicated that the processes leading to a central network position differed significantly for men and women and for whites and nonwhites. Now we inquire whether such processes also differed across the four natural combinations of these factors; whether white men, white women, nonwhite men, and nonwhite women differed in ways that cannot be reduced to the interaction effects of gender and race considered separately. Because the numbers of nonwhite men and women were small (28 and 29, respectively), we have conserved degrees of freedom by deleting the five occupational measures from the regressors analyzed previously. This approach conceals the influence of one important dimension of achievement, but it is warranted in order to better detect subgroup variations in the effects of other variables.[14]

Table 3 shows that hypothesis 3, which posits different regression parameters across the four race-gender subgroups, is confirmed (see Appendix A regarding computation of the test statistic). With respect to all five network ties, significant three-way interactions among gender, race, and the five remaining attributes are apparent. It would appear that the experiences of practitioners in gaining access to strategic network ties are jointly specified by their sexual and racial identities. The detailed results for the four ethsex groups appear in table 5.

The patterns of relative advantage and disadvantage are now much more apparent. For white men, high rank and recent entry into their occupation provided greater network access, but superior education was a consistent disadvantage. White women could also apparently use authority as a basis for claims to network centrality, though much less effectively than men, especially on the dimensions of influence and support. Education had no clear payoff, and experience was virtually irrelevant. Nonwhite women were similar to white women in that some advantage, though generally not statistically significant, accrued to supervisory rank, but unlike the white women in that, for them, superior education and recency of entry into the occupation were much more effective

[14] A rough idea of the consequences of dropping the occupational measures was obtained by regressing the network measures on just these five dummies separately for the four ethsexes. The regression for white men would probably have been improved by the positive effect of "court personnel" and the negative effect of "staff/consultant." For nonwhite men "counselor" would have added a strong negative effect, consistent with other findings for this category. For white women no occupational variable had a strong impact, but for nonwhite women "staff/consultant" represented a persistent disadvantage, just as it did for white men.

factors on which to base a claim to network centrality. Finally, for nonwhite men, rank offered no clear advantage, education was a persistent handicap, and experience was unimportant. In summary, it is worth repeating that the effect of education ranged from positive to neutral to negative across the four ethsex categories. It was a certain benefit only for nonwhite women, and it was a handicap for both white and, especially, nonwhite men. Note also that rank conveyed at least some advantage for everyone but nonwhite men.

To complete the analysis, age also entered the equation in different ways. For white men and nonwhite women, being older was of some advantage; for white women, being younger was consistently more likely to reinforce centrality; and for nonwhite men, age was not an important factor. Not surprisingly, workplace is one variable that provided consistently favorable payoffs for all four race-sex combinations.

The most striking finding here is that nonwhite men emerged as the only group unable to turn any of the bureaucratic symbols of achievement into work-related payoffs. This is as close as the data came to a clear pattern of inequity, but it must again be stressed that these practitioners were not on the average disproportionately isolated, but that their formal positions, experience, and credentials were simply of little use to them in avoiding isolation. The factors that did open up the networks to those nonwhite men who occupied favorable positions are as yet not apparent, but our intuition is that they were primarily able to rely on the fact that they were who they were. The service delivery agencies in the programs had to deal with communities many of which had a disproportionate number of minority inhabitants and with large numbers of male, nonwhite status offenders. Of all male clients processed, 33% belonged to minority groups—predominantly black—but nonwhite males were only about 10% of all practitioners. In such a context being a minority male would be of considerable use in relating to the surrounding community and to the clients, and these practitioners could not in good sense be excluded from the professional networks, even though the claims they made based on authority were apparently ineffective and those based on educational credentials were apparently rejected. If this reasoning is correct, their small numbers (tokenism?) probably *amplified* their functional importance as individuals and improved their bargaining position as a result.[15] We cannot

[15] Colleagues in public school systems report that minority male administrators may be shifted into prominent positions in times of acute minority-majority crisis with insufficient time to assess their preparation for the demands of the assignment. Over an extended period, such a practice could result in a situation in which "objective" contributions (credentials, experience, and the like) bear little connection to their positions in the system's hierarchy. We are indebted to Ramon Martinez, an educational administrator, for this observation.

TABLE 5

REGRESSION OF FIVE NETWORK LOCATION MEASURES ON EXPERIENCE, EDUCATION, RANK, AGE, AND WORKPLACE, PERFORMED SEPARATELY FOR FOUR ETHSEX CATEGORIES

	WORK CONTACTS		INFLUENCE		RESPECT		SUPPORT		ASSISTANCE	
	b	β	b	β	b	β	b	β	b	β
A. White Men ($N=71$)										
Experience	−.005	−.22	−.006**	−.32	−.006**	−.47	−.010**	−.42	−.012**	−.63
Education	−.022**	−.31	−.015**	−.24	−.012*	−.14	−.021**	−.26	−.006	−.01
Rank	.109**	.36	.119**	.45	.102**	.42	.132**	.40	.083**	.32
Age	.001	.03	.004*	.25	.003	.26	.004	.20	.004*	.24
Workplace	.130**	.39	.086**	.29	.080**	.29	−.038	−.10	.022	.07
R^2 ($\bar{R}^2$)	.32	(.27)	.32	(.26)	.24	(.18)	.26	(.20)	.31	(.25)
Intercept	1.046		.866		.852		1.017		.817	
B. White Women ($N=128$)										
Experience	.0004	.01	−.0002	−.01	.002	.07	−.003	−.09	−.002	−.07
Education	.004	.05	.002	.03	−.0003	−.01	−.002	−.02	.005	.09
Rank	.077**	.25	.044*	.16	.074**	.27	.034	.12	.031	.13
Age	−.005**	−.34	−.004**	−.27	−.004**	−.27	−.004**	−.30	−.0003	−.03
Workplace	.110**	.35	.085**	.31	.077**	.27	.077**	.26	.087**	.36
R^2 ($\bar{R}^2$)	.31	(.28)	.20	(.17)	.21	(.17)	.20	(.16)	.18	(.14)
Intercept	.779		.788		.823		.918		.694	

* $P < .05$.
** $P < .01$.

TABLE 5—*Continued*

	Work Contacts		Influence		Respect		Support		Assistance	
	b	β	b	β	b	β	b	β	b	β
	C. Nonwhite Men ($N=28$)									
Experience	.005	.09	−.005	−.10	−.005	−.09	−.009	−.14	.002	.05
Education	−.020	−.40	−.021*	−.42	−.021*	−.41	−.021	−.36	−.020*	−.40
Rank	−.015	−.05	.011	.03	.006	.02	.038	.10	−.029	−.09
Age	−.002	−.05	−.003	−.09	−.003	−.08	−.003	−.08	−.001	−.04
Workplace	.132*	.36	.141*	.37	.178*	.48	.138	.32	.156*	.43
R^2 ($\bar{R}^2$)	.25	(.07)	.30	(.13)	.36	(.21)	.24	(.06)	.29	(.12)
Intercept	1.084		1.167		1.147		1.219		1.092	
	D. Nonwhite Women ($N=29$)									
Experience	−.012*	−.41	−.019**	−.64	−.018**	−.63	−.026**	−.77	−.015**	−.55
Education	.031**	.48	.029**	.45	.028**	.45	.028**	.39	.031**	.51
Rank	.073	.19	.087	.23	.089	.25	.098	.23	.074	.21
Age	.005	.32	.063**	.42	.005*	.36	.008**	.49	.005	.32
Workplace	.195*	.39	.133	.25	.116	.23	.148	.25	.099	.20
R^2 ($\bar{R}^2$)	.42	(.26)	.53	(.40)	.53	(.40)	.58	(.47)	.53	(.39)
Intercept	.117		.154		.199		.166		.198	

* $P < .05$.
** $P < .01$.

directly document this argument, but the survey did provide information on two supporting patterns that bear on the issue.

First, the survey included four measures of practitioners' activism in the communities in which the programs were embedded, including efforts to (1) elicit the cooperation of other community organizations that were not officially part of the status offender programs, (2) raise the level of financial support in the community for juvenile programs, (3) influence community policies with regard to juvenile offenders, and (4) improve the treatment resources available for dealing with the problems of youth. Nonwhite males scored higher than all other groups on all four items (see Appendix B). Moreover, for them, the first item, bringing in additional organizational resources, was positively and significantly related to all five measures of network position. The second item, attempts to raise economic support, had a smaller and inverse effect, and the remaining two had no appreciable impact on network location. In contrast, for the other three ethsex categories none of the activism measures contributed significantly, either positively or negatively, to the explained variance of any of the network measures.[16] It is plausible (though contrary to Granovetter's [1973] argument about the "strength of weak ties") that nonwhite men could profit from their efforts to bring additional organizations in contact with their program, since in this way they could make a potentially unique contribution to the work of other practitioners. But this possibility is tempered by the finding (this time consistent with Granovetter) of an adverse effect for attempts to bring in added financial resources. Even if such attempts were successful it is likely that their time-consuming nature would detract from the ability of specific practitioners to deal with specific client problems, and this may explain the result. At any rate, it is apparent that nonwhite men differed dramatically from their colleagues in the nature of their community ties.

The second supportive pattern involves evidence that the centrality of nonwhite males was also affected in distinct ways by the composition of the client pool in their program. Partial correlations between their centrality scores and the proportion of nonwhite male clients were all positive for this group, ranging from .28 to .86 across the five centrality dimen-

[16] Data not shown. A series of analyses was performed in which the network measures were regressed on collections of variables to which the activism items were added singly and in various combinations. (The small numbers of nonwhites precluded simply adding the community measures to the overall regression equations.) In none of the solutions explored did any of the activism measures explain more than 1% of the variance in network location for white males, white females, or nonwhite females. For nonwhite males the conclusions stated in the text were supported in all of the solutions explored. Together, attempts to involve more community organizations and attempts to raise financial resources accounted for 12%–15% of the variance in the five network dimensions, with the positive effect of the former item generally being much more pronounced than the negative effect of the latter.

sions. Nothing approximating this finding was apparent for the other three categories of practitioners (see Appendix B).

Perrow (1979, pp. 11–12) has cautioned that what constitutes a "rational" or "objective" qualification for a participant will vary with the goals the collectivity is attempting to achieve. The programs we studied represent settings in which making certain claims to work-related instrumentalities on the basis of ascribed attributes was in fact consistent with the principle of bureaucratic rationality. Dealing with minority male youngsters in disproportionately minority communities calls for some resources that are lacking almost by definition among whites and women. The claim on this construction of rationality provided leverage to nonwhite men that counterbalanced but did not obscure the real disadvantages they faced in realizing the benefits of formal rank and training.

SUMMARY AND DISCUSSION

The pressure to define relationships with other community agencies, to establish and maintain a structure of exchange among professionals with different backgrounds and philosophies, and to balance out the claims of different racial and gender groups are strategic problems that the systems we studied share with virtually all service delivery systems and with a large proportion of organizations in general. The findings illustrate the possible theoretical consequences of oversimplifying the importance of race and gender as factors in the internal operation of bureaucratic systems or of ignoring internal organizational realities as factors differentiating the occupational experiences of men and women, whites and nonwhites. A simple regression suggested a clear bureaucratic leveling effect; gender and race appeared to have little impact compared with more traditional occupational and organizational variables. But the first-order interaction effects showed that experiences varied substantially for different categories of participants, even in circumstances in which the ultimate allocation of advantages was essentially equal. However, this analysis was also inconclusive. An examination of second-order interaction effects made the important additional point that the intersection of race and sex produced four distinct patterns for white men, white women, nonwhite women, and nonwhite men.

The rational bureaucratic or "inducements-contributions" model (March and Simon 1958) did not provide an adequate fit to any of these four patterns. However, the results also failed to support a neat, but equally oversimplified, formulation positing clearly polarized competing interest groups, because the experiences of males (and females) were variable depending on race, and the experiences of whites (and nonwhites) were variable depending on sex. In this regard, it is possible to argue that the

path to network centrality suggested for nonwhite women came the closest to confirming the rationalist model. Education was a positive factor only for them, and they faced no obvious handicaps of the sort that were evident for white and nonwhite men. If the assumption is that the disadvantages of being female and nonwhite accumulate in a simple additive fashion, this is a surprising result. It is equally significant that the situation of nonwhite males, the group actually most "deprived," judging from their inability to convert bureaucratic investments into organizational payoffs, seems to have been decisively and favorably altered by their relationship to the surrounding community.

Though a simple and completely consistent picture does not emerge, we are nevertheless willing to take much of what we observed as evidence that the racial and sexual inequality that characterize the larger society did in fact find their way, in mutated forms, into the internal operations of the systems we studied. Discrimination is not just a matter of essential equality or inequality in access to social instrumentalities; it pertains as well to the manner in which access is gained. Differences in the ability to realize the benefits of two specific rational investments, education and authority, are especially important, and it is appropriate to speculate briefly about the meaning of the patterns we observed to the people involved.

We have just noted that nonwhite women were the only persons for whom superior educational credentials were associated with greater network centrality. To shift the angle of vision, it is also possible that they were the only persons *required* to validate their claims to centrality by reference to credentials. If this was in fact true, the pattern we reported could be seen as evidence of a "gatekeeping" function that was not in operation for the other ethsex categories. For white females, education was not related to centrality in the same way. For them, a more important screening criterion seems to have been youth, a finding that also argues against the overcoming of traditional barriers. For both categories of males, education operated in a fashion opposite to the rational organizational model. It is especially interesting that for white men, the ethsex clearly dominant in the larger society, education was of no avail as an element of organizational rationality; in fact, it seemed to function as a handicap. No classical organizational explanation for this paradox is apparent, but some insight can be gained by considering what it means to males to be involved in the kinds of work relationships represented in these social service delivery systems.

Given the job options open to college-trained white men compared with women and nonwhites, working in such programs with their limited mobility opportunities is apt to be viewed by many as a career trap. A level of professional training, usually a bachelor's degree, is required to gain

entry, but beyond the entry point further education is unlikely to lead to substantial career progress and, to express it in the language of exchange theory, the resource that it represents is unlikely to be "bartered" for organizational advantages that may not themselves be particularly gratifying for a person whose major personal points of reference and ambition are external to the work setting. In fact, males who have superior credentials but nowhere to go and who see themselves as trapped in this manner may give off such evidence of disappointment and hostility that they are avoided unless they have other claims to the attention of their co-workers. This could help to account for the relative isolation of some of them from the networks of interaction (see Kanter 1976, p. 416 for a similar argument). In contrast to men, women, whether white or nonwhite, have fewer "comparison alternatives" outside the workplace and therefore do not fit this pattern.

The effects of authority enable us to extend this line of speculation. The ethsex to gain most from high rank was white males. In fact, for them, it appeared to be this factor that offset the disadvantages associated with education. It is our conjecture that white males with authority were able to command, not bargain for, access to the networks of interaction, and it is on this point that they differed most dramatically from women and from nonwhite men. Against this backdrop the situation of nonwhite men presents a special picture of inequality. As with white men, education represented more of a handicap than an advantage in gaining network access, but the counterbalancing effect of authority was missing. Probably their aspirations, like those of their white male counterparts, were focused outside the immediate work setting, and among those with superior credentials the feeling of being trapped would probably be similar. To be sure, the strategic resource represented by their community ties was a source of influence, but since it was their only reliable point of leverage a question could be raised about how personally and professionally gratifying the centrality it provided was to them.

CONCLUSION

In the programs we studied no category of practitioner monopolized or controlled all the scarce and valuable skills, and each category had some effective advantage to balance against its disadvantages. As a consequence, even those that we consider most likely to have encountered discrimination were not defenseless in the face of it. Different patterns will surely be found where the configuration of circumstances differs from that of the systems we investigated. However, we believe that a general inference suggested by our study will bear the test of replication: the leveling of access to the professional exchange networks came about more through

competitive and possibly conflictive interpersonal processes than through intrinsically rational and equitable organizational mechanisms. The formal structure and the public bureaucratic symbols of the agencies provided at best a rough framework within which the practitioners forged their distinct accommodations. In this process, dialectical relationships existed in which given achieved resources were effective for some but not for others and in which a given ascribed attribute or pairing of attributes could function simultaneously favorably and unfavorably for the individual. Certainly these considerations must be taken into account before it can be decided whether a given organizational arrangement is characterized by rationality or equity in the Weberian sense, and even when all the pieces are in place the answer to this question is still likely to be "yes, and no."

APPENDIX A

Computation of Test Statistic for Hypothesis 3

The computation of the test statistic is more complex for hypothesis 3 than for hypotheses 1 and 2 and warrants more discussion. The residual sum of squares from the constrained model is computed as follows:

$$ESS_c = \sum_{i=1}^{N}\Big[Y_i - \Big(a + \sum_{k=1}^{5} b_k X_{ik} + cM_i + dW_i + \sum_{k=1}^{5} f_k MX_{ik} + \sum_{k=1}^{5} g_k WX_{ik} + hMW_i\Big)\Big]^2,$$

where Y_i is the ith value of the dependent variable; X_{ik} is the ith value of the kth regressor; M and W are dummy variables for male and white, respectively; MX_{ik}, WX_{ik}, and MW_i, are product terms; and a, b_k, c, d, f_k, g_k, and h are estimated regression parameters. The residual variation from the unconstrained model is then

$$ESS_u = \sum_{j=1}^{4}\sum_{i=1}^{n_j}\Big[Y_{ij} - \Big(a + \sum_{k=1}^{5} b_k X_{ijk}\Big)\Big]^2,$$

where Y_{ij} is the ith value of Y in the jth gender-race subgroup; X_{ijk} is the ith value for the jth subgroup of the kth regressor; n_j is the size of the jth subgroup; and a and b_k are estimated regression parameters.

APPENDIX B

Community Activism and Client Composition

Scores on the four measures of practitioners' activism ranged from 1 ("Never") to 7 ("Almost Constantly"). In the order in which these items

are discussed in the text, the mean scores appear in the unnumbered table below.

	Item 1	Item 2	Item 3	Item 4
All subjects	2.20	2.95	2.83	3.21
White males	2.28	3.07	3.13	3.35
White females	1.95	2.63	2.58	2.77
Nonwhite females	2.43	3.32	2.68	3.36
Nonwhite males	2.82	3.69	3.32	4.57
Significance (ANOVA)	.018	.020	.087	.000

The partial correlations between centrality scores and the proportion of same-ethsex clients are presented in the unnumbered table below. The client measures are program proportions which were attached to the records of practitioners as variables. We have no direct data on actual practitioner-client ties. The findings for nonwhite males are as expected, and we call attention especially to the correlation (.86) between the proportion of nonwhite male clients and the centrality of the nonwhite male practitioner in the network of mutual support. Explanations of the findings for female practitioners and especially the anomalous result for white males are not apparent to us.

	Work Contacts	Influence	Respect	Support	Assistance
White males	−.42	−.51	−.37	−.63	−.15
White females	−.15	.05	−.04	.19	−.01
Nonwhite males	.28	.44	.43	.86	.28
Nonwhite females	−.004	−.06	−.19	−.02	−.27

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Social Constructionist and Positivist Approaches to the Sociology of Emotions[1]

Theodore D. Kemper
St. John's University

Social constructionist and positivist approaches to the sociology of emotions differ in three respects: (1) social constructionists generally reject the importance of the biological and physiological substrate in the determination of specific emotions, while positivists affirm the opposite view; (2) social constructionists suppose that emotions are largely determined by social norms for emotion, or "feeling rules," while positivists assert that social structure, particularly the outcomes of actors' power and status relations, determines emotions; and (3) social constructionists, following a symbolic interactionist model, propose that actors must define situations before emotions will be experienced—but they do not explain how this is done, or what categories actors use to help them define situations; positivists on the other hand offer a specific social structural category scheme for defining situations and determining the emotions those definitions produce. These issues are discussed and suggestions for reconciling the two views are proposed.

Two articles on the sociology of emotions have appeared recently in this *Journal* (Shott 1979; Hochschild 1979). Although the topic is relatively new, it already reflects one of the fundamental divisions of the discipline, that between social constructionist and positivist approaches.[2] Shott and

[1] Some of the ideas of this paper were presented at colloquia in the Departments of Sociology at the University of California, Santa Barbara, and Stanford University. Requests for reprints should be sent to Theodore D. Kemper, Department of Sociology, St. John's University, Jamaica, New York 11439.

[2] While these terms are not perfect designations for the positions described in this paper, they are current in discussions of present-day orientations in sociology and capture certain important differences in emphasis that apply to the study of emotions (cf. Zeitlin 1973; Rossides 1978; Collins and Makowsky 1978). Here they are used as ideal types and are meant to differentiate broadly between those who view emotions as social constructions or as interpretive accomplishments of the actor, and those who view emotions as more or less jointly determined by social structure and biology; between those who emphasize idiographic, historical, and cultural factors, and those who emphasize nomothetic, transhistorical, and cross-cultural understanding; between those who generally reject efforts to predict behavior and emotions, and those who venture to do so; between those whose methods include participation, personal observation, and an emic (Pike 1967) approach, and those whose methods include experiments, statistical manipulation of data, and an etic approach. Despite these differences, individual practitioners are often eclectic and thus may not fit neatly into either ideal type.

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Hochschild both espouse the former view. The purpose of this paper is to examine the social constructionist position and to argue that it errs in certain respects and in others presupposes a positivist understanding of emotions. To an important degree the two approaches are not so much antagonistic as complementary. A major problem is that the constructionist position is both too ambitious and not ambitious enough. This will become clear as we proceed.

In general, social constructionists hold that there is great plasticity to human emotions because emotions are largely disconnected from biology (e.g., Shott 1979). This allows social and cultural norms (e.g., Hochschild's [1979] "feeling rules") to determine almost exclusively the emotions appropriate in given situations, or significantly to guide their construction by the actor (Shott 1979). Cognate with these ideas is the view that individuals experience emotions only after interpreting and defining their situation (Shott 1979; Hochschild 1979; Averill 1980), although social constructionists offer few clues about how actors actually define situations to produce specific emotions.

The positivist approach, on the other hand, maintains an important link with biology and the physiological concomitants of emotions; rejects the view that cultural norms determine emotions, looking instead to social structure and to the outcomes of social relations; and, while accepting the constructionists' view that emotions depend on how the situation has been defined, unlike them, offers concrete, empirically grounded formulations for how actors define situations.

I shall treat these differences in turn. Before this a terminological note is in order. I shall use the term "social structure" to mean the vertical arrangement of actors relative to each other along the relational dimensions of *power* and *status*. Although this is not a common definition, it will be seen to come close to many sociologists' understanding of the term. By power I mean actions that are coercing, forcing, threatening, punishing, and the like, producing thereby a relationship of domination and control of one actor by the other. This definition of power follows more or less in the tradition of Weber (1946, p. 186), who speaks of power as the ability to realize one's own ends, "even over the resistance" of the other (see also Collins 1975). Social structure consists, in part, of the positions of actors vis-à-vis each other along the power dimension.

Status, on the other hand, is a scalar dimension reflecting the amount of uncoerced, willing compliance, approval, deference, reward, praise, emotional or financial support, even love, that actors accord each other. As in the case of power, some actors ordinarily receive more status, others less. This notion of status has been elaborated in the work of Barnard (1946), Zelditch (1968), and Kemper (1974; 1978*b*).

For many analytical purposes the distributions of power and status are

the important elements of what we ordinarily discuss under the rubric social structure. Social structure thus defined is a general concept. It applies at the macrolevel, as in the relations between social categories (e.g., male and female), organizations, social classes, or societies; it applies also at the microlevel, where the actors are individuals in interpersonal relations. This definition of social structure is thus focused on power and status relationships and is meant to contrast sharply with social and cultural norms and prescriptions as a social stimulus for emotions. As for the power and status dimensions that form the social structure, I shall justify these further below. Here I wish to obtain only a provisional assent so that the distinctive features of the argument between constructionist and positivist views of emotions can be brought sharply into focus. I turn to these now.

EMOTIONS AND BIOLOGY

Sociologists of emotion, whether social constructionist or positivist, seek the social stimulus sources of emotions, introducing sociological insight into what was once almost exclusively a psychophysiological domain. But the psychophysiologists themselves are split into two camps in their understanding of emotions, and this has some bearing on the views of sociologists. One camp holds that the underlying physiological processes for all emotions are the same, and that only cognitive labeling and social settings differentiate emotional experience (see Schachter and Singer [1962] for the classic statement of this position). This is a very beguiling hypothesis for sociologists, and social constructionists have eagerly adopted this view, since it allows for relatively great malleability of emotions according to certain social and cognitive factors (Shott 1979).

With virtually no physiological complications to hinder the way, constructionists can support a relatively simple solution to the problem of explaining emotions, namely, *cultural and social norms,* as these are seen to apply in the given situation. The norms simply tell us what to feel (Shott 1979; Hochschild 1979; Averill 1980): sad at funerals instead of happy, happy at weddings instead of sad, and glad at our successes instead of bad.

However, there is a second psychophysiological opinion, which holds that physiological processes differ for different emotions (see Funkenstein [1955] for the classic statement of this position). *This in no way negates a sociological or social stimulus approach to emotions.* It does strongly imply that social stimulus and physiological process in emotion are not indifferent to each other, but are related more like key and lock: particular social stimulus keys fit particular physiological locks to produce particular emotions. Perhaps the most important sociological consequence

of this particularist psychophysiological theory is that it is especially congenial to a certain type of social stimulus: specifically, variables denoting the operations of social structure—that is, the power and status relationships of actors. These rather than social norms produce emotions. Different outcomes in power and status relations instigate different physiological processes, which are in turn related to different emotions. This is the positivist position (Kemper 1978*b*).

Indeed, a sociological theory of emotions cannot be indifferent to the psychophysiological theory with which it must ultimately link in any complete theory of emotions. When constructionists reject or sharply limit the physiological contribution to emotions, they support the social-norm explanation they favor. When positivists argue for the importance of physiological processes, they are supporting the social structural explanation.

The issue cannot be settled conclusively here, but it is instructive to examine a recent constructionist analysis of supposed social and cognitive latitude in the determination of emotions that misconstrues the data of a flawed experiment, leading to a wrong inference about the role of physiological processes, ergo a wrong inference about the importance of cognition and social norms. The problematic argument is found in Shott (1979).

According to Shott's (1979) summary statement of her constructionist position, "affective labeling of physiological states can be manipulated . . . within a range bounded by the recognition of cues indicating some minimal physiological arousal and by *extremely intense* arousal" (p. 1330, emphasis added). This says, in effect, that within very broad limits of physiological arousal, virtually any emotional label can be pinned to the arousal. Hence physiology plays a relatively restricted role in the determination of emotions. This conclusion by a sociologist must inevitably depend on the work of psychophysiologists, in this case Nisbett and Schachter (1966). They proposed, in words that Shott is paraphrasing above, but whose carefully qualified application she has not rendered accurately, that "we should be able to alter the labeling of a bodily state only within a range bounded at the lower end by the existence of some arousal, and at the upper end by experiences so extreme that no amount of manipulation of cognitions will persuade the individual to attribute his bodily state to an artificial source" (Nisbett and Schachter 1966, p. 228; also quoted in Shott 1979, p. 1323).

Since constructionists allow for physiological arousal—otherwise there would be nothing to label—the issue seems to turn on the question of how "extreme" the arousal has to be before arbitrary labeling fails. In the experiment in which Nisbett and Schachter supposedly confirmed their conclusion, and upon which Shott's constructionist proposition relies, the matter comes into focus in certain differences between a "low fear" and a "high fear" group.

Subjects in the low-fear condition reported an average fear score of .52 measured by responses on a four-point self-rating scale in which 0 = not at all, and 3 = extremely. Subjects in the high-fear condition reported an average fear score of .96. The spread between the two means was statistically significant, but the substantive difference appears to be slight, less than half-a-point on the four-point scale. More important, however, for evaluating Shott's conclusion is the location of the (high-fear) "extreme" group on the fear scale. Assuming that the verbal anchor for the number "one" on the rating scale was a word signifying "a little," or "somewhat," the extreme group in this experiment was feeling *less than* "a little" or *less than* "somewhat" afraid. Since this group failed to mislabel its emotional state, though it supposedly had some opportunity to do so, it must have passed beyond the extremity limits proposed by Nisbett and Schachter (1966, p. 228) to a point where "no amount of manipulation of cognitions will persuade the individual to attribute his bodily state to an artificial source."

Yet this group was only very moderately afraid. Thus the results seem to show that the range of *non*manipulability of cognitions for purposes of emotional labeling (i.e., between a score of less than one and a score of three on the scale) is quite large. Contrary to Shott, who misconstrued Nisbett and Schachter, we could conclude from this experiment that *only within a very narrow range, at very low levels of emotional arousal,* can individuals be induced to mislabel their emotional states. Hence real physiological cues dominate emotional attributions even at relatively low intensities of emotion. It means also that physiological concomitants of emotion play a much stronger role in the determination of emotion than social constructionists generally acknowledge. Properly interpreted, the results appear to support the positivist position.

Would that this were all that needs to be said about Nisbett and Schachter's experiment. The above corrects Shott's misinterpretation of their relatively careful statement about the manipulability of emotional experience by cognitive labeling. Unfortunately, the experiment contains a flaw that invalidates any interpretation at all, whether constructionist or positivist.

Shott (1979, p. 1323) reported that "only certain subjects—those in a low-fear, low-shock[3] condition—could be induced to misattribute arousal caused by shock and fear to a placebo." The flaw is that the experimental paradigm allowed the supposedly high-fear subjects to experience fear arousal and its symptoms *before* learning that they would also be ingesting a (placebo) drug. Therefore, they would not be likely to attribute their arousal to the placebo, which they obtained only after fear was aroused. On the other hand, the low-fear group, which, significantly for

[3] There is, in fact, no separate "low-shock" condition in the experiment. It is the same as "low fear."

Shott, misattributed its fear and shock symptoms to the placebo, was entirely disabused of either fear or the likelihood of any effects from shock. They were told: "Before you get worried, let me tell you not to let the word 'shock' bother you. There's not going to be any discomfort and I'm sure you'll enjoy the experiment" (Nisbett and Schachter 1966, p. 229). These subjects were told that the supposed drug they took would produce symptoms that in reality stem from fear or electric shock.

We might surmise that unafraid subjects who are naive about the symptoms accompanying electric shock, and who are specifically told by a straight-faced experimenter to expect these symptoms from a drug, are likely to misattribute their symptoms to the drug. Even so, only six out of 10 subjects made this misattribution. However, Shott implies a comparison between this group and a high-fear group in which only two out of 12 subjects misattributed their symptoms to the placebo. But we see that such a comparison is invalid, since the symptoms of the high-fear group were instigated before the placebo was administered.

I have gone into some detail in order to show how flawed the evidence is for the constructionist conclusion that affective labeling can be manipulated within a very wide range, from "minimal physiological arousal [to] extremely intense arousal" (Shott 1979, p. 1330).[4]

Ignoring the flaws just discussed, there is a further point to make about Shott's constructionist argument based on the Nisbett and Schachter experiment. Shott acknowledges that at some level of arousal no amount of mislabeling or social construction will work. Thus, the original stimulus of arousal—and there is always such a stimulus—will lead to an authentic and apparently genuine labeling of the emotion. This suggests that for the constructionists there are two different principles of emotion: one is constructionist, with arousal presumably being labeled however social norms or cognitive interpretation would have it; the other is labeling according to an authentic, stimulus-emotion nexus. A positivist must ask whether it is theoretically economical to endorse two principles of emotion, with some indefinite boundary line between their operations, thus leaving a large *terra incognita* in between, where we do not really know which principle is operating.

The positivist view here is more straightforward. Individuals label their emotions according to the relational conditions of their situation (this will

[4] Although I have dealt exclusively here with Shott's position on the question of biology, other social constructionists hold somewhat similar views, for example, Averill (1980, p. 313), who writes that "basically [he agrees] with much of what Schachter has to say"; or Hochschild (1979, pp. 553–54), who describes an "organismic view" of emotions—"in large part, biological factors account for the questions the organismic theorist poses" (p. 553)—and dismisses it as insufficiently sociological because it does not deal with "feeling rules." Elsewhere, Hochschild (1977) also endorses the Schachter and Singer position.

be discussed below). If someone insults us, we usually know it, and the anger we feel is not tacked onto our arousal out of a grab bag of possibilities, which is what the Schachter and Singer paradigm essentially offers. We may not always be aware of what caused us to feel a given emotion, or even what emotion we are feeling. But this is different from saying that once aroused—in ordinary circumstances—any emotional label can be assigned to our feeling.

Currently, the entire tradition of work in psychophysiology (starting with the classic Schachter and Singer [1962] experiment) which underlies the social constructionists' view that physiological arousal does not significantly discriminate emotions is now under heavy attack (Kemper 1978*b*, chap. 8). Some recent experiments attempting to replicate or confirm this work have failed to do so (Maslach 1979; Marshall and Zimbardo 1979). Even in experiments supposedly favorable to the tradition (e.g., Erdemann and Janke 1978) results are ambiguous at best (see Kemper 1980).

Furthermore, social constructionists unaccountably ignore the large body of contrary evidence that does find physiological differences between different emotions. I refer to the "Funkenstein hypothesis" (Funkenstein 1955), evidence for which permits the further surmise that physiological processes (specifically those involving the chemical neurotransmitters norepinephrine and epinephrine) and social relations of human actors (specifically those involving power and status acts) may be linked in a single theoretical frame (Kemper 1978*b*). While this view is clearly speculative at present, what must be counted important is that there is any ground at all for such a position. (This link between physiology and sociology is not related in any way to the controversial approach of sociobiology [Wilson 1975]. Space does not permit presentation here of the full details of the argument, which has been abundantly developed in Kemper [1978*b*].)

From a positivist point of view, there is nothing antisociological in finding that physiology plays a differentiating and crucial role in the emotions. There is still sufficient room for sociological considerations, as will be presented below.[5]

[5] There are two hindrances to a rapprochement between sociology and physiology. One is that sociologists would have to learn some physiology. This is not difficult, since the fundamentals of the field are well-ordered, and, generally, knowledge is reliable. But it would be arduous keeping up with the literature. The second is that sociologists may fear giving up their birthright through any accommodation with biology. Although sociology seriously begins where biology leaves off (Kemper 1978*b*, p. 27), the controversial field of sociobiology makes the extravagant claim that sociology is subsumable under the rubric of biology (Wilson 1975). These difficulties aside, sociologists might want to consider that somewhere in human behavior and physical structure there is an *interface* between social and biological phenomena. The Funkenstein hypothesis, and its further development in Kemper (1978*b*), is valuable because it gives us a glimpse of that point of contact between the two disciplines and their

CULTURAL DETERMINISM OF EMOTIONS

Having largely rejected the influence of biology on emotions, social constructionists generally argue that cultural prescriptions and social norms are the most important determinants of emotions. With the idea of "feeling rules," Hochschild (1979) elevates these norms to the status of a special concept. She writes, "If we reconsider the nature of emotion . . . we are struck by the imperial scope of social rules" (1979, p. 551); and "The social guidelines that direct how we want to try to feel may be describable as a set of socially shared, albeit often latent (not thought about unless probed at), rules" (1979, p. 563). These feeling rules presumably explain why "generally speaking, . . . people feel gay at parties, sad at funerals, happy at weddings" (1979, p. 552). For Averill (1980, p. 305), "emotions are . . . transitory social roles, or socially constituted syndromes. The social norms which help constitute these syndromes are represented psychologically as cognitive structures." Averill further links roles together in the appropriate dramatic metaphor of a "plot." He continues: "In the case of social roles, the plot is the cultural system. This means the emotions can only be understood as part of the culture as a whole" (1980, p. 315).

Shott (1979, p. 1319) too sounds the cultural theme: "Social norms clearly have substantial impact on the interpretation, expression, and arousal of emotion, a point succinctly made by Clifford Geertz (1973, p. 81): 'Not only ideas, but emotions too, are cultural artifacts.' "

In order to establish that social norms determine emotions, constructionists point to differences in emotional expression in different cultures (Shott 1979, p. 1320; or to changes in feeling rules over time (Hochschild 1979, pp. 567–68); or to be the existence of normative guides to behavior in situations involving feelings and emotions, such as *The Art of Courtly Love* by Andreas Capellanus ([ca. 1185] 1941) dating from the 12th century.[6]

All this is very classically sociological. Question: What explains the uniformities of behavior (or feeling) in society? Answer: Cultural norms. By this argument, failure to emote according to social rule is deviant (Hochschild [1979, p. 560] even speaks of "affective deviance") and must be understood according to such standard explanations as cultural or subcultural differences (Shott 1979; Hochschild 1979), failed socialization (Shott 1979), or pathological personality (Shott 1979, p. 1330; Hochschild 1979, p. 560; Averill 1980, p. 331).

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very different levels of phenomena. The social constructionist approach to emotions severely minimizes interest in this interface and the questions it poses.

[6] This reference is found in the preprint of Averill (1980) but was apparently edited out because of space limitations.

Explanations of this kind have been tried before in sociology. For example, Parsons (1954) explained gender roles in this way. Lewis's (1964) "culture of poverty" is a similar type of explanation. The trouble with both of these, as has been pointed out—and the major failing of all cultural norm explanations of social regularities—is that they ignore social structure. If women had chosen in the past (taking Parsons's description as accurate) to pursue only certain roles, it was not simply because that was the cultural norm, though indeed it was a norm. It was because men and women were related to each other in a relatively firm structure of dominance and submission (power) and of differential deference and demeanor (status) in which males preempted the roles they desired because they had more power to do so and thus could promote more status for themselves. Women's roles were residual, and the norms simply underwrote the existing structure of power and status (Kemper 1974).

In the case of the so-called culture of poverty, it has been shown that social structure, not culture, dominates underclass behavior and outlook. The actions of lower-class individuals are not so much the result of a cultural pattern acquired through socialization as they are an adaptation to very low power and very low status in society, as expressed in unemployment, underemployment, and associated deprivations of housing, nutrition, and cultural amenities (Valentine 1968; Coward, Feagin, and Williams 1974).

Nor need the explanation of emotions rely on cultural or social norms. There are, of course, norms that prescribe certain emotions in certain situations. Thus, there certainly is a cultural prescription for feeling sad at a funeral. But what if the dead person was your personal enemy or a barbarous political tyrant? Must we then go to another cultural prescription or to failed socialization or to pathology to explain why one might in the circumstances described feel happy at the funeral? And if one is gay at a party, is one responding to social norms to "feel gay"? Would that emotional life were as plastic as the social constructionists maintain! In that case there would rarely be dull parties.

The ineluctable fact is that social relations of power and status, sometimes crystallized into an enduring structure, sometimes fluid and evanescent, determine our feelings at funerals, parties, weddings, and similar occasions of interaction. I refer here to real emotions, not forced smiles at parties where we feel bored, or put-on tragic miens at funerals where we feel indifferent. It is what our fellow participants do to us and what we do to them—the social relations that constitute the existing social structure—that evoke our emotions. At a party where others cater to us by their warm welcome and expressed pleasure at our presence, where they feed our mind with witty talk or funny jokes, where our visual sense is treated to the

stylish and revealing garb of others, where hosts have shown themselves to have spent or labored mightily to offer us good drinks or foods, where we can make physical-sexual contact with other persons who unmistakably welcome our touch and firm embrace, and so on . . . it appears that then—our status so enhanced—we might feel gay. Otherwise, social norms to the contrary, forget it!

Similarly, we do not grieve or feel the depressed emotional state of mourning at a funeral, feeling rules to the contrary, unless we were in a state of happy relationship to the dead person, or to his or her survivors, which evokes grief or mourning sentiments from us because of the loss of that relationship or our sympathy for those who have lost it. If we hated the son-of-a-bitch, that is, if he used so much power on us, or gave us so much less status than we felt we deserved, he will more than likely go to the grave unmourned by us; except insofar as any death is a premonition of our own death and thus gives us a depressing sense of the futility of our strivings.

Cultural and social norms for feelings *do* exist. But these are epiphenomenal, pointing to the mere surface of the phenomenon, rather than to its explanatory core. Social constructionists cite Durkheim in support of their position (e.g., Hochschild 1979, p. 556). But Durkheim was not a social constructionist on the question of emotions. Consider the following:

> If, as often happens, we see in the organization of the family the logically necessary expression of human sentiments inherent in every conscience, we are reversing the true order of facts. On the contrary, it is the social organization of the relations of kinship which has determined the respective sentiments of parents and children. They would have been completely different if the social structure had been different, and the proof of this is, in effect, that paternal love is unknown in a great many societies. [Durkheim (1933) 1964, pp. 349–50]

But should we assume that paternal love is unknown, as Durkheim says, because social norms for paternal love are lacking? They may indeed be lacking, but Durkheim ([1933] 1964) explains his assertion in a footnote to the material quoted: *"This is the case in societies where the matriarchal family rules"* (p. 350, emphasis added). If this does not conclusively establish where Durkheim stood in the conflict over whether rules or social structure takes precedence, the following should also be considered:

> There are certain ways of mutual reaction which, finding themselves very conformable to the nature of things, are repeated very often and become habits. Then these habits, becoming forceful, are transformed into rules of conduct. The past determines the future. In other words there is a certain sorting of rights and duties which is established by usage and becomes obligatory. The rule then . . . only expresses in clear-cut fashion the result of a given situation. [Durkheim (1933) 1964, p. 366]

Mutatis mutandis, we can say the same about the relationship of rules for feeling to the underlying social interactional realities that give rise to feelings. First comes the social relationship, with its concrete behavioral pattern and its characteristic emotions. Only later does a rule emerge to guard the relational pattern by guarding the emotions it evokes.

The relationship between social structure and norms for emotion was brilliantly conveyed by Elias ([1939] 1978), who analyzed the historical development of table-and-toilet behaviors, which have become increasingly circumscribed by rules of courtesy, etiquette, civility, and the like, and how, as a result, the frontiers of such emotions as shame, disgust, and contempt have shifted over the centuries. Such an argument would ordinarily fuel the social constructionist position, since it seems to say that we feel these emotions because social norms have arisen to define certain situations as deserving them. Yet, as Elias shows, social structure determines the norms and the ensuing emotions. As the social structure of Renaissance Europe changed—the centralization of power, the emergence of court life with its dependence of courtiers on the pleasure and preference of their princes and rulers, the growth of a bourgeoisie whose rising power threatened the favored status of courtiers and the associated nobility, the apparent need of the bourgeoisie to distance itself from yet lower strata in society (peasants and urban proletariat)—as these social structural conditions emerged, so did the prescriptions for behavior whose violation engendered specific emotions. But, as Elias makes incontrovertibly clear, the process of behavior change, of prescription, of emotional differentiation resulted from the effort to maintain or enlarge status differences, to demarcate social relations and social structure with unmistakable clarity, so that all would know that just precisely such-and-such was required in order to meet the demands of status due.

Elias indicates two social relational processes at work. First, a ruling elite as a group, in more or less frequent interaction in the environs of the royal court, developed particular patterns of behavior involving speech, food, toilet habits, etc. This was inevitable, since it appears that every human group in close contact develops its own behavior pattern. This pattern was designed to render the appropriate status due to one's peers, with whom one lived in the relatively close quarters of the court, and especially great courtesy to the prince, upon whose favors one's standing in the court depended. Yet this courtier group came in contact also with lower groups, whose manners and behavior patterns, nurtured among themselves, were inevitably different. Whose manners of speech, of dress, at table, etc., would take precedence? It is, in fact, the very sign of higher status that the lower group will defer, that its pattern of behavior will be treated as less desirable, not only by the higher group, but by the lower group as well. This is a true status system, as described by Weber (1946).

Yet, if the lower group does not voluntarily defer, the ruling elite also has the power to enforce its way. Nor need the power show in more than a sneer. Thus a set of rules for relational conduct developed by a small leading group took precedence as the code for all groups. Violation of the code not only revealed social incompetence, which evoked embarrassment and shame, but also betrayed one's less than courtier origins, which evoked contempt from one's superiors.

A second process helped further to differentiate manners and emotions. The pattern of the ruling elite inevitably diffused down to those of lower status who were in contact with it, thus blurring the status distinction. If the bourgeoisie had table manners as refined as those of aristocrats, what was to tell them apart? Refining further the original differences, pushing further the frontier of emotional differentiation, became a way of reaffirming the social structure, so that the lower rank was virtually always off balance, so to speak: behind the times, puzzled at the new, comical in its *arriviste* puffing and panting to keep up, and contemptible because of its inevitable gaucheries in trying to do so. Ultimately, changes in social structure abolished this game, for the aristocracy failed for lack of money. The bourgeoisie got to buy "everything," as Weber (1956, p. 179) put it (cited in Form and Rytina 1969, p. 20), even to become the leading arbiters of taste, manners, and the proper emotions of the alimentary canal.

Rules of etiquette, manners, etc., are thus code books for the kind of behavior that allows status to be conveyed to persons of different rank. They are guides to how to enact a social relationship of a given kind. *The Etiquette of Race Relations in the South* by Doyle (1937) was the accurately titled description of the relational behaviors that whites and blacks engaged in to reflect and preserve the social structure (or relationship) between blacks and whites. Violation of racial etiquette by blacks would evoke white anger and black fear, because anger follows from a loss of status where the other is responsible for the loss, and fear is evoked when one is relatively defenseless, as blacks were, against the power of the other (Kemper 1978*b*, chap. 6).

In the matter of etiquette at the milder level of mere courtesy, failure to observe the implicit relational rules provokes emotions of contempt, embarrassment, or mild anger, depending on who is sinned against and who does the sinning.

Some sense of the problematic nature of social norms as the principal determinants of feeling can be found in the following statement by Hochschild (1979, p. 563, n. 10):

> That we can single out such a thing as "feeling rules" is itself a commentary on the ironic posture of the self legitimated in modern culture. Modern urban cultures foster much more distance (the stance of the observing ego) from feeling than do traditional cultures . . . [which] put people

> directly inside the framework of feeling rules and remove ironic distance and sense of choice about them.

This seems to say that in traditional cultures almost everyone feels what he or she is supposed to feel, and therefore there is no need to "manage" feelings according to social norms. Were there such a need, there would have to be feeling rules; but since there is no need, feeling rules do not exist. The idea of "people directly inside the framework of feeling rules" appears to be a circumlocution for this idea. But if there are no feeling rules in traditional cultures, how do members of such cultures learn to feel glad at parties, sad at funerals, etc.? Or could it be that there *are* feeling rules, but they are usually successfully internalized in childhood, thus not requiring further consideration in adulthood? This would be tantamount to claiming that socialization of feelings is virtually a total success. But if socialization of feelings is so successful, why not perfect behavioral socialization as well, which may be less difficult than the perfect socialization of feeling? In which case, deviance, like feeling rules, would prove to be a peculiarly modern concept.

It is easy to see that logical problems are engendered by Hochschild's effort to accommodate the notion that feeling rules may not be accessible in all cultures. Of course, another solution to this dilemma is to view emotions as products of social relational outcomes which directly evoke emotions, that is, to take a positivist social structural approach. Feeling rules may then exist in modern societies, but only as part of our greater self-consciousness about processes carried on by other means, in perhaps the same way that both we and primitive cultures have art, but only we have developed what is virtually an art about art—criticism—and, to show how far this can be taken, art about art about art, as exemplified by I. A. Richards's (1948) elegant work.

Averill (1980, p. 320), apparently also uncomfortable about some inaccessible aspects of the constructionist mechanism, writes that "adults[']emotional reactions are likely to proceed automatically . . . without verbal mediation." He means here something a little different from what Hochschild seems to mean by people being "directly inside the framework of feeling rules," but there is the same sense of a "blind spot" where the process disappears from view. A nagging problem for a positivist is that both Hochschild and Averill abandon rules for feeling just at the point where it would seem most crucial for them to be available if the case for them is to be proved.

It is not as if some hint of the relational provenience of emotions is totally lacking among social constructionists. Shott (1979, p. 1318) cites Kemper's relational outcome definition.[7] Averill (1980, p. 314) writes that

[7] Emotions result from "real, imagined, anticipated, or recollected outcomes of social relationships" (Kemper 1978*a*, p. 32).

"the person who says, 'That makes me angry' or 'I love you,' is not simply labeling an internal state; he is entering into a complex relationship with another person." While he does not go on to see the relational matrix that gives rise to such emotions in the first place, at least a sense of relationship is indicated. However, the most interesting relational allusion is found in Hochschild (1979).

She is concerned with the problem of "managing emotions," that is, getting one's emotions to conform to what one understands the "feeling rule" to be. She suggests certain techniques for emotion management, including "the cognitive":

> There may be various types of cognitive emotion work. All can be described as attempts to recodify a situation. By recodification I mean reclassification of a siuation into what are previously established mental categories of situations. *As in an initial, more automatic codification of a situation,* deliberate recodification means asking oneself (*a*) What category in my classification schema of situations fits this new situation? (the schema may include *blame-in* situations, *blame-out* situations, *credit-in* situations, *credit-out* situations, etc.). . . . In deliberate recodifications, one tries to change the classification of outward and inward reality. [Hochschild 1979, p. 562, n. 9, emphases added]

This statement is noteworthy in that it offers some definitions of situations that are relational in character. Blame-in situations must be where the actor was agent in hurting or harming another; in blame-out, agent and victim are reversed. Credit-in situations must be where the actor was the agent of an act meriting reward from others; in credit-out, the other is the agent and deserves the reward. These formulations hint at some systematic way of classifying situations in relational terms. The few given are assimilated easily into the comprehensive power-status relational scheme found in Kemper (1978*b*).

A second and revealing point about Hochschild's statement is the acknowledgment of two kinds of relational classification: the "initial, more automatic codification of a situation" and the "deliberate recodification." Significantly, both do the same thing, namely, classify situations relationally, *and this produces the emotion.*

A brief example illustrates this point: I see my spouse at a distance and I feel authentic happiness (she is the epitome of the ideal person in how she treats me and others). On another occasion, I see my spouse at a distance and (for whatever reason) I do not feel happy, but I know the feeling rule that says I should. I then think about the perfect way she treats me and others, and I become authentically happy. In both cases, the emotion was produced, not by a feeling rule, but by understanding my relationship with my spouse.

There are two final issues to examine in the matter of the cultural de-

termination of emotions. The first is that some societies do not appear to report the same emotions as does ours; the second is that some standard emotions in these societies are expressed differently from our mode of expression. These societal variations seem to support the social constructionist view of emotions. According to Shott (1979, p. 1319), the existence of social norms would lead us to "expect distinct vocabularies of emotion to characterize different societies, and, in fact, they do." An illustration of this is that among Tahitians "such emotions as fear and shame . . . are culturally expected and encouraged and used as a principle of explanation, while passionate feelings (particularly hostile or sad ones) are made 'culturally invisible,' almost inaccessible to communication (Levy 1973, pp. 273, 307, 324). Thus, a Tahitian whose mate has died interprets the subsequent 'strange' feelings of loss as illness (Levy 1973, p. 324)" (Shott 1979, p. 1320).

Levy's (1973) work on the Tahitians is of relevance to both issues. On the question of differences among societies in their vocabularies of emotion, Levy distinguishes between emotions that are "hypercognated" (having a relatively extensive emotional vocabulary, allowing for nuances and relatively acute discriminations of feeling) and emotions that are "hypocognated" (with a relatively meager vocabulary, allowing few affective discriminations). Among Tahitians, hypercognated emotions include anger, shame, and fear, while hypocognated emotions include loneliness, depression, and guilt (Levy 1973, p. 287). Socialization practices, as reported by Levy, undergird this differentiated emotional vocabulary.

Adopting a social constructionist position, one might be tempted to believe that hypocognated emotions are not experienced, but Levy (1973, p. 324) writes:

> When Tihoni [a Tahitian], feeling strange after being separated from his *vahine* [wife], interprets his feelings as illness and in so doing accepts a pervasive cultural pattern of playing down feelings of loss, it is evident that in some way and at some level he must know that he has suffered a significant loss. That is why his separation from his *vahine* made him feel sick or strange in the first place. That is, one "feels" considerably more than cultural forms make consciously accessible.

Here Levy is in fact supporting a positivist view of emotions, for he is saying that, *despite the lack of cultural vocabulary,* the feeling of loss still results from the fact of loss, from the relationship now severed. (Tihoni's problem was that his wife had refused to return with him to their home island after several months of visiting her father. She also kept their only child with her. It is clearly the relational loss that stimulated Tihoni's discomfort.) But, in general, such feelings are not frequent among Tahitians. Levy (1973, p. 306) explains: "For one thing, most people are seldom alone. When they lose someone through desertion or

death, they soon find someone to take his place." Here, again, Levy supports a positivist, social structural explanation for the relatively low prevalence of sadness or loneliness, and, therefore, for the hypercognation of this emotion.

Social constructionists also make a point of the fact that the sense of loss among Tahitians is expressed verbally as a feeling of illness (Shott 1979, p. 1320), rather than in a conventional language of loss. Levy writes that "there are no unambiguous terms which represent the concepts of sadness, longing, or loneliness in the sense of 'depressed' or 'sad' because of lack of friends, companionship, and so on" (p. 305). Tahitians might use such terms as: " 'feeling troubled' . . . 'not feeling a sense of inner push' . . . 'feeling heavy' . . . 'feeling fatigued' . . . and a variety of other terms all referring to a generally troubled or subdued bodily state" (p. 305). Thus, again, social construction appears to dominate the emotions. But these bodily symptoms are part of a general syndrome of depression. Murphy, Wittkower, and Chance (1970) surveyed psychiatrists in 30 countries to determine whether or not there is a common, cross-cultural syndrome of depression. Their conclusion was that the most reliable symptoms, cross-culturally, were "depressive mood, fatigue, insomnia, and loss of interest in the social environment" (p. 480). While the fit between the items of the cross-cultural syndrome and Tahitian symptoms is not perfect, clearly there is a sufficient overlap to show that Tahiti is not culturally unique in its expression of depression.

Levy (1973, pp. 325–26) also concludes:

> Although we have seen various special Tahitian shapes and nuances of the phenomena associated with various feelings and emotions, the central tendencies referred to by such terms as *riri* (anger), *ri'ari'a* (fear), *here* (love), *mihi* (grief), and *mauiui* (pain) are . . . the same as those in the words used to gloss them.

Thus, although nuances differentiate Tahitian forms of expression of emotion from those of the West, and a different form of social organization leads to a lower likelihood of certain emotions (e.g., loneliness or depression), the crucial point is that, when emotions are felt, it is because of the same fundamental relational conditions that evoke them elsewhere, and, in important respects, the "central tendencies" of their expression are not different from what we experience in the West.

CONSTRUING, INTERPRETING, OR DEFINING THE SITUATION

According to social constructionists the normative determination of emotions does not occur automatically. Something intervenes between feeling rule and feeling, specifically, a stimulus situation which must be construed, interpreted, or defined.

On this point, Shott explicitly adopts a symbolic interactionist orientation, which "focuses on the actor's definitions and interpretations and on the emergent, constructed character . . . of an emotion" (Shott 1979, p. 1320). Averill (1980, pp. 309–10), too, takes the symbolic interactionist position: "The experience of emotion involves an interpretation of one's own behavior," and "the object of an emotion is dependent upon an individual's appraisal of the situation . . . a meaning imposed on the environment—a cognitive construction, so to speak."

Hochschild (1979, p. 566), on the other hand, introduces the notion of " 'framing rules' . . . according to which we ascribe definitions or meanings to situations. For example, an individual can define the situation of getting fired as yet another instance of capitalists' abuse of workers or as yet another result of personal failure." Elsewhere: "A situation (such as a funeral) often carries with it a proper definition of itself ('this is a time of facing loss'). This official frame carries with it a sense of what it is fitting to feel (sadness)" (1979, p. 563).

Hence social constructionists agree that the situation must be defined or construed before an emotion can be experienced. Only the most unreconstructed behaviorist would disagree. But questions remain: How should a science of emotions describe situations? Is there a systematic way to do so? Ought the sociology of emotions attempt to define situations beforehand, eticly (Pike 1967), so that emotions are predictable? Can we assume that actors are capable of defining situations in a standard way, so that, regardless of who the actor is, if a situation is defined in a certain way, one and only one emotion will be experienced?

Social constructionists appear to part company on these questions. From the apparent implication of "framing rules," we may gather that Hochschild would accept a predictable set of emotions, if the framing rules (and the feeling rules which go with them) were known. Shott, on the other hand, appears to follow a radically idiographic perspective, as her frequent citations of Blumer show. (Blumer [1962] is acknowledged to be the major proponent of what might be called the "indeterminacy principle" in the analysis of human action. Because of the emergent nature of action and emotion, we presumably cannot know them beforehand.) Yet, if emotions depend on the interpretation of the situation, it seems that all who define the situation similarly ought to experience the same emotion. The problem, in part, comes down to whether or not it is possible to have a standard set of categories for defining situations which will link them logically and empirically with emotions.

The social constructionists have not provided this. A faint hint of such a scheme is given by Hochschild in the notions of blame-in, blame-out, etc., cited above. But these do not constitute a comprehensive set, nor is there any attempt to articulate them with specific emotions. Shott, who

it might be thought would eschew efforts to classify situations a priori, nonetheless does so. For example, guilt is evoked when an actor apparently construes the situation to mean that he "commits or contemplates some 'immoral' action, then takes the role of the generalized other (or some significant other) and accepts its perceived judgment of oneself as morally inadequate" (Shott 1979, p. 1325). Also, "shame is provoked by the realization that others . . . consider one's *self* deficient, while embarrassment arises from awareness that others . . . view one's *presentation* of self as inept" (Shott 1979, p. 1325, emphasis in original). As in the case of Hochschild's situations, Shott's are ad hoc and incomplete. The social constructionists provide no overarching framework of situations to which one may refer for the prediction of emotions.

The positivist viewpoint is that for heuristic purposes a useful classificatory system of situations does exist. It stems from a multitudinous set of observations of actors in different situations judging each other's actions, or being rated by observers. The evidence for a delimited set of situational categories is truly impressive, and it is important to note that no contradictory evidence has been offered.

I am referring here to the literature (reviewed extensively in Kemper [1978*b*]) that identifies two dimensions of relationship in interaction settings. These have been variously labeled, as may be seen in table 1, but the underlying relational themes are coercive control of one actor by another, and positive social relations, or power and status, as they were described above.

TABLE 1

POWER AND STATUS FACTORS AND THEIR SOURCES

Power	Status	Source
Individual prominence and achievement	Sociability	Carter (1954)
Assertiveness	Likeability	Borgatta, Cottrell, and Mann (1958); Borgatta (1964)
Authoritarian control	Hostility-rejection	Zuckerman et al. (1958)
Control	Affection	Schutz (1958)
Autonomy vs. control	Love vs. hostility	Schaefer (1959)
Dominance-submission	Hostility-affection	Laforge (in Foa 1961)
Dominance	Friendliness	Burke and Bennis (1961)
Psychological autonomy vs. psychological control	Acceptance vs. rejection	Schaefer (1961)
Control	Affection	Lorr and McNair (1963)
Power	Leader-member relations	Fiedler (1964)
Interpersonal deprivation	Interpersonal seeking	Longabaugh (1966)
Unacceptable techniques	Acceptable techniques	Marwell and Schmitt (1967)
Equal vs. unequal	Friendly vs. competitive and hostile	Wish, Deutsch, and Kaplan (1976)
Power	Status	Myers (1977)

Although the power and status dimensions have emerged in recent years as a result of systematic observation of interaction, they are not new in the analysis of human action. The earliest known version of these dimensions is found in the work of the pre-Socratic Greek philosopher Empedocles, who postulated two dynamic forces in the universe, namely, love and strife (Cleve 1969). These can easily be assimilated to status and power, respectively. Freud ([1937] 1959, pp. 349–50), in fact, declared that Empedocles was his "great predecessor," for love and strife (status and power) are also akin to Freud's own *eros* and *thanatos*. The two-dimensional model of social relations has found support not only in studies of small groups and in the clinical work of Freud, but in cross-cultural work as well. Triandis (1972, p. 270) found that superordination-subordination (power) and intimacy (status) were the "fundamental dimensions of human social behavior that are obtained with different methods of human investigation." In a science with few generalizations that hold up across time and space, the two-dimensional model of social relations is a significant exception.

Because of the compelling nature of the model, many authors now employ it as an organizing framework for observation or analysis, including Winch (1958), Couch (1960), Brim et al. (1962), Straus (1964), Cancian (1964), Pepitone (1964), Mishler and Waxler (1968), Carson (1969), Boulding (1970), Triandis (1972), Swenson (1973), and Benjamin (1974), among others. Smith-Lovin (1979) has shown how the power and status dimensions are cognate with the basic dimensions of the semantic differential.

There can be no ultimate guarantee that the power-status model is correct. As with all models it can serve only heuristically, in this case to bring under one conceptual umbrella the full range of possible definitions, interpretations, and constructions of relational situations for the purpose of understanding emotions.

The power-status approach allows the analyst to predict, for example, that if an actor construes, defines, or interprets an interactional outcome as a gain of status for self (one is better liked, more appreciated, more rewarded, etc., in any of the vast number of culturally specific status currencies by which such improvements of status are payable), the actor will experience a certain emotion, usually happiness (Kemper 1978*b*). The actor will experience this emotion because happiness is *directly evoked*. No feeling rule for happiness is necessary according to the positivist approach, once the actor has construed the relational situation as status gain for himself.

Could it happen that the actor is not happy when status gain occurs? Yes, if the actor expected more status gain than was actually attained; or if the status gained was from a person low in his hierarchy of salience;

or if at the same time he gained status, he also learned that his child may be fatally ill; or if someone he was competing with gained even more status than he did, and so on.

What is important about these seeming exceptions to the generalization about status gain leading to happiness is that, while they modify the emotional outcome, they do so only by enlarging the relational canvas, including other actors, other relationships, the previous history of the relationship (in the form of expectations), and the like. But in all cases, the relational classificatory framework of power and status remains the same. Hence, social constructionists are no more devoted to the problem of defining and interpreting situations than are positivists. The difference is that, while the constructionists merely declare the importance of doing so, positivists offer tools for the task, which makes possible the prediction of particular emotions from particular social relational outcomes (Kemper 1978*b*).

CULTURE IN THE STUDY OF EMOTIONS

The argument developed thus far might seem to deny culture and norms a serious role in the production of emotions. Nothing could be further from the truth. The positivist position accepts that there are feeling rules, and that these rules vary by culture. But culture contributes to the understanding of emotions differently from the way social constructionists believe it does. Feeling rules are interesting only after the fact, so to speak, when there is a felt need to manage feelings in a more suitable direction. Culture, however, plays a crucial role in the generation of authentic (not managed) emotions in the first place. It does this in several ways.

First, culture—via norms, models, etc.—prescribes the power and status rights and deserts of actors in different relationships, and the tolerable limits of these rights. For example, it says that parents may spank their children, but a stranger may not; that an employer may fire a recalcitrant worker but not put him in chains; that spouses should honor each other until death; that pupils in a classroom should make an effort to answer questions when called on by teachers; and that partners to a business deal whose terms are specified by contract are not obligated to inquire about each other's families. These are all instances of the way in which cultural prescriptions specify the degree of status and power that actors in specific relationships owe, deserve, or are permitted vis-à-vis each other. Hochschild's (1979) "framing rules" are of this character. Violation of these cultural prescriptions in the power-status domain produces emotions, while conformity produces yet other emotions. If there is a feeling rule that specifies the emotion, it is there only after the fact, as Hochschild (1979,

p. 562) recognizes, that is, after the actual power-status relationship (or violation of its culturally specified conditions) has already evoked the authentic emotion. Individuals who fail to experience the emotions usually evoked in a given relationship may become aware of a feeling rule, but in fact they are sensing that the power-status features of the relationship are not following the culturally prescribed pattern for that relationship.

A second important contribution of culture to the production of emotions is that it specifies the power and status relational significance of *occasions*. The pageantry of a royal coronation or of the funeral of a national leader is directed toward evoking the important power and status implications of the throne or office just vacated in death, but which is again immediately filled. The very great effort to arouse emotions by music, colorful and rich attire, rhythmic patterns of marching by massed groups of participants (soldiers, children carrying flags, etc.) is directed toward enhancing the sense of power and status of the figure who is either beginning or ending his public career. A sense of reverence, awe, majesty is generated because these occasions are of great significance for the power and status system of the society, and because, as a consequence, the technology of emotional excitation is brought fully to bear (Collins 1975, p. 59).

A third important contribution of culture to emotions is that it specifies the power and status significance of concrete *behaviors* and *objects*. Thus, in Western societies, to dress in black is to signify loss, one's own changed status. To send flowers is to signify affection, the elevated status/regard one has for the other. To raise the third finger of one's hand to another is to signify disdain and contempt, a power act designed to indicate the low status in which one holds the other. In the famous example discussed by Hall (1959), Latin Americans approach very near to one to indicate friendship and interest, while North Americans require much greater distance for the acceptance of such status-enhancing sentiments from relative strangers. Cultural variability is greatest at the level of behaviors and objects. Yet underlying the variability is the fact that any particular behavior or object serves to indicate a culturally defined level of power and status in a relationship with others. It would be heuristically absurd to adopt the view that behaviors and objects do *not* have relational significance.

While the actors, occasions, behaviors, and objects are the stuff out of which situations are defined and constructed, the positivist approach to emotions understands all these in their power-status relational significance. Thus knowing the culture allows one to interpret the behavior of others, to define situations, and to predict emotions.

With these ideas in place, we may look now at Durkheim's discussion of mourning (cited by Hochschild 1979, p. 566) for its bearing on the

constructionist argument. Durkheim ([1915] 1965, p. 442) writes: "Mourning is not the spontaneous expression of individual emotions. If relations weep, lament, mutilate themselves, it is not because they feel themselves personally affected by the death of their kinsman. Of course it may be that in certain particular cases, the chagrin expressed is really felt."

Here Durkheim distinguishes between those who experience authentic emotions of grief and a larger portion who put on their grief by acting as if they were truly affected. The latter appear to be responding to a social norm for feeling (although, given Durkheim's description, we can see that it is a norm for enacting the behavioral accompaniments of grief rather than for feeling an emotion). Why do they do this? According to Durkheim ([1915] 1965, p. 443):

> It is a duty imposed by the group. One weeps, not simply because he is sad, but because he is forced to weep. It is a ritual attitude which he is forced to adopt out of respect for custom . . . independent of his affective state. Moreover, this obligation is sanctioned by mythical or social penalties. . . . Therefore, in order to square himself with usage, a man sometimes forces tears to flow by artificial means.

We see here that while sadness is enjoined, all that is actually required is the simulacrum of sadness. One's emotions, except perhaps fear of sanctions for nonperformance, may remain relatively untouched. Durkheim ([1915] 1965, p. 446) continues, however:

> An individual . . . if he feels strongly attached to the society of which he is a member, feels that he is morally held to be participating in its sorrows and joys; not to be interested in them would be equivalent to breaking the bonds uniting him to the group; it would be renouncing all desire for it and contradicting himself.

Here we have a somewhat different case, since the mourner now starts with an emotion of loyalty to society and a "desire" for attachment. What follows then is behavior that conforms to society's (i.e., *one's own*) wishes in the given instance, namely, to put on the mourning face, even though the loss is not personal. Though attenuated, it is nonetheless one's own loss, because it is society's loss. In this sense, in small societies, wholehearted commitment to one's group engenders a true emotion, though probably of lesser intensity than for immediate kin, whenever a member of that small group dies. Durkheim ([1915] 1965, p. 446) has a further point to make that bears on the argument:

> When the Christian, during the ceremonies commemorating the Passion, and the Jew, on the anniversary of the fall of Jerusalem, fast and mortify themselves, it is not in giving way to a sadness which they feel spontaneously. Under these circumstances, the internal state of the believer is out of all proportion to the severe abstinences to which they submit them-

> selves. If he is sad, it is primarily because he consents to being sad, and he consents to do it in order to affirm his faith. The attitude of the Australian during mourning is to be explained in the same way. If he weeps and groans, it is not merely to express an individual chagrin; it is to fulfill a duty of which the surrounding society does not fail to remind him.

Taken at face value (see Hochschild 1979, p. 566) this seems to affirm the normative determination of the emotions of the believers and mourners. Indeed, without norms to prescribe the behavior, no formalized mourning behavior would take place, no ritual would be enacted. However, ritual, arouses emotions not present before its enactment. Whatever spark of ing behavior would take place, no ritual would be enacted. However, ritual and the emotion aroused may not be the putative desideratum of the occasion (e.g., sorrow at a funeral or recollection of a tragic historical occurrence). In fact, the net result of enacting mourning rituals is to make the mourners feel good:

> The foundation of mourning is the impression of a loss which the group feels . . . which results in bringing individuals together. . . . Of course they have only sad emotions in common, but communicating in sorrow is still communicating, and every communion in mind, in whatever form it may be made, raises the social vitality. . . . This surplus energy effaces the more completely the effects of the loss. . . . Presently one stops mourning, and he does so because of the mourning itself. [Durkheim (1915) 1965, pp. 447–48]

Thus, whether or not one is a primary mourner with authentic emotions of sadness, participation in the mourning rites and ceremonies energizes the group of which one is a member and thereby gives comfort to the true mourners while arousing the authentic emotions of communion and solidarity among all, even those with a lesser sorrow.

Perhaps society is too smart to expect that injunctions to feel a given emotion will be, or can be, followed. It is just smart enough, however, to recognize that if the social organizational and social relational conditions are met, the desired emotions will flow of themselves, authentically. And it is these conditions that are framed as rules.

CONCLUSION

Social constructionists who study emotions have sought to explain emotions by social rules for feeling. In this they have been too ambitious, since genuine feeling results from "real, anticipated, imagined, or recollected outcomes of social relationships" (Kemper 1978*a*, p. 32, cited in Shott 1979, p. 1318). Additionally, the empirical data do not support the constructionist theory that radically detaches emotions from physiological underpinnings. On the other hand, social constructionists have correctly

identified the requirement that actors identify the relationship and its outcome before an emotion will be felt. But here they have not been ambitious enough, for they have failed to explain how actors do that.

The positivist approach to emotions accepts an important link between social relational conditions that evoke emotions and the types of physiological response that produce specific emotional reactions. Though the data are not conclusive on this point, there is an open door here for ideas and research to pass in both directions, ultimately to build a *sociophysiology*. The positivist also accepts what Hochschild (1979, p. 553) calls the "organismic approach" to emotions, in which emotions are "automatically triggered" (p. 554) once the social stimulus, properly defined by the actor, is in place. This approach is primary in the study of emotions, since it explains the authentic feelings of individuals regardless of whether they square with social conventions, with what other people are feeling, or with what one felt on the last similar occasion. Positivists understand the interest in social rules for feelings but see the rules as epiphenomenal. Even social constructionists understand that the rules do not directly produce emotions; what does produce them is to create, actually or imaginatively, the social organizational or social relational conditions that directly evoke feelings. Finally, positivists accept the need to define and interpret situations before the emotion can be experienced. With the power-status model, they offer an empirically derived formulation of social relational situations that are intimately linked with emotions.

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Parental and Peer Influences on Adolescents' Educational Plans: Some Further Evidence[1]

Mark Davies and Denise B. Kandel
Columbia University and New York State Psychiatric Institute

The specific influence of parents and peers on adolescents' educational aspirations is examined in a relational sample of matched adolescent–parent–best friend triads ($N = 762$). A nonrecursive model of peer influence is also estimated. Most previous estimates of interpersonal influences are based on the adolescent's perceptions of the significant other's attitudes or behaviors rather than on the significant other's self-reports of these attributes. Estimates of interpersonal influences based upon self-reported attributes are presented. It is shown that perceptual measures inflate estimates of interpersonal influence. Independent data from parents document that perceptual measures of significant others' attributes reflect not only attributes of the person being perceived but also attributes of the perceiver. Parental influence on the adolescent's aspirations is stronger than peer influence, and this influence does not decline over the adolescent years. Peers are involved in a process of reciprocal influence, and peer influences are stronger among girls than among boys.

Interpersonal influences are crucial intervening links in causal models of educational and occupational attainment. The influential model developed by Duncan and Sewell and their associates (Duncan, Featherman, and Duncan 1972; Sewell, Haller, and Ohlendorf 1970; Sewell, Haller, and Portes 1969; Sewell and Hauser 1975), and further tested and replicated by other investigators, describes a four-step social-psychological process. In the model, interpersonal factors in adolescence mediate the effects of socioeconomic background and ability on levels of aspirations and on subsequent educational and occupational attainment in early adulthood. The importance of interpersonal factors for aspirations is underscored in a recent review by Spenner and Featherman: "The encouragement of one's parents and the plans of one's peers appear to shape ambitions more directly and with greater impact than any other source. Their effects are stronger than the direct influence of one's scholastic aptitude or previous

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academic achievement, and much stronger than any direct influence from one's social origins per se" (1978, p. 392).

The Wisconsin model of attainment was first developed on a cohort of farm boys (Sewell et al. 1969) and of male seniors from a statewide Wisconsin sample first contacted in 1957 and recontacted in 1964–65 (Sewell et al. 1970). The model has now been extensively estimated and replicated on different sets of data; the effects of factors have been estimated for various subgroups in the population (see, e.g., Alexander, Eckland, and Griffin 1975; Alwin and Otto 1977; Duncan, Haller, and Portes 1968, 1971; Hauser 1969, 1971; Hout and Morgan 1975; Otto 1978; Otto and Haller 1979; Picou and Carter 1976; Porter 1976; Pugh 1976; Williams 1972; Wilson and Portes 1975). With rare exceptions, most analyses have focused on senior male students. Separate estimates for boys and girls have been presented by Alexander and Eckland (1974), Williams (1972), and Hout and Morgan (1975); and for black and white adolescents by DeBord, Griffin, and Clark (1977), Kerckhoff and Campbell (1977), and Portes and Wilson (1976).

The process of educational attainment is an area of sociological inquiry that is characterized by an unusual degree of methodological sophistication, replication, and cumulation of efforts. The overall sophistication of the analyses contrasts with the oversimplification in the conceptualization, measurement, and interpretation of the interpersonal variables. Researchers are well aware of these shortcomings (Duncan, Featherman, and Duncan 1972, p. 13). The treatment of the interpersonal variables suffers from two limitations. In most models, especially those tested on the Wisconsin data, social influences are measured by a composite index that groups together the indicators of parental, peer, and teachers' influences as "significant others' influence" (SOI) (Sewell et al. 1969; Woelfel and Haller 1971). Although versions of the model with the social influences disaggregated were presented subsequently (e.g., Sewell and Hauser 1972), the initial model, in which the influences are grouped, seems to have been the most influential in further replications by other investigators (Porter 1976; Spencer 1976; Wilson and Portes 1975; Yuchtman and Samuel 1975). Even when interpersonal influences are disaggregated, with certain exceptions (Duncan et al. 1972; Hauser 1972; Otto 1978; Sewell and Hauser 1972; and Williams 1972) investigators discuss the mediating effect of these variables as a group and not of each type of influence separately (see, e.g., Hauser, Sewell, and Alwin 1976; Otto and Haller 1979; Otto and Alwin 1977; Alexander et al. 1975). Such a differentiation is important for understanding the social psychology of adolescent development and the formation of aspirations. Because the socialization processes relevant to status attainment take place in adolescence when educational and occuptional plans are formed, theoretical considerations require that the

influences of parents and peers be examined and discussed separately. Indeed, much of the treatment of adolescent development involves discussions of the relative importance of inter- and intragenerational influences for socialization and of the adolescent's potentially conflicting involvements with parents and with peers (Bengston 1970; Coleman 1961; Kandel and Lesser 1972).

In addition to grouping various sources of influences, another limitation of most previous estimates of interpersonal influences is that they are often based on the adolescent's perceptions of the significant others' attitudes or behaviors rather than on the significant others' self-reports of these attributes. As noted by Duncan et al. (1972), the use of perceptual variables leads to a situation in which the independent variable "may well be contaminated to some degree by the dependent variable which it supposedly helps to explain" (p. 137; see also Haller and Portes 1973; Hauser 1969, 1971; Williams 1972). That this contamination indeed takes place will be established in the present article. To the extent that earlier studies have measured interpersonal influences on the basis of self-reports rather than perceptions, they contain only one such independent measure, either for parents or for peers but not for both in the same study, making it difficult to arrive at proper and comparable estimates of each source of influence. Kerckhoff and Huff (1974) measured parents' reports of their educational aspirations for their children but did not consider peer factors. Peer influence from self-reports of one or more friends has been analyzed in several studies which measured parental influences on the basis of adolescents' perceptions (Campbell and Alexander 1965; Duncan 1970; Duncan et al. 1968; Hout and Morgan 1975; Otto 1977, 1978; Otto and Alwin 1977; Otto and Haller 1979). In earlier research (Kandel and Lesser 1969, 1972), we obtained independent reports from parents and peers. Mothers' aspirations for their children were more highly correlated with adolescents' educational aspirations than were best friends' own aspirations. However, no causal model was tested in that earlier study, and no estimates of the influences of parents and peers were presented.

Two different models of interpersonal influences have been estimated and presented in the literature. The most common is the recursive model, discussed above, with significant others' influence entered as endogenous variables to mediate the effects of background factors on aspirations. A more complex, nonrecursive model developed by Duncan (Duncan et al. 1968, 1971; Duncan 1970; Duncan et al. 1972) allows for the reciprocal influence of friends upon one another and is probably a closer representation of the actual processes of peer influences than the simple recursive model (see also Hout and Morgan 1975). In the models, two different mechanisms of socialization are assumed and represented in the measures of interpersonal influences included for parents and teachers, on the one

hand, and peers, on the other. The adult measures, which include parents' levels of aspirations for their children or their encouragement toward attending college, index the socializer's prescription or expectation of appropriate behaviors for the adolescent, that is, the influence of significant others as "definers" (Woelfel and Haller 1971). The peer measures, on the other hand, which consist of the friend's level of aspiration for himself or the proportion of friends going on to college, index the influence of significant others as exemplars or role models (Kandel, Kessler, and Margulies 1978*a*, 1978*b*; Kandel and Lesser 1972; Otto and Haller 1979; Picou and Carter 1976; Woelfel and Haller 1971). In most models, family socioeconomic status is measured by parental educational or occupational attainment, which could be considered a measure of parents as role models for their children. However, the variables are always treated as exogenous and never included in the set of social-psychological variables assumed to measure processes of interpersonal influence.

This paper makes use of relational data from a sample of triads of adolescent–parent–best schoolfriend in order to investigate the interpersonal influences of parents and peers on adolescents' educational aspirations. Specifically, we address the following issues: (1) What are the relative influences of parents and peers on levels of educational aspiration? (2) What distortions are introduced into models of status attainment when measures of parental influences are based on perceptions rather than on self-reports, and what is the source of these distortions? (3) What are the results of a replication of the nonrecursive model of peer influences developed by Duncan et al. (1968, 1971) when the parametric estimates of parental influences are based on self-reports rather than on perceptions? (4) What variations occur in observed patterns of interpersonal influences of parents and peers among adolescents of different sexes and different ages?

MODELS ESTIMATED

Two general models are estimated. One is a modified version of that part of the general recursive Wisconsin model of the status attainment process that deals with the formation of educational aspirations. Four variables are specified as predictors of adolescent aspirations (see fig. 1). A latent measure of family socioeconomic status, based on father's and mother's education and father's occupation, is entered as an exogenous variable and is assumed to determine the adolescent's educational aspirations and school performance as well as the nature of the interpersonal influences impinging on him. The latter variables, which include levels of parents' aspirations for their child as well as peers' aspirations for themselves (as measured by the best schoolfriend), predict adolescents' aspirations. Al-

though adolescents' friendship networks presumably include more than one friend, reliance on a single friend to measure peer influence appears to underestimate only slightly the total effect of peer influence. Indeed, the zero-order correlation between adolescents' and best friends' aspirations has been reported at .45 when based on five friends (Otto 1978) compared with .37 for a single friend (Duncan et al. 1968), these estimates presumably based on the same data set.

Although we have aimed to replicate the formal structure of the Wisconsin model, we have been only partially successful. There are important differences between the two models. In the present analysis, family socioeconomic status is measured by a latent variable rather than by a factor-based index. A measure of adolescent mental ability, such as IQ, was not available and was not included in the model. Academic performance is measured by the adolescent's self-reported grades and not taken from school records. Finally, important differences appear also in the measures of interpersonal influence. In most studies, the measure of parental influence consists of adolescents' perception of their parents' encouragement toward college attendance (cf. Sewell et al. 1969; Wilson and Portes 1975; Otto and Alwin 1977). In the present analysis, we rely on the parents' specific aspirations for their children as reported by the parents and as perceived by the child. The peer measure is based on the best friend's self-report of his own aspirations, a single best friend selected by each focal adolescent. In previous studies, the measure of peer influence was based on perceptions of friends' college plans (e.g., Sewell et al. 1969; Hauser 1972; Alexander et al. 1975) and rarely on the friends' self-reports of their aspirations. Exceptions include Hout and Morgan (1975), and Otto and Alwin (1977). In the present analyses, not only is the assessment of influence based on the significant others' self-reports of their attitudes rather than on perceptions, but identical measures are used for both significant others and the adolescent. This is of particular advantage in assessing the relative impact of different sources of interpersonal influence. Although the original Wisconsin model did not posit any path among the interpersonal parental and peer variables, the model we have estimated allows for correlated error between parental and peer aspirations. Finally, no measure of teacher's influence is included in the present model.

The second model we have estimated is based on Duncan et al.'s (1968, 1971) nonrecursive model of peer influence and assumes reciprocal paths between the two friends' aspirations. In Duncan et al.'s (1968, 1971) original model, all other variables were endogenous with each aspiration assumed to be determined by parental aspirations, socioeconomic background, and school performance. The present analyses incorporate the slightly more complex causal model tested in the recursive model.

All results are presented for males and females separately.

METHOD

Sample

The analyses are based on a convenience relational sample ($N = 762$) of matched adolescent–parent–best schoolfriend triads. The data were obtained from five schools included in the second wave of a larger longitudinal study carried out on a multistage random sample of 18 public secondary schools in New York state in fall 1971 and spring 1972 (see Kandel, Single, and Kessler 1976). Students in the five schools were different from their counterparts in the remaining 13 schools: they were more likely to be male, aspired to more education, and had parents with more education. Although the mean levels of the variables did differ, the correlations between the variables were similar (data not presented).

Structured self-administered questionnaires were given to the entire student body in the five schools, making it possible to collect data from the student's best schoolfriend. Since in 79% of the cases the best schoolfriend is the best friend overall, the data provide good estimates of the role of friends in general. Two to three weeks after each school was surveyed, questionnaires were mailed to one of the student's parents (alternately mothers or fathers). Because the question on educational aspirations was included only in the second wave, the data for this paper are drawn from triads from the wave 2 sample. Adolescents were asked to report their parents' educational aspirations for them but not their perceptions of their best friends' plans.

Of the 4,343 adolescents eligible to be matched to both their best friend and one parent, 772 (18%) were successfully matched to both. Those included in triads compared with those not included were more likely to be female (59% vs. 48%), to have parents with more than a high school education (53% vs. 44% for fathers, 43% vs. 33% for mothers), to have an A or B grade point average (77% vs. 55%), and to aspire to at least four years of college (70% vs. 57%). The differences are statistically significant and clearly indicate that the triad sample is not a random sample of those at risk to be included. Comparison of the correlation matrices (not shown) for respondents in the triad sample versus those not included suggests a potential bias in parameter estimates. Six variables were included in the matrices: father's education, mother's education, father's occupation, adolescent's grade point average, adolescent's perception of parents' aspirations, and adolescent's educational aspirations. The average absolute value of the differences between the pairs of 10 correlations is .09 for males and .06 for females, with the correlations generally higher in the triad sample. This selection bias will tend to inflate estimates of parameters relating these variables.

Measurement of Variables

Adolescent and best friend variables were measured identically.

Socioeconomic background is a latent variable measured by three manifest indicators: father's education, mother's education, and father's occupation. Parent education was coded in terms of highest number of years of schooling completed, ranging from 8 to 18 years. Categories were assigned values corresponding to the midpoints of the interval they covered. Business and technical school was given a value of two years plus high school. We assumed an average of two years of graduate school for those indicating attendance at graduate or professional schools. Father's occupation was assigned a score on the Duncan SEI scale (Reiss et al. 1961). Father's education is the most reliable of the manifest indicators of the latent variable (see fig. 1).

Academic performance is the self-reported grade point average for the previous semester.

Parental educational aspirations for adolescent are coded as the highest number of years of schooling aspired to, ranging from 11 to 18 years. Levels included less than high school; high school graduation; some college or technical school; college graduation; and graduate school.

Perceived parental aspirations are the adolescent's perception, coded as highest number of years of schooling parents were seen to desire for the adolescent.

Adolescent and best friend aspirations are the same categories as parental aspirations. Correlations, means, and standard deviations of variables in all models that were estimated are displayed in the Appendix.

Analytical Strategy

Parametric estimates were obtained using maximum-likelihood techniques (Jöreskog and Sörbom 1978). Initially, estimates for males and females were obtained separately. Subsequent models were estimated with equality constraints between the sexes. Equality of the parameters was assumed in the following sequence: measurement model for socioeconomic background, structural coefficients between socioeconomic background and the endogenous variables, between grade point average and the remaining endogenous variables, and, finally, equality of all parameters. A test of the hypothesis that the sexes do not significantly differ on a particular set of parameters is available upon inspection of the difference in χ^2s between sequential models. A large increase in the χ^2, with respect to the gain in degrees of freedom, for an equality constrained model, induces the conclusion that the particular set of parameters constrained to be equal are sex specific. For

unstandardized estimates reported, a single estimate indicates equality between the sexes.

In order to control for possible school effects, since the sample was drawn from five different schools, all analyses were carried out on covariance matrices based on the weighted average intraschool correlation coefficients, with the exception of the age-specific analyses. Estimates of the within-school and between-school components of variance for adolescent aspirations (2.72 and 0.56, respectively) yield an intraclass correlation of 0.17, approximately equal to results in previous studies (Hauser 1971). Between-school variability, though present, is small relative to within-school variability for adolescent aspirations.

When comparing coefficients across groups and across models, we will cite unstandardized coefficients, with the advantage that the metric for number of years of schooling has a readily understood substantive meaning. When comparing the relative importance of different variables within a particular model, we will discuss the standardized coefficients for all models, which are displayed in table 1.

RESULTS

Relative Parental and Peer Influences on Adolescent's Aspirations

Results from the basic social-psychological model of interpersonal influences on adolescents' educational aspirations are displayed in figure 1. Figure 1*a* is based on the adolescents' perceptions of their parents' aspirations for their children, and figure 1*b* on the parents' self-reported aspirations.

Several trends are readily apparent:

1. Parents' influences on their adolescent children's aspirations are much stronger than the influence of best friends. Depending upon which measure of parental aspirations is used, the ratio of parental to peer effects varies by a factor of two or eight.

2. The perceptual measure of parental aspirations introduces a number of important differences in the results compared with self-reports: (*a*) the perceptual measure increases the amount of variance explained in adolescents' aspirations by almost 40%, from 41% to 58% for boys, or 37% to 50% for girls; and (*b*) the perceptual measure inflates the size of parental effects by about 50% for both sexes and reduces the size of peer effects for girls. Thus, perceptual measures inflate the importance of parents relative to that of peers. As will be documented below, the increased total explained variance as well as the increased size of the parental effect result from the fact that adolescents' perceptions of parental aspirations partly reflect the adolescents' own aspirations for themselves.

TABLE 1

STANDARDIZED COEFFICIENTS FOR PSYCHOSOCIAL MODELS OF INTERPERSONAL INFLUENCES ON ADOLESCENTS' EDUCATIONAL ASPIRATIONS PRESENTED IN FIGURES 1 THROUGH 3, AND FOR RECIPROCAL PEER INFLUENCE MODEL IN FIGURE 4

	FIGURES									
	1*a*		1*b*		2		3		4	
PARAMETERS	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
Measurement model:										
SES to father education*	.86	.86	.85	.85	.85	.85	.84	.84	.90	.90
SES to mother education*	.58	.58	.59	.59	.59	.59	.60	.60	.61	.61
SES to father occupation*	.62	.62	.62	.62	.62	.62	.63	.63	.61	.61
Parental aspiration to self-reported parental aspiration	...	...	...	...	...	...	.78	.77	.72	.73
Parental aspiration to perceived parental aspiration	...	...	...	...	...	...	.21	.39	.41	.41
Adolescent aspiration to perceived parental aspiration	...	...	...	...	...	...	.58	.45	.38	.40
Adolescent aspiration to self-reported adolescent aspiration	...	...	...	...	...	...	.95	.97	.95	.96
Structural model:										
SES to GPA	.18	.19	.19	.19	.19	.19	.19	.19	.23	.23
SES to self-reported friend aspiration	.12	.10	.13	.11	.14	.11	.14	.11	...	...
SES to perceived parental aspiration	.25	.23	...	...	...	...	...	...	...	...
SES to self-reported parental aspiration	...	...	.37	.34	.37	.34	...	...	...	...
SES to *parental aspiration*	...	...	...	...	...	...	.48	.45	.50	.45
SES to self-reported adolescent aspiration	.06	.05	.07	.06	.02	.01	...	...	...	...
SES to *adolescent aspiration*	...	...	...	...	...	...	−.09	−.07	−.20	−.17
GPA to perceived parental aspiration	.38	.33	...	...	...	...	...	...	...	...
GPA to self-reported parental aspiration	...	...	.33	.29	.33	.29	...	...	...	...
GPA to *parental aspiration*	...	...	...	...	...	...	.42	.37	.39	.34
GPA to self-reported friend aspiration	.25	.20	.25	.20	.25	.20	.25	.20	...	...
GPA to self-reported adolescent aspiration	.26	.21	.36	.29	.24	.20	...	...	...	...
GPA to *adolescent aspiration*	...	...	...	...	...	...	.24	.19	.08	.07
Perceived parental aspiration to self-reported adolescent aspiration	.58	.53	...	...	.53	.50	...	...	...	...
Self-reported parental aspiration to self-reported adolescent aspiration	...	...	.35	.31	.16	.14	...	...	...	...

NOTE.—Variables listed in italics are multiple indicator latent variables ($N = 762$).

* Constrained to be equal across sex.

TABLE 1 (*Continued*)

Parameters	Figures									
	1*a*		1*b*		2		3		4	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
Self-reported parental aspiration to perceived parental aspiration	...	...	...	...	.51	.52	...	...	...	...
Parental aspiration to *adolescent aspiration*	...	...	...	...	...	...	.65	.55	.86	.67
Self-reported friend aspiration to self-reported adolescent aspiration	.08	.13	.05	.20	.07	.12	...	...	...	...
Self-reported friend aspiration to *adolescent aspiration*	...	...	...	...	...	...	.02	.16	...	...
Friend aspiration to *adolescent aspiration*	...	...	...	...	...	...	...	...	.09	.36
Adolescent aspiration to *friend aspiration*	...	...	...	...	...	...	...	...	.17	.40
Error variances and covariances:										
Measurement model:										
Father education*	.51	.51	.53	.53	.53	.53	.54	.54	.44	.44
Mother education*	.81	.81	.81	.81	.81	.81	.80	.80	.79	.79
Father occupation*	.78	.78	.78	.78	.78	.78	.78	.78	.79	.79
Self-reported parental aspiration	...	...	...	...	...	...	.62	.64	.69	.69
Perceived parental aspiration	...	...	...	...	...	...	.66	.65	.67	.67
Self-reported adolescent aspiration	...	...	...	...	...	...	.31	.26	.31	.26
Structural model:										
GPA	.98	.98	.98	.98	.98	.98	.98	.98	.97	.97
Perceived parental aspiration	.87	.90	...	...	.86	.85	...	...	...	...
Self-reported parental aspiration	...	...	.84	.87	.84	.87	...	...	...	...
Parental aspiration	...	...	...	...	...	...	.72	.77	.71	.78
Self-reported friend aspiration	.95	.97	.95	.97	.95	.97	.95	.97	...	...
Friend aspiration	...	...	...	...	...	...	...	...	.59	.72
Self-reported adolescent aspiration	.64	.71	.77	.80	.65	.71	...	...	...	...
Adolescent aspiration	...	...	...	...	...	...	.65	.72	.59	.72
Perceived parental aspiration to self-reported friend aspiration	.00	.23	...	...	...	...	...	...	...	...
Self-reported parental aspiration to self-reported friend aspiration	...	...	.09	.12	.09	.12	...	...	...	...
Parental aspiration to self-reported friend aspiration	...	...	...	...	...	...	.10	.20	−.28	−.20
Adolescent aspiration to *friend aspiration*	...	...	...	...	...	...	...	...	.14	−.52

Note.—Variables listed in italics are multiple indicator latent variables (N = 762).

* Constrained to be equal across sex.

3. The direct effects of parental aspirations are stronger than the effect of any other variable in the model, with one exception. The standardized effects of self-reported parental aspirations are approximately equal to the standardized effects of academic performance (see table 1).

4. Opposite results obtain regarding the relative effects of interpersonal influences on boys and on girls. Whereas parental aspirations are strong, significant, and equal in importance for both sexes, peer effects, though weaker than parental effects, are much stronger for girls than for boys.

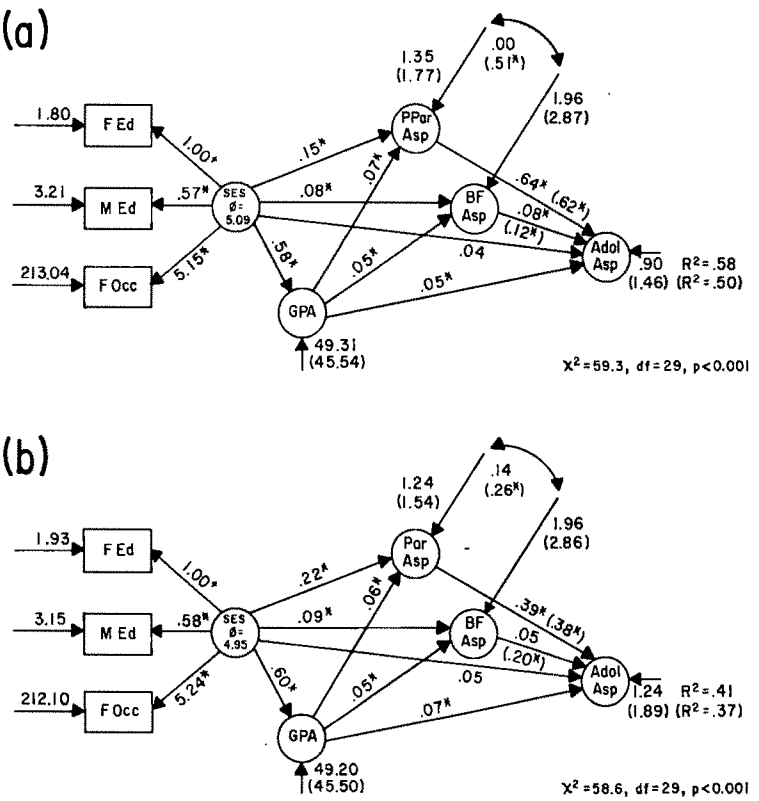


FIG. 1.—Basic psychosocial model of interpersonal influences on adolescent's educational aspirations with single parental measure ($N = 762$). *a*, Parental influence measured by adolescents' perceptions of parents' aspirations for the adolescent. *b*, Parental influence measured by parents' self-report of aspirations for their child. Unstandardized coefficients for males and females in parentheses. Single coefficients are presented for parameters constrained to be equal across sex. Variables: F Ed = father's education; M Ed = mother's education; F Occ = father's occupation; SES = socioeconomic status; GPA = grade point average; BF Asp = friend's educational aspiration; Par Asp = self-reported parental aspiration; P Par Asp = perceived parental aspiration; Adol Asp = adolescent's educational aspiration. † = fixed parameter; * = greater than twice standard error.

Friends appear to be relatively unimportant in determining the aspirations of boys but are quite influential in determining the aspirations of girls. In fact, peer effects are insignificant for boys when parental self-reports rather than adolescent perceptions of parental aspirations are used in the causal model. Academic performance affects adolescents' educational aspirations to the same degree for each sex: 0.7 additional years aspired for each 10-point increase in grade average, although the standardized effect for males ($\beta = .36$) is slightly larger than the standardized effect for females ($\beta = .29$) (see Marini and Greenberger 1978). This sex differential results from educational aspirations being less variable among males than females.

5. Socioeconomic status is the strongest determinant of parents' aspirations for their children, stronger than the adolescent's school performance.

6. Socioeconomic background has no statistically significant direct effect on aspirations. To the extent that it has any effects, they are indirect. The most important effect is through parents' aspirations for their children, those in turn determining the adolescents' aspirations for themselves. Social class has additional substantial indirect effects through the adolescents' school performance and level of the best schoolfriend's aspirations. The latter effect reflects the fact that friendships develop among youths who share certain attributes (Kandel 1978*b*; Otto 1977; Rhodes, Reiss, and Duncan 1965). Furthermore, the social class effects on best friends' aspirations may reflect not only homophily among friends on the basis of social class background (Otto 1977) but also the role played by parents, in controlling their children's friendships.

The original Wisconsin model did not posit any path among the interpersonal variables. The correlated error between parents' and peers' aspirations in the present models may be interpreted as a parental effect—however, not as a direct effect of parents' aspirations on the aspirations of the adolescent's friend, but as an indirect effect on the kinds of friends their children select. Hence, the correlated error was estimated and not the structural path from parental aspirations to best friends' aspirations. The correlated error is generally higher for girls than for boys (for whom the parameter in fig. 1*a* is zero), suggesting that parents may have more control over the kinds of friends their daughters select than over their sons' choices. The inclusion of these parameters provides a better fit of the overall model than one without them, as indicated by a reduction in the χ^2 value from 82.2 to 59.3 in figure 1*a*, significant at $P < .001$ for 2 degrees of freedom.

The estimates of peer influence presented in the models include an upward bias, since similarity between friends at any one time results from two factors: selection effects due to similarity in educational aspirations prior to the formation of the friendship, and socialization effects that

represent true peer influence or increased similarity as the result of continued interaction among friends (Kandel 1978*a*; Cohen 1977). Similar reservations have been stressed by Duncan et al. (1968), although not demonstrated empirically. Analyses we reported elsewhere based on a longitudinal sample of friendship dyads suggest that selection and socialization effects on a variety of attributes are approximately equal in importance (Kandel 1978*a*). The estimates of peer influence based on cross-sectional data presented in a model such as that displayed in figure 1 may overestimate the effects of peers by as much as 100%.

7. Despite the large amount of variance explained by the models, a comparison of the matrix of covariances regenerated from the estimates in each solution and the observed matrix shows statistically significant residual variation.[2]

The evidence suggests that parents are considerably more potent than peers at influencing their children's educational planning. As noted earlier, information from a single friend appears to provide but a slight underestimate of the influence of a larger group of peers.

Joint Effects of Perceived and Self-reported Parental Aspirations

The availability of perceived and self-reported parental aspirations allows us to investigate the interrelationships between the two measures and the role they play in determining adolescents' aspirations. We can establish whether or not parental attitudes have an influence on their children only to the extent that these attitudes are correctly perceived by the child. Such a test has not previously been reported in the literature, although Kerckhoff and Huff (1974) discussed in detail the independent contribution of self-reported and perceived parental aspirations to adolescents' aspirations. In addition, we can also analyze the nature of perceptions and the extent to which they depend on the attributes of the person being perceived or on those of the perceiver.

To assess the joint effects of perceptions and self-reports, a model was estimated in which both types of data were included simultaneously. In addition to their direct effects on adolescents' aspirations, parental self-reports were assumed to have an indirect effect through the children's perceptions of parental attitudes. The results displayed in figure 2 indicate that both direct and indirect effects of parental self-reported aspirations are present and that both are significant. Perceptions, however, have much larger direct effects than self-reports: standardized β's for percep-

[2] For each sex in the model presented in fig. 1*b*, the highest of the residual correlations is 0.12, with the mean of the absolute residual correlations equal to 0.04. Whether this residual variation is due to misspecification, nonlinearity, or other causes is unknown. The sample size is large enough to yield a significant χ^2 when small residual variation exists. No systematic pattern existed in the residual correlations.

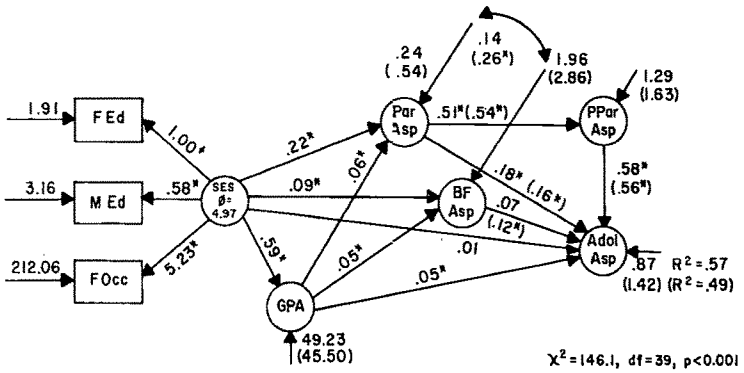


FIG. 2.—Psychosocial model of interpersonal influences on adolescent's educational aspirations with parents' self-reported and perceived aspirations for adolescent (N = 762). Unstandardized coefficients for males and females in parentheses. Single coefficients are presented for parameters constrained to be equal across sex. See fig. 1 for explanation of abbreviations of variables.

tions are .53 and .50 among males and females, respectively, compared with .16 and .14 for self-reports, with the former three times as large as the latter.

These findings are important, for they document that the interpersonal influences of parents are exerted in subtle ways and not necessarily with the awareness of the adolescents being influenced. Parental aspirations have substantial direct effects beyond those mediated by the adolescent's perceptions of them.

However, the effects attributed to perceived parental aspirations are themselves overestimated.

Perceptual Distortions of Parental Aspirations

Indeed, we document that perceptions reflect not only the attributes of the person being perceived but also the attributes of the perceiver, those very same attributes that are seemingly being predicted by the perceptions.

Psychologists have noted the tendency for individuals to distort their views of others to make them consistent with their own beliefs. These psychological theories include Sherif's notion of assimilation/contrast (Sherif and Sherif 1948), Festinger's cognitive dissonance theory (1957), Heider's balance theory (1958), and more recent developments in attribution theory (Harvey, Ickes, and Kidd 1976). The extent to which adolescents' perceptions of parental educational aspirations may represent a projection of their own aspirations has not previously been estimated with independent data from parents, although Hauser (1969, 1971) and Kerckhoff and Huff (1974) have argued that adolescents' perceptions of parental

educational aspirations may represent a projection of their own aspirations. Indeed, with no independent data from parents, Hauser (1971, pp. 124–25) reports that the effect of adolescents' aspirations on perceptions of father's aspirations is twice as large as the effect of a latent variable for parental encouragement on adolescent perceptions. In a sample of parent-adolescent dyads, Kerckhoff and Huff (1974) found a residual covariance between perceptions of parental aspirations and adolescents' aspirations after controlling for background variables and actual parental reports of their aspirations. However, the magnitude of these effects was not reported.

A simple test of the determinants of perceptions would be a nonrecursive model that would include direct paths from parental aspirations to adolescents' perceptions and aspirations, and reciprocal paths between adolescents' perceptions and aspirations. Attempts to test such a model presented difficulties arising out of the weak identification of the reciprocal paths. Locating an instrument for adolescent's perceptions of educational aspirations, namely, a variable with direct effects on adolescent's perceptions and only indirect effects on adolescent's aspirations, was an empirically, if not theoretically, impossible task. Correlation between the structural regression residuals for the two variables was usually greater than —0.85, making interpretation difficult. These high negative correlated errors may be a consistent feature of certain nonrecursive models (see Gillespie and Fox 1980). Such a model could be estimated only by setting the direct path from parental aspirations to adolescent aspirations to zero.[3] Therefore a different type of model was tested, similar to one proposed earlier by Hauser (1971). Two latent variables were posited, parental aspirations for the adolescent and adolescents' educational aspirations for themselves, both affecting in part the adolescent's perceptions of parental aspirations. A regression of perceived parental aspirations on parental reported aspirations and on adolescent's own aspirations was embedded in a measurement model (see fig. 3). The paths to the variable, perceived parental aspirations, indicate the relative magnitude of the sources of adolescents' perceptions of their parents' aspirations. Since both latent variables are measured in the same metric, the size of the loadings to the manifest indicator can be compared.

As illustrated by the results in figure 3, adolescents' aspirations for themselves determine in part their perceptions of parental aspirations. The

[3] Since the models reported in fig. 1 clearly indicate that such direct paths are positive, a series of successive nonrecursive models were run. In these models, similar to fig. 2 but with reciprocal paths between perceptions and adolescents' aspirations, we fixed the path from parental aspirations to adolescents' aspirations successively at various points in the range from 0.00 to 0.60 and estimated the reciprocal paths. In this series, the effects of adolescents' aspirations on perceptions of parental aspirations were larger than the reverse paths from perceptions to aspirations.

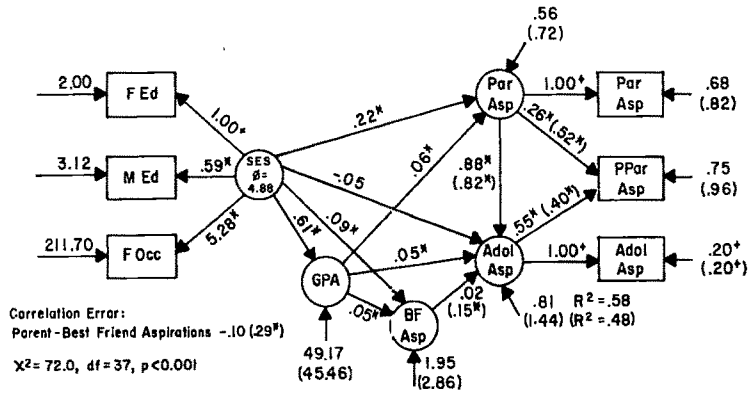


FIG. 3.—Psychosocial model of interpersonal influences on adolescent's educational aspirations with perceived parental aspirations included as indicator of parental and adolescent aspirations ($N = 762$). Unstandardized coefficients for males and females in parentheses. Single coefficients are presented for parameters constrained to be equal across sex. See fig. 1 for explanation of abbreviations of variables.

effects of adolescents' aspirations on perceived parental aspirations are not statistically different either between sexes or within each sex. The effect of parental aspirations on adolescents' aspirations includes direct and indirect effects and should be considered the total effect the parent has on the adolescent.[4]

The strong effect between adolescents' aspirations and perceptions of parental aspirations illustrates that perceptions are not simply the reflections of the other's attributes but a reconstruction of that other in line with the perceiver's own characteristics. The perceiver's own characteristics are as important a factor in this construction of reality as the external object's attributes.

Reciprocal Peer Influence

It was suggested earlier that the estimates of peer influence in the basic recursive model may not be an accurate representation of the socialization influence of friends because the model does not allow for a reciprocal process in which friends influence each other. A final model (see fig. 4) was estimated—a modified version of Duncan's nonrecursive model of peer influences on educational aspirations—in which reciprocal paths between friends' aspirations are assumed to be present (Duncan et al. 1968,

[4] However, parents' aspirations for their children may themselves be the result of a reciprocal process, with parents revising their aspirations for the child in line with the child's stated aspirations. To the extent that this occurs, estimates of the effect between adolescents' aspirations and their perceptions of their parents' aspirations will be increased. For technical reasons, it was not possible to embody this additional nonrecursive path in the model.

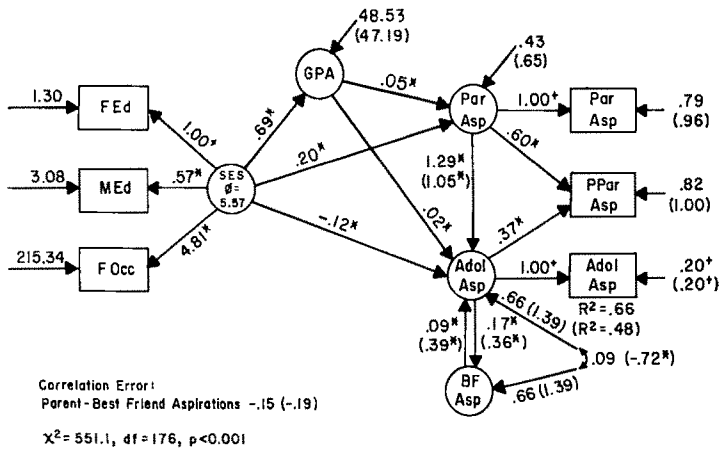


FIG. 4.—Nonrecursive psychosocial model of interpersonal influences on adolescent's educational aspirations. Reciprocal paths between adolescent's and best friend's aspirations. Perceived parental aspirations included as indicator of parental and adolescent aspirations ($N \leq 389$). Unstandardized coefficients for males and females in parentheses. Single coefficient is presented for parameters constrained to be equal across sex. Best friend variables are included for estimation of the model, and their structure is identical to that used for the adolescents. See fig. 1 for explanation of abbbreviations of variables.

1971; Duncan 1970; Duncan et al. 1972). Identical predictors were entered for the best friend and for the adolescent, although, among the best friend variables, only educational aspirations is presented in figure 4 and in table 1. With the exception of the reciprocal paths between friends' educational aspirations, the other structural coefficients and measurement model parameters were constrained to be equal across the friendship dyads. The number of cases for which information was available from the best schoolfriends' parents is only 389. The reduction in cases is caused by the requirement that data be available on quadrads, for ego and alter and their respective parents. A synthetic matrix of correlations was used, with correlations based on the largest number of cases available.[5]

The important finding of this nonrecursive model is that best friends are indeed involved in a process of mutual influence and that the influence of each friend on the other is approximately equal in importance. The effects of parental aspirations on adolescents' aspirations are still much

[5] Comparison of the magnitude of the peer effect (unstandardized) on adolescents' aspirations in the recursive (.02 for males, .15 for females) and in the nonrecursive model (.09 for males, .39 for females) should be approached cautiously. The effect is inflated in the recursive model by potential selection effects and in the nonrecursive model by the negative correlation between the residual terms of the regressions involving the pair of variables with reciprocal paths (see Gillespie and Fox 1980). The degree to which each of these effects inflates the peer effect and contaminates direct comparison of the size of the coefficients is unknown.

larger (standardized β's of .86 and .67 among males and females, respectively) than the effects of best friend's aspirations (.09 and .36). These results contrast with those of Duncan and his associates (Duncan et al. 1968; Duncan et al. 1972), who concluded that parental and peer influences were almost equal in importance. However, other studies, based on the same Lewanee sample analyzed by Duncan, report slightly higher effects of parental encouragement than peer aspirations on adolescent aspirations (Otto and Alwin 1977; Otto and Haller 1979). The apparent contradiction between the findings of the present study and those of Duncan et al. (1968) may result in part from the different operationalizations of the parental and peer aspiration measures in the two studies. In Duncan et al. (1968) best friend's ambition was measured by a weighting of educational and occupational aspirations. The indicator for parental aspirations was the unweighted sum of the parents' perceived degree of encouragement for continued schooling. To what extent actual parental aspirations and parental encouragement are similar can only be guessed at. There is a suggestion in the literature on peer influences, however, that the effects of encouragement variables on adolescent aspirations are much weaker than the effects of actual aspirations (Picou and Carter 1976). Thus, the use of a parental aspirations variable in the present study would increase the importance of parents relative to peers, compared with results based on a parental encouragement variable.

Age Differences in Interpersonal Influence on Aspirations

Although clearly there is increasing involvement with peers throughout the teen years (Bowerman and Kinch 1959; Douvan and Abelson 1966; McDill and Coleman 1965), whether or not such increasing involvement is accompanied by increasing detachment from parents is an unresolved issue (Kandel and Lesser 1972). In order to investigate possible differences in the relative influence of parents and peers over the age span of 14 through 18 years represented in the sample, the recursive model displayed in figure 3 was run in each of the four high school grades (table 2). Because of insufficient sample sizes within each school, pooled covariance matrices were not estimated. Instead, covariance matrices by sex and by grade were estimated, ignoring school. All parameters were constrained to be equal across the sexes by grade, except for the effects of parents and best friends on adolescent educational aspirations and the variances of the residual terms of the regressions.

The results indicate that parental influences increase through time, apparently changing at a faster rate for girls than for boys, although parental influences are uniformly higher for boys than for girls. For boys, peer influence is negligible in all grades, while for girls, peer influence is highest

TABLE 2

ESTIMATES OF TOTAL INTERPERSONAL INFLUENCES IN THE CAUSAL MODEL PRESENTED IN FIGURE 3 ON ADOLESCENTS' EDUCATIONAL ASPIRATIONS BY SEX AND BY GRADE

	MALE			FEMALE		
GRADE	Parent	Best Friend	*N*	Parent	Best Friend	*N*
9.....	.68*	.04	42	.36*	.33*	63
	(.54)	(.04)		(.28)	(.34)	
10....	.71*	−.02	108	.66*	.21*	167
	(.55)	(−.02)		(.52)	(.22)	
11....	1.02*	.09	85	.64*	.14	109
	(.79)	(.10)		(.50)	(.15)	
12....	.91*	.10	77	.86*	.20	110
	(.71)	(.10)		(.67)	(.21)	

NOTE.—Perceived parental aspirations included as an indicator of parental and adolescent aspirations. All other estimates in the model are virtually identical to those reported in fig. 3 and are not reproduced in the table. Other variables included in the analysis but not reproduced are socioeconomic status, including father's education, mother's education, and father's occupation, and grade point average. Standardized coefficients are presented in parentheses; $\chi^2 = 308$, df = 214, $P < .001$.

* Greater than twice standard error.

in the ninth grade and decreases in the later years of high school. Far from declining over the years of late adolescence, parents' influences over their children's future life plans increase relative to the influence of peers.

CONCLUSION

The Wisconsin model of status attainment constituted an important theoretical advance in the understanding of the process of educational and occupational attainment among males in the United States. The model could account for more than twice as much variance in educational attainment as the most systematic model that preceded it, that of Blau and Duncan (1967). The major distinctive feature of the Wisconsin model was the inclusion of social-psychological variables in the attainment process, namely, educational and occupational aspirations in adolescence and the interpersonal influences of significant others in shaping these adolescent aspirations.

The findings presented in this paper provide new and important insights into the role of interpersonal processes in the formation of educational aspirations in adolescence. Because the data are derived from a relational sample of triads composed of adolescents, parents, and best schoolfriends, the role of significant others could be ascertained not solely on the basis of perceptions but also from the self-reports of these others. The results inform our understanding of the social-psychological processes involved in status attainment, and of adolescent socialization more generally.

Parents are stronger influences than best friends in determining the

educational aspirations of adolescents. Furthermore, the importance of peers as socializers of educational plans is probably even weaker than is indicated by the present coefficients. Indeed, the models attributed all similarity in aspirations between friends to the effect of each friend's influence on the other and to social status and parental aspirations. The parameters for friends' influences are overestimated by the effects of assortative pairing in which similarity in educational plans precedes friendship formation. This is a fundamental problem of these models, as stressed by Duncan et al. (1968).

Far from declining over the adolescent years, the influence of parents relative to that of best friends increases. These data provide further supporting evidence for the growing realization that, far from rejecting their parents, adolescents rely on them for matters involving future life goals and careers (see Kandel and Lesser 1972; Williams 1972). This interpretation of adolescent development rejects the notion of an all-embracing generation gap. Although there is no doubt that peers can exert very strong influence in certain areas of adolescent behaviors, especially on those issues, such as the use of drugs, that are relevant to their current lifestyles (Kandel 1973; Kandel et al. 1978*a*, 1978*b*), on matters relevant to future life goals, parents are clearly of greater importance than peers.

To the extent that peer influences play a role, they appear to be more important among adolescent girls than among adolescent boys. Although Maccoby and Jacklin (1974) concluded in an extensive review of the literature on sex differences that affiliation needs were not stronger among girls than among boys, empirical data have been accumulating suggesting the existence of such sex differences (see e.g., Douvan and Abelson 1966; Rosenberg and Simmons 1975; Blyth, Simmons, and Bush 1978). The present findings indicate that the greater peer orientation of girls than of boys is accompanied by greater susceptibility to the influence of peers in the formation of future life plans.

Parental influences are crucial intervening links that explain the effects of social class on status attainment, effects that are mediated through parental aspirations for their children. Social class is a stronger determinant of parental aspirations than the adolescent's level of academic performance is. While the mediation of social class stratification effects through parental educational aspirations and encouragement has been noted by previous investigators (Sewell and Hauser 1972, 1975; Otto and Haller 1979), the existence of such mediation gains further support from our analyses, which are based on a direct assessment of parental aspirations as well as on the child's perception of such aspirations.

Indeed, a major contribution of this paper is to establish the potential distortions introduced into analyses that rely exclusively on a person's report of the characteristics of a significant other as an indicator of the

influence of that other. These distortions are of two types. On the one hand, perceptions *underestimate* the influence of the significant other. We have found that parental influence, in the form of parents' educational aspirations for their children, exerts itself directly in affecting the level of the child's own aspirations, without the child's apprehension of his parents' values.

On the other hand, the use of perceptions *overestimates* the importance of the significant other. In part perceptions reflect the very attributes of ego they are supposed to explain. This process was clearly demonstrated by regressing adolescents' perceptions of parents' aspirations on own aspirations and on parents' self-reported aspirations. Both paths were significant. A recursive model that includes only a perceptual measure of parental aspirations leads to greater explained variance of adolescent aspirations than a model that includes only parental self-reports, to an inflation of parental effects, and to distortions in the relative size of all other factors in the model.

Yet models of the educational attainment process have consistently relied on perceptual measures of parental aspirations. These models share this feature with much of sociological and social-psychological research concerned with the interpersonal influence of interacting actors upon each other. In the absence of relational samples in which independent data are obtained from alters and from egos, a common procedure involves measuring the influence of the significant other upon ego from ego's reports of the other's characteristics that are of interest (whether they are behaviors, values, or attitudes). This strategy is justified by reference to W. I. Thomas's famous dictum: "If men define situations as real, they are real in their consequences" (Thomas and Thomas 1928, p. 572). What this position omits from consideration, however, is that social reality as perceived by the individual is not only a reflection of the outside world but also a projection onto that world of a perspective that reflects the needs and social position of the perceiver. Individuals have a need for congruence between themselves and the outside world and will reconstruct the outside world to maximize congruence (Festinger 1957; Heider 1958). Furthermore, individuals located differently in the social structure may perceive the same event differentially, each from their own socially induced perspective. The use of perceptual indicators of interpersonal influence leads to tautological conclusions, because perceptions, which are used as predictors of a particular attribute, are themselves partly determined by those attributes.

APPENDIX

CORRELATIONS,* MEANS, AND STANDARD DEVIATIONS OF VARIABLES IN PSYCHOSOCIAL MODEL OF INTERPERSONAL INFLUENCES ON ADOLESCENTS' EDUCATIONAL ASPIRATIONS FOR MALES (Upper Diagonal) AND FEMALES (Lower Diagonal) IN FIGURES 1 THROUGH 3, AND FOR RECIPROCAL PEER INFLUENCE MODEL

	ADOLESCENT							BEST FRIEND						
	Father Education	Mother Education	Father Occupation	Student GPA	Perceived Parental Aspiration	Parental Aspiration	Student Aspiration	Father Education	Mother Education	Father Occupation	Student GPA	Perceived Parental Aspiration	Parental Aspiration	Student Aspiration
Father education (A)	1.0	.507	.584	.186	.222	.311	.232	.094	.104	.113	.134	.093	.093	.098
Mother education (A)	*.492*	1.0	.373	.202	.256	.374	.265	.166	.289	.074	.149	.120	.089	.164
Father occupation (A)	*.526*	*.274*	1.0	.197	.182	.304	.214	.113	.068	.175	.091	.098	.060	.131
Student GPA (A)	*.104*	*.182*	*.071*	1.0	.436	.424	.569	.137	.175	.172	.273	.227	.154	.227
Perceived parental aspiration (A)	*.247*	*.353*	*.181*	*.370*	1.0	.511	.707	−.031	.058	−.012	.170	.086	.123	.118
Parental aspiration (A)	*.337*	*.347*	*.204*	*.342*	*.520*	1.0	.538	.133	.157	.102	.208	.120	.189	.202
Student aspiration (A)	*.211*	*.284*	*.137*	*.424*	*.672*	*.490*	1.0	.114	.059	.156	.268	.122	.062	.213
Father education (BF)	*.064*	*.073*	*.088*	*.065*	*.093*	*.064*	*.113*	1.0	.649	.567	.312	.428	.345	.261
Mother education (BF)	*.028*	*.021*	*−.005*	*.079*	*.111*	*.129*	*.056*	*.578*	1.0	.343	.222	.352	.248	.203
Father occupation (BF)	*.010*	*.012*	*.093*	*.152*	*.070*	*.071*	*.134*	*.586*	*.259*	1.0	.167	.137	.235	.021
Student GPA (BF)	*.083*	*.203*	*.134*	*.367*	*.336*	*.259*	*.355*	*.188*	*.196*	*.203*	1.0	.258	.300	.380
Perceived parental aspiration (BF)	*.075*	*.109*	*.064*	*.204*	*.232*	*.177*	*.309*	*.366*	*.286*	*.322*	*.304*	1.0	.380	.639
Parental aspiration (BF)	*.082*	*.150*	*.007*	*.228*	*.247*	*.211*	*.217*	*.345*	*.400*	*.256*	*.388*	*.558*	1.0	.486
Student aspiration (BF)	*.132*	*.160*	*.139*	*.276*	*.328*	*.246*	*.371*	*.249*	*.290*	*.226*	*.409*	*.660*	*.525*	1.0
Means:														
Boys	13.80	13.10	51.88	84.51	16.32	16.42	16.06	14.27	13.67	52.54	84.31	16.21	16.63	15.92
Girls	*13.31*	*13.14*	*48.68*	*85.52*	*15.55*	*15.75*	*15.51*	*13.62*	*13.30*	*49.42*	*84.99*	*15.57*	*15.87*	*15.50*
Standard deviations:														
Boys	2.63	2.20	19.92	7.21	1.32	1.32	1.48	2.43	2.36	17.38	7.25	1.36	1.13	1.44
Girls	*2.60*	*2.20*	*19.14*	*6.83*	*1.50*	*1.43*	*1.72*	*2.74*	*2.13*	*19.14*	*7.18*	*1.57*	*1.50*	*1.78*

NOTE.—A = adolescent; BF = best friend.
* Weighted intraschool pairwise correlations.

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The North-South Earnings Gap: Changes during the 1960s and 1970s[1]

Charles Hirschman
Cornell University

Kim Blankenship
Duke University

Regional inequality within modern nation states has been a persistent feature of advanced capitalist economies, although the long-run expectation is one of convergence. We test the convergence hypothesis by examining the trend in the earnings gap between workers living in the South and North (the non-South) of the United States from 1960 to 1976. The results show some convergence for advantaged workers, although a substantial regional differential persists for black workers, blue collar workers, and workers with lower educational attainment. Most of the earnings gap between workers in the North and South cannot be explained by differences in the industrial, occupational, or educational composition of the work force.

The persistence of regional inequality in advanced industrial societies is an anomaly that challenges the tenets of neoclassical and modernization theories. While inequality between regions, classes, and persons may widen in the early stages of economic development, the long-run expectation is one of convergence—a leveling of income differences (Kuznets 1955, 1963; Williamson 1965). Nonetheless, regional income differences are a fairly common feature of many developed nations in Europe and North America. Southern Italy, the "Celtic Fringe" of the United Kingdom, and the South in the United States are a few of the most obvious examples. In this paper we measure the trend in earnings inequality between workers in the American South and North (the non-South) from 1960 to 1976, a period of significant socioeconomic change. We test a hypothesis of "convergence" which posits that regional income differences should be a function of variations in industrial structure and skill composition of workers. The empirical

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findings provide only minimal support for the convergence hypothesis. Alternative interpretations are discussed in the concluding section.

According to conventional theory, convergence results from the structural incorporation of outlying regions into the national economy. Mobility of labor and capital are thought to act as equilibrating forces within a national system. Indeed, the low wage levels in depressed regions may be the very stimulus that attracts investment and development. Other factors—such as the expansion of transportation, a standardized educational system, and national mass media that shape common cultural orientations—should also work as leveling mechanisms in the process of socioeconomic change. The convergence thesis is intertwined with the "logic of industrialism" hypothesis (Kerr et al. 1964) that is the bedrock of modernization theory.

In spite of the long-standing regional differences between the North and South, there is evidence of considerable convergence, both socially and economically, during the 20th century, especially since World War II (McKinney and Thompson 1965). McKinney and Bourque (1971) examined census data from 1900 to 1960 and found diminishing differentials in urbanization, industrial and occupational composition, educational attainment, and other indicators of socioeconomic status. More recently, Featherman and Hauser (1978, chap. 7) have found only minimal differences in the industrial and occupational composition of the male labor force between the North and South by 1973, although they note about a 10-year lag in educational levels in the South. While regional differences in economic attainment (income and earnings) continue to exist (Featherman and Hauser 1978, chap. 7; Farley 1977), they would be expected to diminish as part of the general trend toward convergence. The 1960s and 1970s are a particularly appropriate period for an examination of North-South economic inequality. Civil rights legislation, the substantial flow of migrants from the North to the South, and the general forces of socioeconomic change are thought to have diminished regional distinctions, especially in the economic sphere.

Our focus is on regional differences in earnings—labor income derived from wages, salaries, and self-employment in businesses and farms. First, we ask whether absolute and relative earnings differences between the North and South have narrowed in recent years. Then we turn to a test of the convergence hypothesis which suggests that regional differences in earnings are a function of differing economic structures and human capital resources. As the regions converge in their industrial structure and the populations become homogeneous with respect to skills, earnings differences should also be minimized. This hypothesis is tested for subgroups of the population, by race, sex, and socioeconomic position, to see whether regional patterns in inequality vary with other bases of stratification in

American society. We do not address broader questions of regional differences in family income or the impact of governmental redistribution of income.

DATA AND VARIABLES

Our analysis rests on three broadly comparable data sources for 1960, 1970, and 1976, namely, the 1960 and 1970 population censuses and the 1976 Survey of Income and Education (SIE).[2] In each case, earnings data were collected with reference to the calendar year prior to the interview, 1959, 1969, and 1975, respectively. Other characteristics, such as occupation, education, and age, were recorded as of the time of the interview, which was April for the censuses and from April to July for the 1976 SIE. In this analysis, we have used the 1/10,000 sample tapes from the 1960 and 1970 censuses, which were prepared by the Bureau of the Census (U.S. Bureau of the Census 1972, 1975). The SIE was approximately a 1/500 sample of noninstitutional households in the United States (U.S. Bureau of the Census 1977). From the master public use SIE data file, we selected a 10% systematic random sample of all individual records, appropriately weighted.

In order to limit our analysis to those who regularly receive earnings, our comparable universe from the three data sources includes all persons, aged 25–64, who reported receiving nonzero earnings during the previous year and had a known occupation, industry, and educational attainment. These criteria yielded samples of 5,279 for 1960, 6,392 in 1970, and 14,953 in 1976. The larger sample size in 1976 makes statistically significant results appear somewhat more common for a given absolute difference.

The South consists of the states identified within that region by the Bureau of the Census, namely, Delaware, Maryland, West Virginia, Virginia, Kentucky, Tennessee, North Carolina, South Carolina, Georgia, Florida, Alabama, Mississippi, Arkansas, Louisiana, Oklahoma, and Texas. We refer to the non-South as the North, although it includes the Midwest and Far West.

Earnings are defined as money receipts from wages, salaries, and self-employment (farm and nonfarm). In order to make comparisons across time, the earnings data for 1959 and 1969 were converted into 1975 dollars, adjusting for changes in the Consumer Price Index for the interven-

[2] The data in all three sources were collected by the U.S. Bureau of the Census (1972, 1975, 1977), which maintains fairly standardized methods of data collection across censuses and national surveys. The one important difference is that the 1976 SIE sampled only from the noninstitutional household population, while the censuses covered both the household and institutional population. Since the institutional population is very small and few institutional residents receive earnings, this difference in the universes should not be a major source of bias.

ing years (U.S. Bureau of the Census 1978, p. 266). Because we are not comparing welfare levels but standards of pay, earnings data are not adjusted for regional differences in the cost of living. Moreover, cost of living data are available only for urban areas, and recent data suggest that there are only minor differences between urban areas in the South and other regions (Bahr and Ptacek 1975).

The basic ascriptive variables of age, sex, and race are coded in conventional fashion. Age, used as a control variable in the multivariate analyses, is divided into 10-year age groups (25–34, 35–44, etc.). Race is coded in two categories: black and nonblack. In this scheme, the small number of Asian-Americans and persons of other races are classified with whites rather than with the black population. Education and occupation are coded into both categorical variables and interval-scale variables, depending on the particular analysis. Industry is used as a control variable and is coded into 12 major categories.

TREND IN REGIONAL INEQUALITY: RACE AND SEX

Table 1 presents two summary measures of the trend in regional inequality: the ratio of mean annual earnings of southern to northern workers, and the "earnings gap" between the southern and northern mean earnings.[3] In addition to the figures for the total population of workers, we present trends for men and women separately, and by race for both men and women. First, we will consider overall differences (under the heading, "Gross"), then turn to the results adjusted for regional differences that might account for the observed differences (according to the convergence hypothesis).

In 1959, mean earnings in the South were about 75% of mean earnings in the North. The absolute gap between the means in the two regions was about $2,300 (1975 dollars). There was a definite trend toward convergence during the 1960s, as the southern mean rose to 88% of the northern figure and the absolute gap narrowed by almost half to less than $1,300 in 1969. However, the gap widened to almost $1,900 during the first half of the 1970s and the relative ratio dropped to 84%. This reversal does not correspond to the expectation of a consistent secular trend implied by the convergence hypothesis. This change might be attributed

[3] There is considerable controversy over the appropriate statistical indicators of inequality. A long-standing debate is over the use of relative or absolute measures of income differences (Villemez and Rowe 1976). It is possible for an income gap to be narrowing relatively while the absolute difference is widening. Conceptually, we prefer to focus on absolute earnings differences (measured in dollars) because consumption, and therefore differences in life-styles, between populations are a function of differential purchasing power generated by income. But in deference to prior work, we present our findings with relative and absolute indicators.

TABLE 1

TRENDS IN RELATIVE AND ABSOLUTE DIFFERENCES IN MEAN EARNINGS (In 1975 $) BETWEEN WORKERS[a] AGED 25–64 IN THE SOUTH AND NON-SOUTH, BY SEX AND RACE: UNITED STATES, 1959, 1969, 1975

	RATIO OF SOUTH TO NON-SOUTH MEAN EARNINGS[b]						EARNINGS GAP[c]											
	Gross			Net[c]			Gross			Net[c]			MEAN EARNINGS[d]			SAMPLE SIZES		
	1959	1969	1975	1959	1969	1975	1959	1969	1975	1959	1969	1975	1959	1969	1975	1959	1969	1975[f]
Total population (25–64)	74	88	84	81	90	88	2,321*	1,282*	1,888*	1,671*	1,077*	1,312*	8,344	10,688	10,956	5,279	6,392	14,953
Males (25–64)	75	88	85	81	91	88	2,880*	1,621*	2,208*	2,101*	1,195*	1,718*	10,539	13,700	14,382	3,476	3,937	8,791
Black males (25–64)	61	63	70	71	72	76	2,636*	3,874*	3,446*	1,842*	2,725*	2,650*	5,418	8,595	9,601	274	330	495
Nonblack males (25–64)	80	94	89	84	94	90	2,336*	928*	1,639*	1,872*	877*	1,548*	10,977	14,167	14,807	3,202	3,607	8,296
Females (25–64)	78	92	88	88	93	95	961*	457*	708*	514*	405*	306*	4,112	5,858	5,902	1,803	2,455	6,162
Black females (25–64)	57	67	70	77	80	81	1,431*	1,790*	2,122*	646*	1,005	1,293*	2,471	4,617	6,064	220	260	542
Nonblack females (25–64)	88	97	91	90	95	96	529*	168	542*	436*	287	250*	4,340	6,005	5,881	1,583	2,195	5,620

SOURCES.—1960 population census, .001 sample; 1970 population census, .001 sample (15% neighborhood file); 1976 Survey of Income and Education, .10 systematic random sample (U.S. Bureau of the Census 1975, 1972, 1977, respectively).

[a] Sample consists of all workers who received any earnings during the previous year and reported occupation, industry, and education.

[b] Ratio of the mean earnings of all workers in the South to the mean earnings of all workers in the non-South multiplied by 100.

[c] Absolute difference between the mean earnings of all workers in the non-South and mean earnings of all workers in the South (non-South—South).

[d] Mean Earnings of all workers in the United States.

[e] Earnings differences between the non-South and South adjusted for the additive effects of occupational, industrial, and educational composition, using multiple classification analysis.

[f] Unweighted number of cases.

* Statistically significant at the .05 level.

to the stark differences in the economic conditions of the two periods; the 1960s were a period of rising prosperity, while the early 1970s saw two major recessions and rapid inflation that left average incomes almost stagnant. Poorer economic times were associated with a widening of regional economic inequality.

The pattern of a narrowing of regional inequality in the 1960s followed by a widening difference holds for some, but not all, subpopulations of workers. It holds for male and female workers as a whole, but the picture for black workers is much more complex.

For black men, there has been a slow but steady trend toward *relative* equality between the regions. Yet there are much larger initial differences in regional inequality for blacks, and a very different pattern emerges in absolute inequality as measured by the earnings gap. In 1959, black males in the South averaged 61% of the comparable mean for the North. Among white males, southern workers averaged 80% of their northern cousins' earnings. Clearly, the southern disadvantage was most acute for blacks. The relative regional ratio for black men had closed to 70% by 1975 (compared with 89% for whites), but the absolute regional difference of $3,500 in 1975 was almost $900 greater than that in 1959. It can be argued that northern residence conferred a greater economic advantage (than southern residence) on black men in 1975 than in 1960.

For working women, regional inequality must be understood in the context of the very low earnings of women everywhere. With mean earnings of women only slightly more than half those of men, there is a fairly low ceiling that constrains many other institutional differentials. Thus North-South differentials seem rather modest in comparison with those for men. For white women there is only a modest regional difference of about $500 (less in 1969) that is barely statistically significant. For black women, however, there is a substantial regional differential that has narrowed in relative terms over the years but widened in absolute dollars. Indeed the 1975 earnings gap of over $2,000 between black women in the South and the North is one of the most dramatic findings in the table.

How are these differentials and trends to be explained? Why do southern employers pay less to their workers than those in the North? Why have the regional differences narrowed for white workers, but remained wide for black workers, both male and female? There is a variety of possible explanations for these patterns, relating to the regional structures of employment and the differential composition of the working populations. Perhaps most obvious is that employment in the North has traditionally been concentrated in industries with higher wage levels. Another common explanation for lower earnings in the South is the lower skills of southern workers, both occupationally and educationally (McKinney and Bourque 1971). In fact, one of the central elements of the convergence hypothesis

is that the elimination of educational differentials between the regions will ultimately result in the elimination of regional disparities in earnings.

In table 1, under the headings labeled "Net," we present measures of North-South earnings differentials (both relative and absolute indicators) that have been statistically adjusted for the regional differences in industrial structure, occupational composition, and educational levels. Using a regression equation (multiple classification analysis) predicting earnings, the region variable (South and non-South) was entered as an independent variable along with three sets of categorical variables for industries (12 categories), occupations (10 categories), and years of completed schooling (five categories).[4] The resulting regional coefficients are "net" of the additive effects of the three other variables and can be interpreted as the magnitude of the economic inequality which would exist if the South and North shared a common industrial structure and had equal occupational and educational distributions.

In general, the net earnings gap is roughly two-thirds to three-quarters of the gross differences for the various populations in table 1. In other words, part of the regional inequality in earnings is due to differences in the regional structure of employment and skill levels, but most of the gap is independent of these factors. This is true for 1975 as for 15 years earlier. Workers employed in the same industries, with the same occupations, and with comparable education tend to receive less in the South than the North. Whatever the source of the net regional difference, it remains a very real disadvantage, especially for black workers.

TREND IN REGIONAL INEQUALITY AMONG WORKERS: SOCIOECONOMIC GROUPS

If there is a drift toward regional equality, as there seems to have been for white workers, has it been a glacial one engulfing workers throughout the socioeconomic spectrum, or has its path been more narrow and selective? To address this issue, we have assembled data on mean earnings by major occupational categories for 1959, 1969, and 1975 and by highest level of schooling completed (table 2). While some of the same individuals may be included in two (or three) time points, our focus is on the trend

[4] These variables were chosen because they represent the key factors in the convergence hypothesis and also because they were measured in a comparable fashion in the three data sources (the 1960 and 1970 occupational classifications are comparable for the broad categories). Additional variables would have been desirable, especially rural-urban location. However, neither urban-rural residence nor a metro-nonmetro classification was available for all cases in the census public use sample tapes. The farm-nonfarm distinction is captured in the industrial classification. We also estimated regression equations using the natural log of earnings as the dependent variable. The results are essentially the same as those with earnings in dollars.

TABLE 2

TRENDS IN RELATIVE AND ABSOLUTE DIFFERENCES IN MEAN EARNINGS (in 1975 $) BETWEEN WORKERS AGED 25–64 IN THE SOUTH AND NON-SOUTH, BY OCCUPATION AND YEARS OF COMPLETED SCHOOLING: UNITED STATES, 1959, 1969, 1975

	Ratio of South to Non-South Mean Earnings						Earnings Gap						Mean Earnings			Sample Sizes		
	Gross			Net[a]			Gross			Net[a]								
	1959	1969	1975	1959	1969	1975	1959	1969	1975	1959	1969	1975	1959	1969	1975	1959	1969	1975
Occupation																		
Professionals	77	97	95	84	101	95	3,085*	509	900	2,113*	−123	842	12,821	14,720	16,383	598	918	2,615
Managers and proprietors	86	95	78	93	95	84	2,240	919	4,152*	1,102	944	3,075*	15,924	18,453	17,994	456	564	1,843
Sales, nonretail	89	84	88	95	87	95	1,538	2,556	2,117	646	2,069	875	13,363	15,708	16,590	150	202	404
Clerical	98	100	99	100	98	101	131	8	56	−34	128	−75	6,440	8,079	7,128	749	1,064	2,522
Sales, retail	92	114	75	89	106	81	334	−1,012	1,670*	470	−453	1,245*	4,379	7,545	5,962	215	238	432
Foremen	83	85	85	81	86	88	2,060*	2,333*	2,128*	2,321*	2,085*	1,630*	11,806	14,639	13,534	118	159	319
Craftsmen	74	86	79	81	88	87	2,670*	1,791*	2,382*	1,854*	1,488*	1,442*	9,445	11,977	10,838	756	864	1,804
Operatives, manufacturing	79	74	81	83	80	87	1,524*	2,345*	1,547*	1,193*	1,764*	1,016*	6,869	8,282	7,623	709	771	1,340
Operatives, other	72	86	109	75	93	116	2,174*	1,292*	−857	1,911*	679*	−1,444	6,929	9,133	9,768	357	401	783
Service, nonhousehold	63	84	79	76	89	85	1,872*	1,124*	1,307*	1,169*	715*	873*	4,581	6,561	5,728	500	681	1,678
Service, private household	68	79	81	73	68	78	562*	482	448	459*	782	540	1,474	2,071	2,130	143	87	150
Laborers, nonfarm	56	71	72	66	75	77	3,148*	2,724*	2,329*	2,324*	2,316*	1,894*	6,186	8,406	7,516	247	232	507
Farmers	53	83	103	69	92	114	3,505*	1,603	−263	2,196*	704	−1,251	6,379	9,222	9,487	189	129	380
Farm laborers	50	61	64	65	77	91	1,762*	1,947*	2,055	1,131*	1,023*	468	2,582	3,909	4,809	92	82	176
Years of Schooling																		
0–8	72	77	84	81	80	87	1,900*	1,936*	1,249*	1,245*	1,653*	991*	6,021	7,624	7,076	1,712	1,263	1,694
9–11	68	83	75	79	87	83	2,647*	1,630*	2,200*	1,726*	1,297*	1,458*	7,676	9,367	7,978	1,163	1,277	2,090
12	80	92	92	81	95	95	1,755*	890*	734*	1,672*	569*	513*	8,316	10,403	9,491	1,360	2,302	5,699
13–15	92	110	93	100	106	93	751	−1,219	758	−14	−735	742*	9,341	12,257	10,922	519	742	2,584
16 or more	88	97	91	94	99	90	2,120	489	1,684	925	195	1,917	16,483	16,936	18,489	525	808	2,886

SOURCES.—1960 population census, .001 sample; 1970 population census, .001 sample (15% neighborhood file); 1976 Survey of Income and Education, .10 systematic random sample (U.S. Bureau of the Census 1975, 1972, 1977, respectively).

NOTE.—Table 1 notes a, b, c, d, and f apply to table 2.

[a] Net of the additive effects of age, sex, race, industry, and education. Several occupations (retail sales workers, manufacturing operatives, and household service workers) are by definition within one industry.

* South/non-South difference is statistically significant at the .05 level.

in regional inequality for entire socioeconomic classes, not individuals within those classes. Table 2 follows the same format as table 1 with gross (actual) measures of relative and absolute inequality and comparable "net" indicators, the latter adjusted for regional differences in age, sex, race, industry, educational composition (when comparing occupational differences), and occupational composition (when comparing educational differences).

A fairly consistent pattern emerges from the results in table 2, although there are a few important exceptions. The regional differentials tend to be smaller for white collar than for blue collar workers, and the convergence trend has been more pronounced among the higher-status occupations. The greatest change has occurred in the regional differences for professional workers. In 1959, southern professionals received only about three-quarters of the earnings of their northern counterparts; the absolute gap was $3,000. Even if one controls for regional differences in demographic, racial, and educational composition, the gap was $2,000. During the 1960s, however, regional differences in earnings for professional workers became insignificant, independent of compositional differences. Differences among managers were insignificant throughout the 1960s but widened mysteriously in the early 1970s. The absolute difference of $4,000 in 1975 (three-quarters of which is independent of composition) was the widest found for any occupation. There is a fairly substantial, though statistically nonsignificant, regional difference for nonretail sales workers, most of which is due to compositional differences. Differences are also statistically insignificant (with one exception in 1975) for clerical and retail sales workers, which may be partially explained by the low earnings ceiling associated with these occupations.

The most consistent findings in table 2, however, are the sizable and persistent earnings differences between the North and South for most blue collar occupations. The ratios of South to North mean earnings are generally in the 60%–80% range, and the significant absolute dollar gaps range from $1,300 to $3,000. While there have been modest reductions in the relative indicator of regional inequality for blue collar workers, this is rarely evident in the absolute earnings gap. There are exceptions to this pattern for a few blue collar categories, but the basic generalization holds. Even after controlling for regional differences in race, sex, and age composition, and in industrial structure and educational levels, the net differences are still substantial—generally in the $1,000–$2,000 range—with only modest reductions over the years.

Turning to the trend by educational attainment in table 2, we find a similar pattern of a larger and more persistent regional inequality for those at the bottom of the socioeconomic hierarchy. None of the differences for

college graduates and only one of the differences for those with some college attendance (net difference in 1975) are significant. However, those with less formal schooling, especially those with fewer than 12 years, face a greater economic disadvantage in the South. The gap has narrowed, particularly for high school graduates, but it remained significant in 1975. As before, the majority of the regional earnings differences are independent of differences in population composition and employment structures between the regions.

Thus we see that the economic differences between the South and North are selective in that some groups of workers are much more adversely affected than others. Even though regional disparities are slowly declining, the most disadvantaged groups have experienced the least progress toward regional equality.

REGIONAL DIFFERENCES IN ECONOMIC RETURNS TO EDUCATION AND OCCUPATION

The foregoing analysis has shown that earnings differences remain between the South and the North, especially for selected occupational and educational subpopulations. These differences have persisted in spite of considerable movement toward interregional parity in economic institutional structure. A related question is whether there are regional differences (and changes) in earnings returns to educational and occupational achievement. The analytical strategy shifts here to one with assumptions of linear relationships between educational attainment, occupational achievement, and earnings. While such assumptions may not fit perfectly, there are significant gains in the analytical power to summarize statistically the complex relationships in the stratification process and to compare these relationships between the regions. Specifically, we regress annual earnings on educational attainment, years of work experience, weeks worked in the year prior to the interview, and occupational achievement for both southern and northern workers, and then we compare the regression coefficients for the two regions at the three time points.[5]

For reasons of space, we present only the findings for male workers, separately by race. Earnings are measured in constant 1975 dollars, education is measured in years of completed schooling, and occupation is measured with Duncan's Socioeconomic Index (SEI). Years of work experience is measured by subtracting the sum of the number of years of completed schooling plus six (assumed age at entry into first grade) from current age. The use of weeks worked last year as a covariate in this analy-

[5] Means and standard deviations of all variables in this analysis are presented in the Appendix.

sis may moderate North-South economic differences if part of the southern disadvantage is due to higher incidence of unemployment or part-year employment opportunities. Thus the test is a conservative one, holding constant potentially important factors.

Table 3 presents the regressions (with unstandardized regression coefficients) of annual earnings on years of schooling, work experience, weeks worked, and occupational SEI for black and white males in the South and North for 1959, 1969, and 1975.

Consistent with our findings in table 1 we see that among white workers, there is a net disadvantage to being in the South, as revealed by the difference in the constant term, even though the difference has decreased

TABLE 3

REGRESSION ANALYSIS[a] OF EARNINGS ON YEARS OF COMPLETED SCHOOL, OCCUPATIONAL STATUS, WORK EXPERIENCE, AND WEEKS WORKED FOR MALE WORKERS AGED 25–64 IN THE SOUTH AND NORTH, BY RACE: UNITED STATES 1959, 1969, 1975.

INDEPENDENT VARIABLES	1959		1969		1975[b]	
	South	North	South	North	South	North
Black males (25–64):						
Years of schooling	197.2*	−29.4	244.3*	499.1*	377.5*	1,896.1*
	(55.9)	(101.2)	(84.3)	(149.4)	(83.4)	(443.8)
Occupation SEI	31.5*	34.4*	73.4*	60.6*	62.7*	209.8*
	(12.3)	(17.8)	(16.0)	(22.1)	(11.7)	(50.6)
Work experience	13.1	−14.8	14.8	49.1	38.1	340.5*
	(17.1)	(27.2)	(23.2)	(34.9)	(19.2)	(93.4)
Weeks worked	77.7*	117.5*	119.8*	206.1*	121.4*	199.1*
	(16.4)	(24.6)	(24.6)	(41.3)	(15.2)	(64.1)
Constant[c]	4,816	6,839	7,621	10,421	8,697	11,260
R^2	.295	.242	.348	.263	.357	.188
N	150	124	155	175	262	233
Nonblack males (25–64):						
Years of schooling	520.0*	669.2*	833.8*	633.3*	916.2*	995.1*
	(110.7)	(82.5)	(130.9)	(76.4)	(146.4)	(103.2)
Occupation SEI	103.6*	111.7*	124.9*	124.1*	117.0*	156.5*
	(16.0)	(10.4)	(18.6)	(9.3)	(19.2)	(12.1)
Work experience	53.6	96.2*	130.2*	91.7*	187.4*	197.6*
	(29.1)	(19.5)	(32.9)	(16.3)	(33.1)	(21.3)
Weeks worked	140.9*	212.3*	219.4*	253.2*	246.5*	231.6*
	(29.8)	(23.0)	(42.1)	(21.9)	(33.9)	(23.8)
Constant[c]	9,924	11,547	13,869	14,453	14,331	15,302
R^2	.211	.188	.193	.211	.116	.121
N	817	2,385	985	2,622	1,476	6,820

SOURCES.—1960 population census, .001 sample; 1970 population census, .001 sample (15% neighborhood file); 1976 Survey of Income and Education, .10 systematic random sample (U.S. Bureau of the Census 1975, 1972, 1977, respectively).

[a] Metric (unstandardized) regression coefficients are presented for each population, with approximate SEs in parentheses below. Variable coding is explained in the footnotes to the Appendix table.

[b] Coefficients are based on weighted sample, but reported N's are unweighted.

[c] Adjusted to the mean level of education and occupation of North.

* Statistically significant at the .05 level.

over time.[6] There does not appear to be consistent evidence of "regional interactions" for white workers in the relationships between education, work experience, weeks worked, and SEI, on the one hand, and earnings, on the other. The slopes are positive and significant for both regions, and the magnitudes of the slopes are generally similar for all three time points. One significant exception is for education in 1969, when the educational regression coefficient of the southern workers is $200 ($833 — $633) higher than the northern figure. The effect of occupational status, as indexed by the SEI, on earnings, has become more steep over time in both regions, but with minimal difference between the South and North. (For white men the results are quite similar to those found by Featherman and Hauser [1978], chap. 7.)

Among blacks, there are considerably more fluctuations, which may reflect the small sample sizes to some extent, as well as real trends and differentials. Clearly the regional difference in the adjusted constant terms is very large in comparison with that for whites—so large that in absolute terms, the dollar gap for blacks has widened over time. The education and SEI regression coefficients are more difficult to interpret. In 1959, both education and occupation had a significant net effect on earnings among black workers in the South, but in the North, only occupation had a significant effect. The effects were much smaller for blacks than for whites and might simply reveal the very low relationships between social background and economic achievement among black workers (Duncan 1969).

By 1969, and especially in 1975, there was a growing difference between the regional coefficients among black men. Educational attainment and occupational standing seemed to bring substantially higher rewards in the North than the South for black male workers. These findings suggest that economic differentiation among northern black residents increased as credentials (education, occupation) gained more influence in employers' decisions about wages and salaries. Northern residence may be becoming

[6] The constant term represents the value of the dependent variable at the intercept values (zero) of the independent variables. Since a person with zero schooling, zero occupational status (indexed by the Duncan SEI), and zero work experience is a rather unreal reference point, the constant term does not yield a meaningful interpretation (in many cases, it is a negative value). A simple transformation which aids the interpretation consists of adjusting the constant to the mean values of the independent variables. The adjusted $a^* = a + b_1X_1 + b_2X_2$, where a is the original constant, b_1 and b_2 are the metric regression coefficients, and X_1 and X_2 are the mean values of the independent variables (see Wright and Perrone 1977, p. 39). For comparison between northern and southern workers, the North mean values on the independent variables are put into both the South and North equations. Thus the resulting difference between the adjusted constant terms holds constant average differences in education, occupation, and work experience.

more of an advantage for highly educated blacks but not for those with lower socioeconomic credentials.

DISCUSSION AND CONCLUSIONS

The South remains a disadvantaged economic region within the United States, as measured by the average earnings of workers. There are definite signs of convergence between the South and the rest of the country, but the gap was far from eliminated in the mid-1970s. The economic handicap of southern residence has been an uneven one, with blacks, blue collar workers, and lowly educated workers earning considerably less in the South than in the rest of the country. Workers in more favored circumstances, including whites, well-educated persons, and those in white collar occupations, have traditionally encountered less of a disadvantage in the South and have been able to reach parity with their northern counterparts in the 1960s and early 1970s.

The lower level of earnings in the South is due in part to the disproportionate number of southern workers in low-paying industries and occupations and with inferior educational credentials. But holding all these factors constant—as well as the racial and demographic composition of the work force—more than half (usually 70%–80%) of the southern disadvantage remains for most subpopulations. And this fact—the net effect of southern residence on earnings—accounts for a fairly constant proportion of the overall disadvantage throughout the 1960s and early 1970s. Furthermore, for white workers the "process of stratification" works in nearly the same way in the North and the South—the earnings returns to schooling and occupational standing are roughly equivalent. However, for blacks (in the 1970s) education and occupation result in much greater economic payoffs in the North than in the South. The findings of this analysis provide only modest support to the convergence hypothesis. The inconsistent patterns and the strong association of regional inequality with other bases of stratification cause us to look elsewhere for theoretical interpretation.

In spite of the widespread acceptance of the hypothesis of regional economic convergence, there is a long-standing alternative perspective of cumulative advantage and disadvantage as inherent in the process of capitalistic development. For instance, Myrdal (1957, chap. 3) suggests that "backwash" effects of economic growth lead to underdevelopment of peripheral provinces and regions (also see Bluestone 1972; Hechter 1975; Dowd 1956). Proponents of the cumulative-disadvantage perspective suggest that it is the relative national political power of local business and labor that shapes regional economic disparities, not simply the spread of

capital and investment from the centers of the national economy. Needless to say, the basic theoretical issues are far from completely formulated, let alone subjected to definitive empirical test. (For an overview of recent theory and research from this alternative perspective, see the special issue on Uneven Regional Development in *Review of Radical Political Economics* [1978].)

In the particular case under study, we need to reconsider why the process of wage determination should differ between the North and South for disadvantaged workers. One interpretation of wage differences could be that they reflect a relative scarcity of labor. However, there do not appear to be substantial differences in regional levels of unemployment (Gellner 1974). Unemployment did rise somewhat faster in the North in the early 1970s, but this was a break from rough parity in the 1960s. Moreover, interregional migration has long served as a mechanism to redistribute surplus labor to regions with greater opportunities. However, there are institutional differences that may have been important: differentials in unionization and in community standards for wages. Southern and northern labor markets appear to differ qualitatively with regard to both factors, although it is not possible to test these hypotheses with census data.

Unionization of the South has lagged far behind that of the North and left southern workers in a much weaker position in the social process of wage determination. Expected wage levels or community standards for appropriate pay also seem to have been considerably lower. These normative elements reflect, of course, the relative power of workers and employers. This interpretation is supported by earlier (1959) differentials in earnings and the trend analysis, both of which show that the most disadvantaged workers experience the greatest regional inequality. Whites, especially the well educated, and those with higher-status occupations were better able to "bargain" with employers on terms that essentially represent national standards. Perhaps this is because of common knowledge of relative pay levels for professionals (and other highly skilled workers) throughout the nation. Blacks and blue collar workers appear to be more constrained by their position and relative lack of power. Their pay seems to be conditioned more by the local job market than by the national standards. Although regional differences have been reduced, southern workers continue to experience a significant disadvantage in their paychecks.

APPENDIX

MEANS AND STANDARD DEVIATIONS OF SOCIOECONOMIC VARIABLES OF WORKERS AGED 25–64 IN THE SOUTH AND THE NORTH, BY RACE: UNITED STATES, 1959, 1969, 1975

	1959		1969		1975	
	Mean	SD	Mean	SD	Mean	SD
			South			
Black males (25–64):						
Years schooling	6.6	4.2	8.1	3.6	10.1	3.4
Occupation SEI	17.8	16.4	19.7	15.4	24.3	19.1
Weeks worked	44.2	11.2	46.5	9.7	44.1	12.4
Work experience	30.0	13.0	26.9	12.7	24.8	13.1
Earnings	4,225	2,607	6,540	3,551	7,908	4,465
N	150		155		262	
Nonblack males (25–64):						
Years schooling	9.8	3.9	11.0	3.7	12.0	3.6
Occupation SEI	36.5	24.0	40.7	23.4	43.5	24.6
Weeks worked	46.5	10.2	48.1	8.4	46.4	10.8
Work experience	26.6	11.9	25.5	12.3	23.5	12.5
Earnings	9,227	9,491	13,480	12,270	13,650	18,194
N	817		985		1,476	
			North			
Black males (25–64):						
Years schooling	9.1	3.3	10.2	3.4	11.4	3.0
Occupation SEI	21.0	16.3	27.3	19.4	32.0	21.8
Weeks worked	45.2	10.7	47.0	8.6	43.2	14.5
Work experience	25.1	11.3	24.7	12.5	23.3	12.4
Earnings	6,861	3,241	10,415	5,458	11,354	19,262
N	124		175		233	
Nonblack males (25–64):						
Years schooling	10.7	3.4	11.6	3.2	12.6	3.2
Occupation SEI	37.9	23.8	39.8	23.3	43.5	24.6
Weeks worked	47.3	8.9	48.0	8.2	46.8	10.1
Work experience	26.3	12.1	25.8	12.7	23.3	12.6
Earnings	11,577	10,953	14,425	10,345	15,261	19,380
N	2,385		2,622		6,820	

SOURCES.—1960 population census (U.S. Bureau of the Census 1975), .001 sample; 1970 population census (U.S. Bureau of the Census 1972), .001 sample (15% neighborhood file); 1976 Survey of Income and Education (U.S. Bureau of the Census 1977), .10 systematic random sample.

NOTE.—Sample consists of all workers who received any earnings in the previous year and reported an occupation, industry, and education. (1) "Years schooling" is coded in single years of completed schooling, ranging from 0 (no schooling) to 18 (six or more years of college). (2) "Occupation SEI" is based on Duncan's Socioeconomic Index of Occupations (SEI). The source for 1960 is Duncan (1961, Appendix B-1); for 1970 and 1976, the SEI scores are in Hauser and Featherman (1977, Appendix B). (3) "Weeks worked" represents weeks worked during the calendar year preceding the census, coded as the midpoint of six intervals: less than 13 = 6.5; 14–26 = 20; 27–39 = 33; 40–47 = 43.5; 48–49 = 48.5; and 50–52 = 51. (4) "Work experience" is estimated by the number of years since the completion of formal schooling. The formula is: work experience = current age − (6 + years of formal schooling). (5) "Earnings" are adjusted to constant 1975 dollars. (6)The 1975 *N*'s are unweighted.

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Suicidal Motor Vehicle Fatalities in Detroit: A Replication[1]

Kenneth A. Bollen
General Motors Research Laboratories

David P. Phillips
University of California, San Diego

Recent research by Phillips suggested that publicized suicide stories triggered a rise in suicides, some of which were disguised as motor vehicle fatalities (MVF). The most striking finding of his research was a 31% jump in California MVF on the third day after publicized suicide stories. Yet, until they are replicated, we do not know whether these results are limited to: (1) California, (2) the time period studied (1966–73), or (3) the method of analysis used. In this research note we replicate Phillips's California analysis with Detroit metropolitan data for 1973–76. We use two different statistical techniques to insure that Phillips's findings are not an artifact of his method of analysis. We find a 35%–40% increase in Detroit MVF on the third day after a publicized suicide story. Our replication suggests that Phillips's most striking result—the third-day peak in MVF—is not limited to a particular geographical region, time period, or technique of analysis.

> We do not even take our own observations quite seriously, or accept them as scientific observations, until we have repeated and tested them. [Popper (1934) 1959]

The above quotation from Karl Popper restates what has long been a truism in science—replication is indispensable. Current sociological texts stress that "replicability is an essential norm of science" (Babbie 1979, p. 522). Yet despite this recognition, replication in sociology is very rare. Perhaps this is so because replication, like most research, is expensive.

[1] This research would not have been possible without the help of a number of people whom we wish to thank. Carroll DeWeese and Don Sunday of the General Motors Research Laboratories, Betty Havlena of the *Detroit News*, and Michele Kapechy of the *Detroit Free Press* were essential in obtaining access to the publicized suicide stories. Tom Laffey and Craig Palmer helped in the data collection and analysis. Lenny Dintzer and Ken Barb provided comments on an earlier draft. Linda Meyer and Marilyn Southern typed several drafts and the final version of this paper. The Inter-University Consortium for Political and Social Research supplied the mortality data. Requests for reprints should be sent to Kenneth A. Bollen, Societal Analysis Department, General Motors Research Laboratories, Warren, Michigan 48090.

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This expense may not be justified unless the research to be replicated has substantial implications or produced counterintuitive results.

One series of studies which, if valid, has substantial implications and surprising results is by Phillips (1974, 1977, 1979). In the first of these investigations, Phillips (1974) showed that U.S. and British suicides increase sharply just after heavily publicized suicide stories (e.g., Marilyn Monroe). He also demonstrated that the greater the publicity given to the story, the greater the increase in suicides, and that the increase in suicides comes primarily in the geographic area where the suicide story was publicized. After ruling out random, seasonal, and yearly fluctuations in the data, Phillips concluded that some people imitate publicized suicides.

In subsequent research (which appeared in this *Journal*), Phillips (1979) argued that, if motor vehicle accidents have a suicidal component, motor vehicle fatalities (MVF) should, like suicides, increase just after a publicized suicide story. Daily California MVF (1966–73) were examined before and after suicide stories and during matched control periods free from suicide stories. The data were corrected for daily, seasonal, and yearly fluctuations. The most striking finding was a 31% increase in California MVF on the third day after the publicized suicide stories. The findings suggested that publicized suicide stories triggered a rise in suicides and that some of these suicides were disguised as MVF.

If valid, these results have a number of important implications. First, if some suicides are disguised as MVF or other "accidents," the true number of suicides may be significantly larger than the recorded number. Suicides may be an even larger public health problem than previously thought. Second, the techniques used to uncover a suicidal component in MVF may be useful in determining the suicidal component in other "accidents." Thus, if accidental falls or accidental poisonings have a suicidal component, they too should increase following publicized suicides. Third, these results raise the possibility that imitation affects other forms of social behavior in addition to suicide. If this is so, it may be valuable to incorporate the concept of imitation into the general body of sociological theory. For an extended discussion of this point, see Phillips (1979, 1980).

One would not wish to dwell seriously on these and other implications unless the California findings by Phillips (1979) can be replicated. Until they are, we do not know whether the findings are limited to: (1) California, (2) the time period studied, or (3) the method of analysis used. In addition, replication is a prudent strategy because it serves as an important complement to tests of statistical significance, whose value has sometimes been questioned (see, e.g., Morrison and Henkel 1970).

The replication presented in this research note shows that Phillips's most striking finding—the third-day peak in MVF—is not limited to a

particular geographical region or time period. It also shows that the third-day peak is not an artifact of his method of analysis, because two different statistical techniques reveal its existence.

DATA

Daily Detroit mortality statistics were obtained for 1973–76 from the National Center for Health Statistics.[2] We compiled a list of all suicide stories appearing on the front pages of both the *Detroit News* and the *Detroit Free Press* (the two largest daily newspapers in Detroit). With the aid of the *Detroit News* reference library index, we obtained all front-page suicide stories from 1973 to 1976. Those stories that did not appear in all editions were dropped. This story list was then used to determine whether the same front-page story appeared in the *Detroit Free Press*. The same date, the day before, and the day after the *Detroit News'* story were checked in microfilm copies of the *Detroit Free Press*. Only those front-page stories that appeared in both papers were used. We eliminated stories that occurred during holiday periods.[3]

METHODS OF ANALYSIS

Quasi-experimental Technique

The first method used is a quasi-experimental technique similar to that used in Phillips's California study. The "experimental period" is defined as the third day after the publicized suicide, corresponding to the third-day peak found in the California data. The number of Detroit MVF in the experimental period is calculated. To determine whether the number of fatalities in the experimental period is unusually high, we compare it with the number of deaths in a matched control period. In general, the control period falls one week before the experimental period.[4] Thus, for

[2] These data were supplied by the Inter-University Consortium for Political and Social Research, University of Michigan. Data for Wayne, Oakland, and Macomb Counties were used.

[3] To correct for the confounding effects of holidays on MVF, we eliminated stories meeting either of the following two conditions: (1) if a holiday fell 0–6 days after the publicized suicide; (2) if a holiday fell 0–4 days before the publicized suicide. The application of the first condition is obviously needed to eliminate the confounding effects of holidays. The second condition is necessary because earlier studies (Phillips 1977, 1979) have shown that in general nonsuicidal MVF occur 0–4 days after the crash. This means that effects on MVF can extend approximately four days beyond the end of a holiday.

[4] The only exceptions to this are stories for which the week before contains a holiday. In this case, the first preceding week that does not contain possible holiday effects is used for the control period.

example, suppose that a publicized suicide occurred on January 20, 1974. The experimental period is three days after this (January 23), while the control period is one week before the experimental period (January 16). By using the same day of the week, the same month (or season), and the same year for the control and experimental period, the quasi-experimental technique controls daily, monthly, and yearly effects.

The publicized suicides examined in this study are presented in column 1 of table 1. The number of Detroit MVF in the experimental period three days after the publicized suicide is shown in column 2. Column 3 indicates the number of Detroit MVF in the control period, and column 4 shows the difference between the number of fatalities in the experimental period and the number in the control period. A positive number in column 4 indicates that more deaths occurred in the experimental period than one would expect from control-period mortality.

As evident from column 4, mortality in the experimental period is higher than expected.[5] The t-test for matched pairs[6] indicates that the findings in column 4 are statistically significant ($P = .012$; $t = 2.7735$, 8 df; one-tailed t-test for matched pairs). Thus, Detroit motor vehicle fatalities increase significantly on the third day after a suicide is publicized on the front page of the *Detroit Free Press* and the *Detroit News*.

The size of the increase in fatalities can be calculated as follows. The sum of the numbers in column 4 (15) is the total number of "excess" deaths on the third day. The sum of the numbers in column 3 (43) is the total number of deaths expected in the experimental period, under the null hypothesis that suicide stories have no effect on motor vehicle mortality. The number of excess deaths divided by the number of deaths expected $= 15/43 = 34.9\%$. Thus, Detroit MVF rise by about 35% on the third day after a publicized suicide. This figure is similar to the 31% increase found in California.

One might hypothesize that the third-day peak is somehow an artifact of the particular statistical techniques used in this section. This hypothesis,

[5] The procedure used in selecting control periods assumes that no significant upward trend in the daily MVFs exists. If such an upward trend did exist, the experimental period, which is in the week following the control period, would be expected to have more MVFs than the control period even if the null hypothesis were true. To explore this possibility, daily MVFs were regressed on consecutively numbered days from 1973 to 1976. The regression results are: MVF $= 4.07 + .005$ observation, with $R^2 = .006$ and $N = 1{,}457$. These results show that the practical importance of an upward trend in the data is very minor. This is indicated by the nearly zero R^2 and by the extremely small regression coefficient. An effect this small ($\beta = .005$) would not have any appreciable effects on the number of MVFs in the experimental period compared with the control period.

[6] This test is described in Hays (1963, pp. 333–35). An alternative test, requiring fewer assumptions than the t-test, is the randomization test for matched pairs (see Siegel 1956, pp. 88–92). When the randomization test is applied to the data in column 4, the results are significant at .023.

though improbable, deserves investigation. One way to test the "statistical artifact" hypothesis is to see whether the third-day peak shows up when different statistical techniques are used to analyze the data. In the next section we discuss a reanalysis of the Detroit data, using linear regression techniques.

Regression Approach

In the quasi-experimental analysis, matched control periods were used to take into account the possibly confounding effects on MVF of the day of the week, the month, and the year. Regression analysis provides an alternative technique to control for these potentially disruptive effects. In the regression analysis, variables representing the day of the week, the month, the year, and national holidays are included as explanatory factors. Not only will this strategy control for the effects of these variables, but it will also provide estimates of how much each factor affects MVF.

An additional set of variables was used to estimate the effects of publicized suicides on MVF. Lagged variables in our regression analysis enable us to estimate the delayed effects of suicide stories. For instance, a

TABLE 1

DETROIT MOTOR VEHICLE FATALITIES (MVF) ON THE THIRD DAY AFTER SUICIDE STORIES PUBLICIZED IN THE *Detroit Free Press* AND THE *Detroit News*

Name and Date of Publicized Suicide (1)	MVF in Experimental Period (2)	MVF in Control Period (3)	Difference between Experimental Period and Control Period Mortality (Col. 2—Col. 3) (4)
J. Scolaro (murder suspect) March 8, 1973	8	7	+1
E. Brudno (ex-POW) June 3, 1973	8	6	+2
S. Allende* (president of Chile) September 11, 1973	14	10	+4
S. Leisen (Detroit woman) December 14, 1973	6	2	+4
C. Chubbuck (newscaster) July 15, 1974	2	3	−1
D. Meirhofer (confessed murderer) September 29, 1974	4	4	0
N. J. Rees (spy) February 29, 1976	4	2	+2
G. Pervinkler (murder suspect) April 6, 1976	1	1	0
R. Harvel (deputy police chief) September 29, 1976	11	8	+3

NOTE.—For detailed definitions of the terms "experimental," "control," and "publicized story," see text. Motor vehicle fatalities are classified by date of death rather than by date of accident—figures are not available in any other form.

* Initial stories about Allende treated his death as a suicide, although later stories cast doubt on this interpretation of his death.

lag of one day allows an estimate of the effect on MVF of a suicide that was publicized one day earlier ($STORY_{t-1}$). On the basis of the California analysis, we hypothesize a third-day peak. That is, $STORY_{t-3}$ is expected to have a significant positive effect on MVF. However, if other lagged values of STORY are significant, this will be revealed by our regression analysis.

An initial equation specified possible effects of the publicized suicide story lagged from zero to six days as well as the daily, monthly, yearly, and holiday effects. When this equation was estimated, many of the coefficients were not significant at the .10 level or better. The equation was re-estimated, retaining only those coefficients that were significant at this level.[7]

Table 2 reports the regression estimates.[8] The regression coefficient of most interest is that of $STORY_{t-3}$. As predicted, a positive and significant coefficient results. Controlling for daily, monthly, and yearly effects, we expect a 1.83 increase in MVF three days after a publicized suicide story.

TABLE 2

MVF REGRESSED ON PUBLICIZED SUICIDE STORY, CONTROLLING FOR SIGNIFICANT DAILY, MONTHLY, AND YEARLY EFFECTS FOR DAILY DATA, 1973–76 DETROIT COUNTIES

Regressor	Regression Coefficient	t-Statistic
$Story_t$	−1.58	−1.95
$Story_{t-3}$	1.83	2.26
Friday	.92	4.95
Saturday	2.32	12.42
Sunday	1.51	8.05
June	1.11	4.68
July	.87	3.72
August	1.42	6.05
September	.80	3.35
October	.84	3.57
November	.88	3.67
1974	−.86	−4.82
1975	−.97	−5.40
1976	−.39	−2.20
Constant	3.81	24.95

NOTE.—$R^2 = .174$; $\bar{R}^2 = .166$; $F = 21.7$; df = 14, 1,439; $N = 1{,}454$.

[7] Although we coded variables to represent the effects of holidays on MVF, we were surprised to find that none of these variables had a statistically significant effect. The inclusion or exclusion of these variables makes no difference to our estimates of the effects of publicized suicides on MVF.

[8] We investigated possible residual autocorrelation by Box-Jenkins's ARIMA modeling of the residuals of the equation (see Hibbs 1973). No evidence of autocorrelation was found.

The strength of $STORY_{t-3}$'s effect (the third-day effect) can be gauged by comparing it with the other coefficients in the equation. The estimated effect of $STORY_{t-3}$ is greater than any of the monthly effects, including those of the summer months, which are traditionally the peak travel months. For instance, a day in June has 1.11 more MVF relative to the other months not included in the equation. This is less than the 1.83 increase in MVF that occurs if a suicide story is publicized three days earlier. In addition, the coefficient of $STORY_{t-3}$ is considerably greater than the day-of-the-week effects for Friday (0.92) or Sunday (1.51). The Saturday coefficient (2.32) is the only daily, monthly, or yearly effect in the equation that is greater than the estimated coefficient of $STORY_{t-3}$. Thus, the effect of $STORY_{t-3}$ is strong in comparison with the other variables.

The regression estimates are compared with the quasi-experimental estimates by calculating the percentage increase in MVF expected based on the regression results. In the previous section the quasi-experimental results indicated about a 35% increase in MVF compared with the control period. The percentage increase in MVF with the regression technique is estimated as follows:

1. The excess MVF three days after each suicide story is 1.826, and for the nine stories, 16.4. This forms the numerator of the percentage increase.

2. The expected number of MVF three days after the story is estimated using the daily, monthly, and yearly coefficients in table 1. This forms the denominator of the percentage increase.

To illustrate the calculation, consider the March 8, 1973, publicized suicide. Three days later (i.e., March 11) was a Sunday. For March 11, 1973, the expected number of MVF is formed by summing the regression constant (3.81) and the day-of-the-week effect (1.51). In this case, the yearly and monthly effects are included in the intercept. The expected MVF on March 11, 1973, was 5.32 (3.81 + 1.51). In a similar fashion, the expected MVF were calculated for the third day after the other suicide stories.

When this procedure is used, the regression-based estimate is a 40.3% increase in MVF three days after a publicized suicide.[9] This estimate is about 5% higher than the 34.9% quasi-experimental estimate. Both estimates are within 10% of the 31% third-day increase found with the California data analyzed by Phillips, so they are relatively close.

We have one unexpected result in table 2. The significant negative coefficient for $STORY_t$ was not predicted. It suggests that on the day of

[9] An alternative estimate of the "excess" MVF is to calculate the actual number of MVF three days after the story and subtract from this the expected number. This leads to an estimated 42.3% MVF increase.

the suicide a decline in MVF takes place. One possible explanation of this finding is a catharsis effect: reading or hearing about other publicized suicides provides a release that deters some potential suicides. That is, the suicidal intentions of some are lived out and defused by the publicized suicides of others. Thus a decline in suicidal MVF may follow. However, this explanation is merely speculative. A similar day-of-the-event decline does not appear in Phillips's (1977, 1979) California data. Little importance should be attributed to this decline unless it also appears in future analyses of other areas of the country.

SUMMARY AND IMPLICATIONS

Although Phillips (1977, 1979) provides evidence of suicidal MVF in California data, these results have not been replicated until now. Our analysis of Detroit metropolitan data supports the earlier finding. Both a quasi-experimental procedure and a regression approach reveal a statistically significant peak in MVF three days after a publicized suicide. This is similar to the third-day peak found by Phillips in the California data. A 35%–40% increase in MVF on the third day is estimated with the Detroit data, which is fairly close to the 31% increase found by Phillips. Thus, our results are consistent with his findings. Publicized suicides may indeed stimulate imitative suicides, some of which are disguised as MVF.

This replication raises an interesting question. Are these findings unique or are they just one example from a general category of imitative behavior?[10] Research aimed at addressing this issue might proceed along several lines by examining some of the following questions:

1. Do rape, arson, murder, and other deviant behaviors increase after publicized stories about these events?
2. Do fictional as well as nonfictional stories trigger imitative behavior?
3. Do stories in media other than newspapers (e.g., radio and television) also lead to imitative behavior?[11]
4. What social groups are the most prone to imitate? Are anomic groups more likely to imitate? Does the influence of publicized suicides differ by race, age, sex, or income?

These questions and many others remain to be answered in this largely neglected research area.

One final point should be noted. Although this paper has provided addi-

[10] Some generalization of the initial findings has already occurred. See Phillips (1978, 1980) for studies on airplane accident fatalities. Research is also in progress to determine the daily suicidal response to publicized stories.

[11] Imitation of television stories has been demonstrated in the laboratory. However, such imitation in the real world is still a matter of controversy (see, e.g., Comstock 1975, pp. 36–40).

tional evidence that imitation affects suicides, we are not claiming that imitation should serve as the sole or even the major explanatory variable in a theory of suicide. Imitation seems best suited for explaining short-term fluctuations in suicide; in contrast, traditional explanatory variables seem most appropriate for explaining long-term fluctuations. Thus, imitation and the traditional explanatory variables serve as complementary, not competitive, explanations of suicide.

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Perspectives on the Legal Order: The Capacity for Social Control[1]

Allen E. Liska
State University of New York at Albany

Joseph J. Lawrence
Central Michigan University

Michael Benson
University of Illinois

This research focuses on the size of municipal crime-control bureaucracies—police departments. The consensus perspective assumes that the legal order reflects social consensus and that the size of crime-control bureaucracies is a response to reported infractions of that order (reported crime rates). The conflict perspective assumes that the legal order reflects the interests of the powerful and that the size of crime-control bureaucracies is a response to perceived threats to such interests. Work by Turk and Blauner suggests that the size of crime-control bureaucracies reflects the relative size of groups dissimilar to authorities and the extent to which such groups are segregated. The above perspectives are tested for 109 U.S. municipal police departments from 1950 to 1972. Empirical support for the conflict perspective is strongest (1) in the South and (2) after the civil disorders of the 1960s.

Drawing on the consensus and conflict perspectives, this research focuses on the size of municipal police bureaucracies—an extremely significant component of the legal order underlying its capacity to deal with people and acts. Since the 1960s, crime-control bureaucracies have increased dramatically in the United States. The Center for Research on Criminal Justice (1975) reports that the proportion of the GNP spent on the criminal justice system increased from .5% in 1955 to 1% in 1971. This is clearly reflected in municipal police size. In 1940 American cities employed 1.7 police officers per 1,000 population, a figure which had remained relatively stable since the turn of the century; that figure grew to 1.9 by 1955, 2.3 by 1965, and 2.5 by 1975.

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According to the consensus perspective, laws reflect a value consensus, and law enforcement is a response to reported law violations. While only partly reflecting the true rate, the level of crime publicized in the media is the socially recognized crime rate. Thus, although people and authorities may use various sources of information in forming an image of the crime problem, the recorded crime rate probably plays a major role in that image and is probably a major source of its systematic variation among cities. Hence, the consensus perspective suggests that municipal police size should generally reflect the reported crime rate.

Most studies report a positive relationship between reported crime rates and police size (e.g., Swimmer 1974; Jones 1974; Land and Felson 1976). These results, however, must be interpreted cautiously. While they may reflect the effect of reported crime rates on police size, they may also reflect the effect of police size on reported crime rates. Most studies do not control for the effect of police size on reported crime rates in estimating the effect of these rates on police size, and the few studies which address this problem (e.g., Swimmer 1974), using two-stage, least-squares estimating techniques, make questionable assumptions about the instrumental variables used to identify the equations (Nagin 1978).

The conflict perspective does not deny that law infractions are a threat to authorities, but its emphasis on social conflict suggests a different thrust. Rather than emphasizing the concern of authorities to control those actions that violate the law, the conflict perspective emphasizes the concern of authorities to control those groups that threaten their interests (Quinney 1977; Turk 1969). This paper deals with two specific hypotheses which stem from the conflict perspective, regarding cultural/racial dissimilarity and segregation.

Cultural and racial dissimilarity.—Turk (1969) argues that culturally dissimilar subordinate groups are perceived by authorities as threatening to the social-political order, and that considerable variation in law formulation and enforcement can be understood as an effort to control such groups. Blauner (1972) states this thesis more forcefully, emphasizing the importance of racial as well as cultural dissimilarities, and arguing that police function as an urban army supporting "domestic colonialism." Indeed, while a relatively small culturally dissimilar subordinate group may not be perceived as posing much of a threat, a relatively large culturally dissimilar group (composing 20%–30% of the population) which is also racially dissimilar may be perceived as a substantial threat to the social order and as posing a substantial problem of social control.[2] Hence, we

[2] Racial groups are distinguished by socially selected physical traits, and ethnic or cultural groups are distinguished by socially selected cultural traits (Wilson 1973). The classification of a particular subpopulation as either a racial or a subcultural group depends on the definitions and beliefs of the society.

might expect a strong relationship between the relative size of such groups and the size of the social control apparatus.[3]

In American society nonwhites constitute a culturally and racially dissimilar subordinate group and are frequently viewed by whites and authorities as a criminal threat. For example, Swigert and Farrell (1976) report that whites and authorities hold criminal stereotypes of nonwhites and their life-styles, and Lizotte and Bordua (1980) report that whites perceive the proportion of nonwhites in a neighborhood as an indicator of the crime problem. Additionally, given the cultural and racial dissimilarity between whites and nonwhites, interracial victimization is likely to be perceived by whites as more dangerous and threatening than intraracial victimization. The percentage of nonwhites in a political unit may then directly affect whites' perception of the crime problem, and by influencing the level of interracial victimization it may also affect the level of fear which whites associate with victimization. Assuming that whites are able to translate their perceived threats and fears into public policy, a specific hypothesis regarding police size follows: the percentage of nonwhites, by influencing the perceived magnitude of crime, may influence police size independently of the crime rate and, by influencing the threat and fear associated with crime, may influence the effect of the crime rate on police size.

Limited support for this hypothesis is available. Studies by Huff and Stahure (1980), Jacobs (1979), and Carroll and Jackson (1979) report a moderate relationship between percentage black[4] and police size, controlling for crime rates and other variables. However, there are several problems with these studies. First, they do not estimate the effects of percentage black for different geographical regions. Given the historical and structural differences among regions, there is no reason to expect the effect of percentage black to be similar across regions. Second, Huff and Stahure's (1980) study is limited to northeastern suburbs, where the percentage black is generally low; they also estimate the effect of percentage black controlling for either property or personal crime but not for both simultaneously. Third, Jacobs (1979) and Carroll and Jackson (1979) do not disentangle the reciprocal effects between crime rates and police size, thereby possibly underestimating the effect of crime rates, relative to the effect

[3] For democratic political orders this relationship is probably curvilinear, rather than linear. As culturally and racially dissimilar subordinate groups exceed 50% of the population, they may gain control of the government.

[4] Given that people with Hispanic surnames are classified as white in the U.S. census, percentage nonwhite used in our study and percentage black used in these studies yield similar results. In our sample in 1970 the correlation between percentage nonwhite and percentage black is .98 in the non-South and .99 in the South. We used percentage nonwhite, rather than percentage black, because the latter is not available for all cities before 1970. Future research should examine the threat associated with particular racially and culturally dissimilar groups, as some research (e.g., Kornblum 1974) suggests that some dissimilar groups are more accepted by whites than are blacks.

of percentage black, on police size. Finally, none of the studies estimate the interaction effects of percentage black and crime rates on police size.

Segregation.—Blauner (1972) argues that the segregation of threatening populations into urban ghettos functions as a vehicle of social control, thereby reducing the need for formal social controls. Specifically, racial segregation, by reducing interracial interaction and possibly interracial victimization as well, may reduce whites' perception of the magnitude and seriousness of the crime problem. Hence, we hypothesize that, by moderating whites' perception of the crime problem, racial segregation may inversely influence police size, and by moderating the threat associated with nonwhites and the crime rate, racial segregation may moderate their influence on police size. (A relatively large culturally and racially dissimilar subordinate population may be perceived as less threatening if it is segregated; and a relatively high crime rate may be perceived as less threatening if the threatening population is segregated.)

In sum, the consensus perspective assumes that laws reflect a value consensus and conceptualizes law enforcement (municipal police size) as a response to reported law violations. The conflict perspective conceptualizes the legal order as reflecting the interests of the powerful, and law enforcement (municipal police size) is viewed as an effort to control threatening populations. Although the perspectives are frequently thought of in absolute terms, it may be advantageous to treat them as relatively applicable and to examine the conditions upon which their relative usefulness is contingent. As racial conflict has traditionally been more prevalent in the South than nonsouthern regions, the conflict perspective (with regard to race) may be more applicable in southern than nonsouthern cities; and because racial conflict increased during the 1960s, the conflict perspective may be more applicable in all geographical regions after the mid-1960s.

PROCEDURES

The consensus and conflict perspectives are tested with data from 109 U.S. cities from 1950 to 1970 at five-year intervals. These cities were selected because their residential segregation levels have been calculated since 1940, making historical comparisons possible. As of 1970 all but one city had a population of 50,000 or more, and most of the largest cities in the country are included.

Police size was measured as the total number of police department employees per capita (per 10,000 population) and was ascertained from the *Municipal Yearbook* and the *Uniform Crime Reports*. Civilian clerical employees were included in the measure, because they free uniformed personnel for the task of social control.

Two patterns of reported law violations are examined: index crimes and

civil disorders. Index crime rates were ascertained from the *Uniform Crime Reports*. Following previous research, the seven serious index crimes were used, categorized into personal crime (homicide, rape, and aggravated assault) and property crime (robbery, burglary, larceny over $50, and automobile theft.)[5] Categorizing these crimes into personal and property crimes minimizes inconsistencies among police departments in classifying specific crimes. Data on civil disorders in 44 of the sampled cities are available from the *Staff Study of Major Riots and Civil Disorders* (U.S. Senate 1968) and the *Report* of the U.S. National Advisory Commission on Civil Disorders (1968). Using the commission's criteria, cities were classified by the severity of the major disorders for each year, and a cumulative index of disorders from 1963 to 1970 was constructed.

For 1950, 1960, and 1970 percentage nonwhite was collected from the U.S. census, and city segregation was taken from published reports (Sørensen, Taeuber, and Hollingsworth 1975). Estimates for the intervening years were made using a straight linear interpolation equation.

Finally, city population size is included as a control variable. The urbanism thesis, developed by Wirth (1938) and critically evaluated by Fischer (1976), suggests that population size is associated with social and cultural heterogeneity, depersonalization, and alienation, as well as with increased reliance on formal means of social control. Because population size is probably also related to crime rates and racial composition, its omission from the analysis might bias estimates of the effects of these variables on police size.

Because of time lags involved in budget decisions, the causal variables were measured two years before the years for which police size was measured. Jones (1974) has argued that budget decisions are normally made within a three-month period preceding the budget's effective date. The 1967 budget, for example, reflects decisions made sometime in late 1966, which can only be based on information available for 1965 and before. Thus, police size is measured in 1952, 1957, 1962, 1967, and 1972, and the causal variables are measured in 1950, 1955, 1960, 1965, and 1970.

A test of the perspectives requires that the effects of crime rates (property and personal), racial composition (percentage nonwhite and segregation), and population size on municipal police size be estimated simultaneously. Ordinary least-squares regression is used to estimate the independent and interaction effects.[6] Because of cultural and social differences between the South and non-South, the equations are estimated by region.

[5] While robbery involves the implicit or explicit threat of violence, it is more like property than personal crime, because it is directed toward acquiring property, not violence, and because of the anonymity between the offender and the victim.

[6] Multicollinearity is minimal. Except for the non-South in 1962, the correlation between any two independent variables does not exceed .45.

Standardized coefficients are used to compare the effects of different variables for a particular year and region, and unstandardized coefficients are used to compare the effects of the same variable across regions and over time.

RESULTS

The findings are presented as follows. First, the effects of the causal variables (lagged by two years) on police size are examined in a cross-sectional analysis (table 1). Although this may yield biased estimates of the effect of the crime rate, it is presented to facilitate comparisons with past research which did not use panel data; additionally, as police size probably does not affect racial composition, the analysis should yield reasonably unbiased estimates of the effect of racial composition on police size. Second, the data are examined for interaction effects. Third, a cross-lag analysis is used to control for the effect of police size on the crime rate and to partially control for the possible effect of unmeasured variables on police size (table 2). Finally, the underlying assumption of this research—that there is a link between the racial composition of a community and its fear of crime—is examined.

Structural (cross-sectional) analysis.—Consider reported crime rates (table 1). The effect of personal crime is not statistically significant for any year in either region, and the direction of the betas is inconsistent; however, the effect of property crime, while not always substantial, is always in the predicted direction and is statistically significant for four of the 10 estimates. Its effect is particularly evident in the non-South.[7]

The effect of percentage nonwhite depends on geographical region and year. It appears to follow the regional pattern of civil rights and protest activities in the 1950s and 1960s. In the South civil rights activities began in the late 1950s. Our data show a dramatic increase in the effect of percentage nonwhite in 1962, which progressively increases, reaching a beta of .57 for 1972. In the non-South, civil rights protests did not occur with similar intensity until the middle to late 1960s. Again, table 1 reveals a substantial increase in the effect of percentage nonwhite in subsequent years. This pattern of relationships is clearly evident in the unstandardized coefficients and suggests that the civil rights protests and urban disorders created or dramatically accentuated the perception that nonwhites represent a threat to the stability of the social order.

Perhaps the urban disorders themselves stimulated efforts to increase

[7] Because reported property and personal crime are correlated (as high as .60 in 1962), the equations were also estimated combining property and personal crime into one index. These estimates are similar to the previous ones, with the beta for the crime index somewhat lower than the beta for property crime.

TABLE 1

CROSS-SECTIONAL ANALYSIS: UNSTANDARDIZED (*B*) AND STANDARDIZED (Beta) COEFFICIENTS FOR POLICE SIZE BY YEAR AND REGION

	1952		1957		1962		1967		1972	
	B	Beta	*B*	Beta	*B*	Beta	*B*	Beta	*B*	Beta
South ($N=44$):										
Population	.008	.36*	.008	.32*	.009	.35*	.005	.22	.005	.13
Segregation	−.071	−.08	−.102	−.09	−.301	−.22	−.370	−.30*	−.530	−.30*
Nonwhite (%)	.097	.25	.132	.28*	.191	.38*	.212	.44*	.402	.57*
Property crime	.053	.04	.327	.28*	.110	.10	.228	.24	.117	.15
Personal crime	.572	.23	.167	.03	−.048	−.01	−.042	−.01	.818	.16
R^2	.28*	...	.32*	...	.38*	...	.49*	...	.62*	...
Non-South ($N=65$):										
Population	.003	.33*	.002	.28*	.001	.18	.002	.33*	.002	.27*
Segregation	−.307	−.28*	−.210	−.18	−.122	−.10	−.036	−.03	.177	.15
Nonwhite (%)	.081	.10	.040	.04	.034	−.03	.111	.16	.218	.31*
Property crime	.070	.03	.479	.29*	.221	.19	.254	.27*	.311	.52*
Personal crime	.595	.06	1.32	.12	2.78	.33	.514	.08	−1.41	−.23
R^2	.16	...	.28*	...	.29*	...	.31*	...	.44*	...

* Significant at the .05 level.



police size; and if percentage nonwhite is related to the severity of civil disorders (Spilerman 1976), the effect of the latter may account for the observed percentage nonwhite effect. The 1972 equation was estimated for the 44 cities (19 southern and 25 nonsouthern) for which civil disorder data were collected. It shows only a minimal effect (betas of .03 and .08 for the non-South and South, respectively), and including it in the analysis does not alter the estimated effects of percentage nonwhite or any of the other variables. Although this analysis is limited to less than 50% of the sampled cities, the findings are very similar to those of Jacobs (1979) and Carroll and Jackson (1979).

The segregation effect also depends on geographical region and year. For the South, the betas and unstandardized coefficients are consistently negative, and, as in the case of percentage nonwhite, they steadily increase after 1962, following the start of the civil rights protests. For the non-South, however, the betas are statistically insignificant after the mid-1950s, suggesting that segregation may no longer be perceived by authorities in the non-South as a mechanism of social control.

Can the racial composition findings be explained by still other variables? It might be argued that the demand for non–crime related police services is influenced by the percentage of the population in poverty and therefore unable to purchase such services. As this percentage is related to the racial composition of a city, particularly the percentage of nonwhites, some portion of the observed racial composition effect on police size may be accounted for as demand for non–crime related services. To examine this interpretation, we reestimated the equations, controlling for the effect of the percentage of families below the poverty line. Although these data were collected for 1970 only, this is the time at which the racial composition effect is strongest. The analysis shows that, although the poverty rate is substantially related to racial composition, it is not related to police size; including it in the analysis does not alter the effects of racial composition and reported crime rates on police size in either region. Further, controlling for the effect of the percentage of just nonwhite families below the poverty line also does not alter the results.

In sum, the effect of racial composition is greater in the South than in the non-South, although in both regions the effect of percentage nonwhite dramatically increased after the civil disorders starting in the late 1950s in the South and in the late 1960s in the non-South. These effects are observed when the effects of reported crime rates, city size, and the proportion of the population which is poor (as indicators of the demand for crime and non–crime related police services) are controlled.

Interaction effects.—The conflict perspective suggests that the effects of racial composition and reported crime rates may not be independent. The effect of reported crime rates (property and personal) on police size may

depend in part on racial composition (percentage nonwhite and segregation), and the effect of percentage nonwhite on police size may depend in part on segregation. To test for such effects, five interaction terms were constructed as multiplicative products of the appropriate variables (percentage nonwhite × reported property crime, percentage nonwhite × reported personal crime, segregation × percentage nonwhite, segregation × reported property crime, and segregation × reported personal crime). With the interaction terms in the regression equations, the levels of multicollinearity are sufficiently high to make the beta estimates unreliable. Thus, the interaction hypotheses are tested by first entering the main terms into the regression equations, and then examining the increments in the R^2 made by the interaction terms using a stepwise procedure.

For the non-South the increments to the R^2 are inconsistent, substantially significant in some years but not in others. For the South, a consistent pattern is shown. The following interaction terms add substantial and statistically significant increments to the R^2: for 1952 and 1957, segregation × property crime increases the variance explained from 28% to 40% and from 32% to 42%, respectively; for 1962 segregation × property crime and segregation × percentage nonwhite increase the variance explained from 38% to 60%; for 1967 segregation × personal crime and segregation × percentage nonwhite increase the variance explained from 49% to 64%; and for 1972 segregation × property crime, percentage nonwhite × personal crime, and percentage nonwhite × property crime increase the variance explained from 63% to 84%. Two patterns are clear. Segregation × property crime is the major interaction term, significantly increasing the variance explained in 1952, 1957, 1962, and 1972; and the interaction effects increase from 1952 to 1972, so that by 1972 three of the five interaction terms make significant contributions to the variance explained.

While increases in the R^2 reveal interaction effects, they do not reveal the direction of the effects. To observe directionality, we examined the betas in the above equations and reestimated the equations separately for cities with a high and a low level of segregation. Consistent with the conflict perspective, the betas show that percentage nonwhite has the strongest effect when segregation is lowest both before and after the civil disorders, and that reported property crime rates have the strongest effect when segregation is lowest, but only after the civil disorders (before the civil disorders, the direction of the effect is inconsistent). Generally, the data suggest that a high level of segregation minimizes the effects of percentage nonwhite and property crime rates on police size in the South, particularly after the civil disorders of the 1960s.

Dynamic (cross-lag) analysis.—The cross-sectional analysis does not isolate the effect of reported crime rates on police size from the effect of

police size on reported crime rates, hence it may yield biased estimates of the former effect. Additionally, if unmeasured variables which affect police size are correlated with reported crime rates and racial composition, the analysis may also yield biased estimates of the effects of both reported crime rates and racial composition. A modified cross-lag analysis (Kenny 1979) provides one approach to both problems. The procedure estimates the effect of reported crime rates and racial composition (time 1) on police size (time 2) with police size (time 1) in the equation, thereby controlling for the effect of police size on reported crime rates, and partly controlling for the effects of unmeasured variables on police size (time 2).[8] Statistically, a residualized measure of police size (time 2), representing a change in police size between $time_1$ and $time_2$, is regressed on the causal variables. Because we were uncertain about the proper time lags, the equations were estimated using various lags.

The coefficients yielded by the equations with police size lagged by seven years (table 2) suggest that the cross-sectional coefficients overestimate the effects of reported crime rates and racial composition on police size but correctly estimate the general pattern of effects. For the non-South, the percentage nonwhite effect is statistically significant only after

TABLE 2

CROSS-LAG ANALYSIS: UNSTANDARDIZED (*B*) AND STANDARDIZED (Beta) COEFFICIENTS FOR POLICE SIZE BY YEAR AND REGION

	1957		1962		1967		1972	
	B	Beta	*B*	Beta	*B*	Beta	*B*	Beta
South ($N=44$):								
Population	.003	.14	.004	.15*	.001	.06	.002	.06
Segregation	−.006	−.00	−.012	−.00	−.200	−.16	−.345	−.20*
Nonwhite (%)	.050	.11	−.004	−.00	.083	.17	.244	.34*
Property crime	.047	.04	−.011	−.01	.060	.06	−.012	−.01
Personal crime	−.425	−.10	−.070	−.01	−.153	−.04	−.032	−.00
Lagged police size	1.09	.76*	1.10	.89*	.70	.68*	1.01	.63*
Non-South ($N=65$):								
Population	.0006	.09	.0002	.03	.014	.18*	.004	.04
Segregation	.016	.01	.129	.11*	−.036	−.03	.084	.07
Nonwhite (%)	−.070	−.07	−.064	−.07	.142	.21*	.118	.16*
Property crime	.284	.17	.230	.20*	.052	.05	.176	.29*
Personal crime	−.326	−.03	−.115	−.01	−.473	−.08	−1.00	−.16
Lagged police size	.90	.75*	1.11	.88*	.71	.79*	.870	.67*

* Significant at the .05 level.

[8] For the above model, a lag measure of police size is a suitable proxy for unmeasured variables when (1) unmeasured variables have influenced police size in the past but no longer do so, or (2) unmeasured variables continue to influence police size but the causal structure is stationary within the lag time period and the values of the unmeasured variables do not change faster than those of the measured causal variables. The latter condition appears to be met by our data. See Kenny (1979) for a general discussion of these issues.

the civil disorders in 1967 and 1972, and a segregation effect is not shown. For the South, the segregation and percentage nonwhite effects are statistically significant in 1972 and approach significance in 1967. If different time lags are used, the estimates change somewhat; however, all analyses show substantial percentage nonwhite and segregation effects for the South in 1967 and 1972 and substantial percentage nonwhite effects for the non-South in 1967 and 1972.

Generally, the cross-lag analysis suggests that before the civil disorders the cross-sectional coefficients may be explained by unmeasured structural and historical forces. However, after the civil disorders police size and variability among cities increased substantially; that is, some departments expanded considerably more than others. This expansion cannot be completely explained by the same structural forces that explain earlier levels of police size, as indicated by the only moderate betas of 1972 and 1967 police size on lagged police size compared with the very strong betas of 1957 and 1962 police size on lagged police size. In the non-South, the expansion in police size is best predicted by reported property crime rates and percentage nonwhite and, in the South, by percentage nonwhite and segregation.

Fear of crime.—An underlying assumption of this research is that nonwhites are frequently perceived by whites as a criminal threat, and that this perception is affected by the racial composition of a community. Support for the first part of this assumption has already been noted (Swigert and Farrell 1976); the second part can be tested by examining the extent to which the racial composition of a community affects whites' fear of crime.

Fear of crime data aggregated for a large number of cities is available only in the National Crime Survey of 26 cities. During 1972 and 1973, 10,000 respondents from each city were asked to what extent they fear to walk at night in their neighborhood.[9] As all of the cities in the survey are included in our sample, we merged the data on fear with our data set and examined the extent to which fear of crime for whites is affected by racial composition, reported crime rates, and population size of a city. A regression analysis shows that only percentage nonwhite has a statistically significant effect (beta = .49), although the property crime effect is also substantial (beta = .47). Further, we have argued that percentage nonwhite affects fear in part because it affects the level of interracial victimization. For robbery, as the victim can generally identify the race of the offender, we computed the ratio of white victimizations which are interracial for each city. The data indicate that, as percentage nonwhite increases, this ratio increases ($r = .75$). When added to the fear equation, the ratio

[9] Data for 13 of the 26 cities are available in Garofalo (1977), and data for the other 13 cities were kindly supplied by James Garofalo.

shows a beta of .50, and the percentage nonwhite beta drops from .49 to .08, implying that the effect of percentage nonwhite on fear is mediated by interracial victimization. These findings suggest that the fear of whites to walk the streets at night is strongly affected by the racial composition of a community, thus providing substantial support for a major assumption underlying our interpretation of the findings on racial composition and police size.

DISCUSSION

In sum, the effect of racial composition on police size depends on geographical region and year (before or after the civil disorders). The analysis suggests that before the civil disorders the observed racial composition effects in the South may be explained by unmeasured variables, but that after the civil disorders the substantial increases in police size which occurred in both regions cannot be so explained. They appear to have been substantially influenced by racial composition. These effects are observed after controlling for reported crime rates, population size, and percentage poor as indicators of the demand for crime control and non–crime related police services. The interaction effects show a similar pattern. They are more distinct in the South and consistently conform to the conflict hypotheses only after the civil disorders. Finally, the analysis provides support for an assumption underlying our interpretation of these findings—that the racial composition of a city affects whites' perception of the crime problem.

An important question remains: Why does segregation decrease police size directly and moderate the effects of reported property crime and percentage nonwhite on police size in the South but not in the non-South? The answer may lie in the pattern of segregation. In many parts of the South, nonwhites reside in small segregated pockets scattered throughout rural, suburban, and central city areas (a remnant of the "back-alley" pattern), rather than being concentrated in one or two large urban ghettos. This pattern of residential segregation may be a more effective means of social control (thereby reducing the threat associated with crime and nonwhites) than the urban ghettos of the non-South and the "new" South.

To conclude, while historical studies (Silver 1974) have noted the role of social conflict in the development of urban police, they have neither estimated the strength of this effect nor isolated it from the effect of reported crime. We have attempted to estimate the independent and interaction effects of racial composition and reported crime from 1952 to 1972 by geographical region. The data reveal that the dramatic increase in police size during the late 1960s and early 1970s cannot be explained in terms of reported crime rates alone. These increases appear to have been in-

fluenced by the relative size of culturally and racially dissimilar groups associated with street crime (as suggested in Turk's theory), and in the South by the extent to which such groups are segregated (as suggested in Blauner's work). In part, the expansion of police size at that time can be interpreted as an attempt to control these groups.

The support which these findings provide for the conflict perspective on social control does not clearly extend to the orthodox Marxian version of the perspective. For when the effects of racial and class (percentage poor) composition of a city are estimated simultaneously, only the effects of the former are observed. Percentage poor (which we interpreted as an index of the demand for non–crime related police services, but which can also be interpreted as an index of the surplus and thus threatening population) shows no effect on police size. Hence, the analysis suggests that the expansion of police size after the civil disorders was an effort more to control a culturally and racially dissimilar population which whites associate with street crime, than to manage economic conflict.

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Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

NETWORKS OF OCCULTISTS: COMMENT ON STARK AND BAINBRIDGE

Stark and Bainbridge ("Networks of Faith," *AJS* 85 [May 1980]: 1376–95) argue, in connection with a proposed synthesis and empirical examination of deprivation and social network models of recruitment to religious groups, that "the occult milieu resembles a mass audience more than a real subcultural phenomenon" (p. 1392). Unfortunately this view distorts the reality of the occult in America today. The authors fail to consider different dimensions of the occult, they misinterpret some of the studies they review, and they generalize too quickly from very limited data.

The central difficulty with this article resides in the peculiar manner in which the occult is conceptualized and related to other marginal beliefs. The occult is defined operationally, in terms of "a number of occult items (belief in ESP, tarot cards, seances, psychic healing, and astrology, or a liking for occult literature)" in a questionnaire administered to "424 *pairs* of close friends selected from the student body of the University of Washington" (p. 1391). This operational strategy assumes that the types and especially the degrees of occult belief relevant to the hypotheses are represented in the sample. The literature, however, provides little support for this position.

In two articles ignored by the authors, Truzzi (1972, 1974) argues for a multidimensional view of the occult. While Stark and Bainbridge seem

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to recognize in their definition that "the occult" is commonly used with reference to various bodies of knowledge (astrology, ESP, spiritualism, etc.), they also use the word as if it somehow referred to a unified phenomenon distinct from satanism, scientology, Eastern beliefs, or any other set of esoteric doctrines. I have expanded Truzzi's notion (see Jorgensen 1978, 1979) by proposing several basic categories of involvement with esoteric beliefs: *popular* participation, *casual* participation, *devoted* participation, and *exclusive* participation (see Balch and Taylor 1977; Damrell 1977, 1978; Lynch 1977, 1980; Scott 1976, 1980; and Tiryakian 1974 for supporting evidence). These categories are not absolute but fluid and interconnected. Much empirical work remains to be done, yet rather distinctive types of participants—commonly bounded by organizational relationships—are observable in most large urban areas.

The ambiguity of Stark and Bainbridge's conception of the occult creates many problems. Bainbridge's (1978) study of a satanic cult and Lynch's (1977, 1980) reports on a pervasively occult group are used as examples of interpersonal bonds in the early portion of the article, but these are disregarded in arriving at the conclusion that the occult lacks cohesive relationships. Although the authors acknowledge Balch and Taylor's (1977) ethnography of a UFO cult and recognize the role of the cultic milieu in the conversion process, they misunderstand and misrepresent it. Balch and Taylor clearly note that members of the cult possessed lengthy histories of occult and cultic involvement. Balch (1979), moreover, stresses the importance of this network, especially friendships, for the conversion experience. Lynch's (1977, 1980) work supports the same contention. Indeed, Lynch's model of conversion makes special mention of the supporting "occult community."

I have reported (Jorgensen and Jorgensen 1977; Jorgensen 1978, 1979, in press) on extensive and elaborate networks of occult practitioners, including what informants called the "esoteric community." Uncovered during an investigation of the occult tarot, this "community" involved 5,000–15,000 people (depending on who gets counted) and over 100 different groups (ranging from small study groups, quasi-religious organizations, and churches to medical clinics and business enterprises). Among members, most of whom are middle aged, linkages include business relations, associations of practitioners and groups, friendships, and common sources of information and communication—newsletters, fliers, directories of members and groups, magazines, and so on. Once or twice a year leaders of the community sponsor a "psychic fair" at which members make new friends, renew old friendships, exchange trade secrets, make money, present images to the general public, and recruit new members. Hartman (1976) provides further evidence of formal and informal networks, many of which cross local, regional, and maybe even national boundaries. Her paper

reports on a survey of "witches, astrologers, Eastern mystics, palmists, serious magicians, spiritualists, parapsychologists and perhaps some Satanists . . . from all over the U.S. [who attended] the Gnostic Aquarian Festival" (p. 169).

Two contentions in particular—that the occult "does not appear to spread through or be sustained by social networks," and that "cult movements find the occult milieu a useful resource of recruits. But, since occult beliefs seem not to spread through social networks, finding a recruit on the basis of his or her prior dabbling in the occult does not give a cult group access to a social network of people similarly predisposed" (p. 1392)—simply are not supported by the facts. This is not surprising since the authors' sample fails to represent the kind of people (and groups) likely to be involved with the occult in a measurable degree.

The deprivation thesis is perhaps more a matter of perspective than a testable proposition. The network model of conversion, however, is subject to empirical demonstration. Such an investigation and analysis necessarily depend on an empirically grounded conception of the esoteric scene, especially the complex interconnections and interrelationships among the occult and other marginal beliefs, their adherents, and organizational networks. This matter simply cannot be decided on the basis of the Stark and Bainbridge article. Its defects suggest that, while quantitative data may be desirable, quantification is manifestly meaningless without qualitative understanding.

DANNY L. JORGENSEN

University of South Florida

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REPLY TO JORGENSEN

As is the custom among bears in the Pacific Northwest, each of us had retired to his cave by November, prepared to live off 20 reams of typing paper gathered during a summer of abundance. But this year our peace was disturbed by a fugitive parakeet who flew all the way from Florida to coach us in the art of growling. We tried to explain that the blessings of both genetics and a favorable environment had made us world-class growlers. But the bird persisted. In order to restore our tranquillity we find it necessary to demonstrate our capacity to growl, as follows.

We understand Jorgensen's desire to get something published in the *AJS*. But he must have known that to do so he had to devote his comment to the wrong paper and send it to the wrong journal. As was clear in our essay, the questions he raises pertain to matters on which we have written often, at length, but elsewhere. We think we have solved Jorgensen's problems more than adequately in those other papers. If not, his objections might appropriately have been directed to the primary sources, not to passing secondary mentions.

For example, had he consulted our paper reconceptualizing sect and cult (Stark and Bainbridge 1979), he would have found that we distinguish three levels of cult activity. *Cult movements* are fully developed religions in a deviant religious tradition. *Client cults* dispense magical services (e.g., those of astrologers, psychics, healers, water dowsers). Often there is some degree of organization among client-cult practitioners, but usually there is little organization among their clients. *Audience cults* deal in myth, weak magic, and esoteric entertainment. Audience cults operate primarily through the mass media but sometimes attract crowds of consumers to lectures, fairs, and the like. Audience cults are not organizations.

Thus we have not only demonstrated disciplined regard for the diversity of cult activities; in addition, we have written several empirical papers

to test the utility of our conceptual scheme (Stark, Bainbridge, and Doyle 1979; Bainbridge and Stark 1980*a*, 1980*b*). Hence for Jorgensen to charge that we lack a multidimensional appreciation of the occult scene is simply a statement about what he has not read.

It is true that we have not read his work either. We are not contrite. Indeed, we think it merely presumptuous of him to write "I have reported," followed by four citations, when in fact only one of the works listed has been accepted for publication. There must be thousands of additional unpublished papers we have not read. Of course, merely having been published does not guarantee that our reconceptualization of "cult" is adequate. If not, Jorgensen ought to send comments to the appropriate journals. They are not so widely read as the *AJS,* but we are sure his vita would not be damaged thereby.

Our *AJS* discussion of the role of networks in recruitment to sects and cults was devoted primarily to cult *movements.* There was no occasion in that paper to mention our conclusion that networks probably also play a role in securing clients for client-cult practitioners. Nor did we repeat our lengthy analysis of how intense interaction among clients, even to the point of a "social implosion," often causes client cults to evolve into fully developed cult movements (Bainbridge 1978; Bainbridge and Stark 1979). Thus Jorgensen's emphasis on the presence of networks in client cults adds support to conclusions we already have published and is mainly irrelevant to the only interesting basis for dispute between us. That concerns the question, What impact does participation in audience cults have on the probability of recruitment by a cult movement? We did not dismiss such participation as of no importance. We noted the excellent field studies by Balch and Taylor (1977) and by Lynch (1977) which suggest that dabbling in audience cults may provide ideological preparation for joining a cult movement. Indeed, we agree that people who exhibit interest in novel mysticisms might be preparing themselves to embrace a novel religious movement, should they encounter one, or indeed that such dabbling might be prima facie evidence of religious seeking and an expression of discontent with conventional faiths. Instead of dismissing these notions, we emphasized a different question: Are audience cults sustained by social networks, and thus, if a cult movement gains a recruit from this audience, does this give the cult access to a new social network through which additional recruits may be gained?

We suggested that the answer to this question is no. In doing so we devoted less than a page in our *AJS* article to reporting briefly on research that has since been presented fully in two other articles (Bainbridge and Stark 1981, in press). We found that audience-cult beliefs and interests play no role in friendship patterns, while interests in client cults and cult movements do show network effects. Finding no concordance among student

friendship pairs on belief in such things as seances, tarot cards, psychic healing, and the like (while finding high levels of concordance on evangelical Christianity), we conclude that people are not selecting friends who share their attitudes toward audience cults and are not influencing their friends on these matters. If this is so, it is hard to describe audience cults as subcultures or social networks.

Granted that our sample was restricted to university students. Still, we did not lack respondents who accepted various occult notions or expressed considerable interest in various audience-cult phenomena. What we lacked was any sign that such beliefs have implications for interaction among close friends. If they do not, it seems that to gain a convert with a background in audience cults does not present cult movements with access to a network of like-minded potential recruits. These findings seemed worth mentioning in our *AJS* paper. If we have been fooled by faulty data or inappropriate analysis, we hope to be corrected. But presumably that will require a critic to examine the studies in question.

Jorgensen's harping on the network character of *some* aspects of cult phenomena is correct, but it is not relevant to our discussion of audience cults. We know people gather to hear all manner of occult lectures and that psychic fairs, UFO conventions, and the like are commonplace. We have been to many such affairs. But it is important to recognize that such events are usually a mixture of client-cult practitioners and an unorganized audience, some of whom are clients of the practitioners but many of whom are merely occult consumers. As noted, often there is considerable organization among client-cult practitioners—they even form trade associations and professional societies. Moreover, many occult gatherings are attended almost exclusively by such practitioners, and it is here that Hartman's (1976) work, cited against us by Jorgensen, is pertinent. We were arguing only that their *clients* usually are not organized; indeed, we seem to have been the first to analyze the process by which the formation of a social network of clients leads to the evolution of client cults into cult movements. More to the point, much of the current interest in and belief in the occult found in the general public does not seem to be a network phenomenon.

For these reasons we suggested only that there are limits to networks of faith and that audience cults lie beyond these limits. Thus they do not represent the first stages in cult formation or the first step by the individual toward cult recruitment. We may well be wrong about this. If so, we hope to learn better soon. But it will require some data, not just casual impressions and praise of "qualitative understanding" to convince us.

RODNEY STARK
WILLIAM SIMS BAINBRIDGE

University of Washington

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WHICH NORMS OF JUSTICE? A COMMENTARY ON HAMILTON AND RYTINA

The article by Hamilton and Rytina in a recent issue of this *Journal* ("Social Consensus on Norms of Justice: Should the Punishment Fit the Crime?" *AJS* 85 [March 1980]: 1117–44) contains some misleading inferences about the nature of the justice norm examined in that article. By demonstrating that respondents fit the severity of the punishment to the seriousness of the crime, Hamilton and Rytina do indeed document a norm of justice: "The punishment should fit the crime." But, contrary to statements by the authors, the use of that principle by respondents in no way indicates that respondents rely on the notion of "just deserts," if that phrase is used in its conventional sense to denote *retributive justice*. That is the case because the notion that "the punishment should fit the crime" is not unique to retributive theories of punishment (indeed, the maxim is usually associated with Beccaria's call for a utilitarian rationale for punishment—deterrence) and, more important, because the public may view the seriousness of crimes as the appropriate criterion for fixing punishments, but for reasons having nothing to do with retribution.

To illustrate, suppose that in assigning punishments, respondents have deterrence in mind as the sole goal of punishment. And suppose that they employ a commonsensical theory of deterrence: the greater the seriousness

of the offense, the more severe the penalty necessary to achieve deterrence (either general or specific). In this case, their recommended punishments would correlate positively with the seriousness of the crimes, but the rationale would be deterrence, not retribution. Similarly, consider the possibility that respondents hold incapacitation to be the sole goal of punishment. And suppose they reason that the more serious the offense, the higher the recidivism rate for those who commit it (i.e., the greater the "commitment" to a criminal career), and therefore the longer the term of incarceration needed to prevent future offenses. In this case, the recommended punishments would again be expected to correlate positively with the seriousness of the offenses, but for reasons other than retribution or deterrence. The fact that respondents use the seriousness of the offense as the criterion for fixing punishments, then, cannot be used to infer the goals or theories of punishment which they employ.

The foregoing considerations are not entirely lost on the authors, who note that "the present study does not explicitly pit this norm [just deserts] against competing norms of justice and thus cannot properly be taken as evidence for the relative strength of this norm" (p. 1120). Nevertheless, they repeatedly state or imply that their results substantiate a norm of "just deserts." If by "just deserts" they mean something other than retributive justice, they are defying conventional usage of that term. Their discussion of the term, however, is ambiguous. Consider, for example, this statement: "While this norm is frequently identified with a retributive approach to punishment, von Hirsch's (1976) essay suggests that the principles of equal treatment for equivalent acts and worse treatment for worse offenses provide a more general underpinning of justice in punishing" (p. 1120). It is truly difficult to imagine how the first clause of that statement differs from the second. The authors further cloud the issue by observing that "deterrence and desert are interdependent bases for punishment" (p. 1120). As numerous philosophers and sociologists of law have observed, that is true in the sense that either theory applied in its pure form may result in gross injustices, and hence justice may best be served by applying the two theories in conjunction. But the theories are surely not interdependent when it comes to stipulating the criteria which determine the severity of the punishment—the "deterrability" of the offense in the case of deterrence, and the seriousness of the offense in the case of retribution.

Hamilton and Rytina's article is important for demonstrating the criteria which members of the public use in fixing punishments, the structure of consensus, and methodological issues in measuring perceived seriousness and preferred punishments. But it cannot and should not be used to infer theories of punishment to which the public subscribes. That can probably

be accomplished only by asking respondents about their goals and rationales for punishment and/or by comparing alternative criteria which respondents may use in assigning punishments, keeping in mind that members of the public may hold to a particular theory of punishment (e.g., retribution or deterrence), but may assign punishments in accordance with criteria which are not formally consistent with the theory (i.e., their theories may be idiosyncratic). The danger in making premature inferences about public theories of punishment is that policymakers who are sensitive to public opinion may implement policies based on a misunderstanding of public sentiment or reasoning.

MARK WARR

Pennsylvania State University

ON PHILOSOPHICAL DISTINCTIONS AND OBSERVED JUDGMENTS

Mark Warr provides a useful service by pointing out that philosophically distinct justifications for punishment could lead to a pattern of data such as we obtained in our article. Although he did not say this explicitly, his argument amounts to a statement that various motives for punishing—such as retribution, deterrence, and incapacitation—would produce an identical fitting of punishment to crime as long as their criteria—such as seriousness, deterrability, and incorrigibility—were highly correlated. The only disagreement appears to be over what to call what we found.

Warr agrees that what we found was that "the punishment should fit the crime." He disagrees with our labeling it "just deserts." We attempted, apparently too briefly, to indicate the generality of our concept by referring to von Hirsch (1976), who suggested possible synonyms—proportionality, just deserts, retribution, and, his own choice, commensurate deserts. Von Hirsch also noted the potential consistency of commensurate deserts with deterrence and incapacitation motives. Our first choice for a label was "proportionality," but the data confirmed only "monotonicity." "Commensurate" connoted a mathematical explanation. "Just deserts" both described the match of increase with increase and evoked the similarity to norms of distributive equity. But we chose it over the uglier "monotonic matching" in full awareness that motives like deterrence or incapacitation could produce results where "the punishment fit the crime."

But so what? If someone believed that more serious offenses should be more severely deterred or that they merited the greater expense of longer

incapacitation, deterrence and incapacitation would simply be prior principles underlying the use of a rule called "just deserts" or some other synonym. If the efficient amount of deterrence or of incapacitation is collinear with seriousness, these are distinctions without differences.

What we observed was a substantial degree of patterning in individual responses. This patterning was not random but reflected some principle such that punishment severity should increase with crime seriousness. Although a variety of motives might produce such a pattern, including any reasonable notion of retribution as well as certain versions of deterrence and incapacitation, the pattern is one of just deserts in that the appropriate fate of the perpetrator was a function of the seriousness of his offense.

Therefore, can the distinction between different philosophies of punishment be empirically important? Here an ambiguity in Warr's discussion provides illumination for a point we repeatedly sought to make. He first maintains that proponents of different punishment theories might display the pattern we observed. To that extent, it is hard to see why the difference is empirically or policy relevant. But he later maintains that the different philosophies lead to different assignments of punishments. In fact, both might be true. It could be that for the crimes we presented, the different points of view substantially converge on a just deserts principle. (We might add that the correlations that were evidence for just deserts were substantial but less than perfect.) It is also possible that there exist stimuli (crimes) which lead to different outcomes under the different philosophies. But the evaluation of the popularity of one standard as opposed to others requires pitting competing standards against each other empirically.

If competing standards are to be evaluated empirically, the stimuli must reflect Warr's second possibility and not his first. In the jargon of research design, unless the evaluations associated with competing standards avoid the pitfall of multicollinearity, the assessment of competing principles will be impossible. We suggested in our article that this might be achieved for justice principles by a comparison of need and merit, which are substantially unrelated in the real world and probably in popular judgment.

Our own preempirical intuition is similar to Warr's first remarks. Although the different theories of deterrence, incapacitation, and retribution may be different philosophically, they seem unlikely to lead to striking empirical differences in actual judgments. For him this entails using a different method, such as the direct elicitation of philosophies, to attack the problem. For those to whom the distinction matters, that seems a reasonable strategy. But we think that the distinction may make relatively little difference in typical concrete judgments. Although we did not test

for such differences directly, the strength of our results suggests that just deserts is the convergent outcome of different popular theories.

STEVE RYTINA

State University of New York at Albany

V. LEE HAMILTON

University of Michigan

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Review Essay: Solving Collective Irrationality[1]

Micromotives and Macrobehavior. By Thomas C. Schelling. New York: W. W. Norton & Co., 1978. Pp. 252. $3.95 (paper).

Interpersonal Relations: A Theory of Interdependence. By Harold H. Kelley and John W. Thibaut. New York: John Wiley & Sons, 1978. Pp. vii+341. $18.95.

Personal Relationships: Their Structure and Processes. By Harold H. Kelley. Hillsdale, N.J.: Lawrence Erlbaum Associates, 1979. Pp. xii+183. $16.50.

Claude S. Fischer
University of California, Berkeley

Economist Thomas C. Schelling describes, in papers collected and adapted as *Micromotives and Macrobehavior,* the ways in which individually rational behavior leads in the aggregate to an irrational collective outcome. His book cogently poses a crucial theoretical question for sociologists: How do societies overcome—to the extent that they do—structural features that tend to produce collective irrationality? We are familiar with forms of this situation under such labels as the tragedy of the commons, the free-rider problem, the Prisoner's Dilemma, and the Hobbesian question. But Schelling shows that these are only special cases drawn from a larger set of interdependencies among individuals that often lead to paradoxical and problematic collective outcomes. Since much of society "consists of institutional arrangements to overcome these divergencies between perceived individual interest and some larger collective bargain" (p. 127), it behooves sociologists to try to understand the structure of these situations and the social processes they engender.

With his usual lucidity, Schelling makes rather complex mathematical models clear to the untutored in an entertaining fashion by use of graphs and scores of intuitively convincing examples. His basic contention is that many social situations are structured so that individual actors, each behaving in a logical, self-interested (but not necessarily selfish) way, will jointly produce an outcome that is collectively less than optimal—for themselves, for bystanders, or for both. This is collectively irrational in that an alternative set of actions would have produced a better joint outcome, but those actions were avoided because they would have been irrational for any given individual. Illustrations abound: panic (each person quite rationally runs to the exit door; no one gets out the exit), inflation

[1] Ann Swidler provided very helpful comments on this essay.

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psychology (each person rationally spends rather than saves a dwindling dollar; the dollar dwindles faster), conservation (each person rationally realizes that his or her automobile trip will not alter the energy situation; everyone keeps driving).

Among Schelling's contributions to our understanding of these situations is a taxonomy of their structures and their possible "solutions." For example, there are "tipping-point" situations that have two possible stable outcomes (e.g., a faculty seminar with a certain minimum attendance will thrive, but if attendance drops below the critical number, it will quickly disband). And there are structures of interdependence that yield tail-chasing processes (e.g., if all parents try to give their children only slightly more than the average number of toys, a spending spree results as the average continually escalates). Schelling refines these models by incorporating individual diversity in tastes. In his well-known discussion of segregation processes, for instance, he shows that even if whites and blacks were both normally distributed in their preferences for racial composition around a "tolerant" mean preference for numerical equality, the most likely end result would still be extreme segregation. And Schelling incorporates into his analyses the existence of some proportion of "martyrs" willing to sacrifice for the common good.

Economists and mathematical modelers will have to judge Schelling's specific arguments. For theoretically inclined sociologists, the key point is the observation that, quite commonly, individual interests do not sum to collective interests; in fact, they are often diametrically opposed. (For empirical sociology, the point is that individual interests cannot be derived from observed collective outcomes.) Sociologists have long noted this phenomenon, but they have not addressed it fully. There are exceptions, to be sure. James Coleman's work on collective political action, experimental research by sociologists such as Gerald Marwell testing specific propositions from collective choice models, and Marxist work on the "internal contradictions" of capitalism are examples of research on elements of collective irrationality. But the central theoretical question stimulated by analyses such as Schelling's—how do societies handle collective irrationalities?—seems to be largely unattended.

Perhaps we are not attending to this question because it appears to have been answered. Many economists consider the market to be a sufficient coordinating mechanism in all except a few unusual situations that may call for extramarket arrangements. But as Schelling and many others point out, the free market yields optimal solutions only in a severely restricted range of social circumstances, and even there it usually requires external management to avoid collapsing into irrationalities and to avoid costly externalities. The politics of collective goods are not reserved for exceptional conditions, but are a constant of social life.

Some sociologists might argue that Schelling's latent assumption of a *Homo economicus* is false, that the social nature of the individual answers the Hobbesian question. Whether this reply is phrased in terms of a "social will," a "collective conscience," or a model of socialization, it implies that

individuals in fact choose collectively rational action. But most such answers, drawn from normative models of society, are too simple. Collective irrationalities arise as often—or more often—when people share values and norms as when they do not. If everyone in New York agreed that the best thing to do on a given day was to visit the Metropolitan Museum, the city would collapse. More seriously, Robert Merton pointed out long ago that the shared values of material success in America produce collective costs. It is clear from the logic Schelling lays out that whether or not actors agree on end values and norms is irrelevant to whether or not collective irrationalities arise. To be sure, many norms tend to suppress the war of all against all (e.g., thou shalt not steal). But approaches such as Parsons's tend to portray a relatively homogeneous set of socialized actors submerging their individual interests to the collective interests across a range of relatively homogeneous situations. Schelling's book and the other works reviewed here force us to focus on variations in structural circumstances. There are situations in which even well-socialized actors will act in collectively irrational ways. We need to be more alert to the particular logics of specific conditions of interdependency.

One promising research approach that does differentiate among situations and their solutions is the close-grained study of how people deal with microstructures that threaten to create collective irrationalities. Here, the contributions of psychologists Harold H. Kelley and John W. Thibaut provide important insights.

Over two decades ago, Thibaut and Kelley (1959) introduced their "theory of interdependence." (Others call their work social exchange theory, but Kelley and Thibaut strongly insist that social exchange is but a subset of the solutions to interdependence.) In a very influential book, they assimilated much of contemporary social psychology into a model of two (or three) rational maximizers joined in a structure of interdependence. The authors adapted the payoff matrix of game theory to represent that interdependence. Cells of the matrix are formed by the intersections of the options—as rows or columns—of each actor. A number in each cell for each actor represents the net value to the actor of each combination of behaviors. Social action can be thought of as trying to "solve" the "problem" posed by the matrix—finding, if successful, the pair of behaviors, or sequence of behaviors, that yields the optimal joint outcome. The logic of the matrix *partly* determines social action by determining the rational choice of each actor.

Kelley and Thibaut's latest work, *Interpersonal Relations,* elaborates and amplifies the original model. One of the basic analytical steps they take is to disaggregate the matrix (i.e., the structure of interdependence) in various ways. They distinguish between the "given" matrix—the set of outcomes as determined or assessed by an outside party—and the "effective" matrix—the set of outcomes as subjectively evaluated by the individual actor and thus the set to which he or she actually responds. The given matrix determines the effective one by means of specific "transfor-

mational processes." In this way, psychology modifies the logic of the situation (see below). Kelley and Thibaut also decompose the matrix, of either kind, into several components: that portion of the outcomes each actor autonomously controls for himself, "reflexive control"; that portion of the actor's outcomes autonomously controlled by the other person, "fate control"; and that portion contingent on the joint action of both actors, "behavior control." (The last is considered behavior control because the other person cannot determine directly what the first actor's outcomes will be, but can determine what action it would be rational for that actor to take.) By combining and weighting each component in various ways, Kelley and Thibaut show that some interdependencies pose problems of coordination, some of cooperation, some of exchange, and many of combinations of these.

The transformational processes describe how personality, culture, and negotiation enter into social action. Instead of responding to the given values of the situation, each actor "reevaluates or reconceptualizes" his outcomes, taking into consideration the other party's potential outcomes, the past and future of the relation, and the context of the interaction. This reevaluation is guided by certain *rules of action,* which may take the form of high principles, folk wisdoms, or unconscious habits. But these rules are ultimately pragmatic guidelines that the actor has learned over a variety of previous interactions. For example, someone who believes in "maximize joint outcomes" (greatest good for the greatest number) will redefine his own effective outcomes as the summation of both actors' given outcomes. The one who believes in "maximize relative difference" (competitiveness) will redefine the outcomes as the difference between the two actors' values. The first rule—maximize joint outcomes—helps solve Prisoner's Dilemmas, if both actors hold to the rule. The second rule—maximize relative difference—is useful in situations where others are also competitive. If one experiences sufficient exploitation in Prisoners' Dilemmas because of holding to the maximize-joint-outcomes rule when others do not, one is likely to shift to the more competitive principle (e.g., never trust anyone). Thus, individuals will vary in how they respond to the same given matrices. And the given matrix does not fully determine action.

Interpersonal Relations contributes to our understanding of the problem of collective irrationality by providing, at the microscopic level, a taxonomy and analysis of various forms of interdependence, and by specifying clearly the place and manner in which cultural and psychological processes intervene between the raw logic of action evident to rational actors and the action itself. It is, however, a detailed and complex book, which may discourage some sociologist readers.

Harold H. Kelley's *Personal Relationships* is a more accessible introduction to the Kelley and Thibaut scheme. It also applies that theory to the concrete study of close relations. The first substantive chapter of the book introduces the matrix convention and summarizes the various structures of interdependence; the second explains transformational rules; and

the third extends the earlier work by examining the actors' attributions of behavior—do they explain each other's actions as responses to the given matrix or as results of transformations? Much of the conflict between dating couples or spouses concerns disagreements over whether action should be attributed to circumstance or to disposition. (She: "You didn't forget to do the dishes! You just don't want to share the housework!" He: "An emergency came up and I couldn't do them! I do want to share the housework!") In the final substantive chapter, Kelley discusses what might be called the metamatrix of personal relations. At this level, the choice actors face is which of two or more transformational dispositions they will adopt in the relation—to be dominant or submissive, cooperative or competitive, and so on. The outcomes here are the long-run average values for each person over the course of the relation. Many sociologists may find this approach to personal relations constricted theoretically, but Kelley's book is a good introduction to interdependence theory and offers useful insights.

While social psychologists will find the Kelley and Thibaut approach a useful way to study the creation of social order at the molar level, most sociologists will still feel the need to address the problem of collective irrationality at the macroscopic, societal level. Schelling's work points up, as I argued earlier, the insufficiency of current analyses of that issue. One possibility we will have to consider more fully is that often the social order is not maintained. Needs do not always give rise to functions, dysfunctions sometimes predominate, and anarchy is a common social condition. There are parts of our society that approximate collective irrationality, if not the war of all against all—some prisons and ghetto streets, for example, or the Yosemite Valley in camping season. And there are some matters for which suboptimal solutions are the chronic outcomes—energy conservation, pollution, inflation, the rapid cycle of fashion and obsolescence, for example. Often collective irrationality is not solved, but just barely contained.

Another possible answer we have underestimated is force. Often, collective irrationalities are avoided by the imposition of force, including violence. Historically, chaos has been tamed by the brutal victory of one party over the others—whether we are talking about nation-states bringing order to national peripheries or about bullies organizing a play group. American sociologists have probably understated the extent to which violence, or latent violence, pervades society. (Force is important in Marxian analyses but has usually been restricted to the analysis of state and class relations.)

In addition to force, explicit and implicit negotiations of "social contracts" are solutions to collective irrationalities. Schelling writes: "What we are dealing with is the frequent divergence between what people are individually motivated to do and what they might like to accomplish together. . . . What we need in these circumstances is an enforceable social contract. I'll cooperate if you and everybody else will" (pp. 128–29).

Such explicit contracts exist in some areas, usually negotiated by faction leaders, as in the cutting of political deals. And they are implicit in many others, such as the etiquette of public places (e.g., "I'll wait my turn in line if you and everybody else will"). But, as Schelling also points out, these contracts are often difficult to enforce. They are embedded in larger games in which abiding by a contract is only one behavioral option (e.g., "If it means missing my train, I'll be damned if I'll wait my turn"). Nonetheless, such social contracts are one common solution to interdependencies (typically, we do take turns in line); their emergence and enforcement ought to be studied more closely.

Finally, morality is a solution to collective irrationality. I do not mean by this values, end goals, or even abstract principles to which people adhere, but rather the day-to-day rules of conduct—the "transformational processes"—that lead individuals to transcend the impelling logic of the "given" situation, to transform their definition of the situation itself, and so to act in collectively rational ways. When and why people apply such rules as "look out for number one," or "do unto others as you would have them do unto you," or "blood is thicker than water" will certainly affect the solutions for potential collective irrationalities. (The particular circumstance is important. Some people, for example, believe in "dog eat dog" in business but also in sacrificing for family at home.) As Schelling points out, many collective irrationalities can be solved without universal adherence to moral principles. Lines will remain orderly, for example, if only a few people break the etiquette; and streets may be safe if only a few people volunteer to be neighborhood watchmen. But there is probably some critical range of adherents below which a social group will suffer collective irrationalities. While one is tempted to call this a state of anomie, it is not really a condition of normlessness: people may share a norm, but it is not one that provides an optimal solution to the problem of interdependence. Amoral familism, as described by Edward Banfield and George Foster, is a shared principle among peasants, but it merely exacerbates collective problems. Morality, as referred to here, is a collection of transformational principles that resolve collective irrationalities. How societies evolve, teach, and sustain adherence to such rules often enough to contain the collective irrationalities of social life is another inadequately studied sociological problem.

While the question of social order seems to be a hoary one, the nonsociological works reviewed here—and the intellectual traditions they represent—call for some rethinking of our more conventional answers. Our standard metaphors—the "breakdown" of the social mechanism, the "malfunction" of the social organism, the "rupture" of the social fabric—mislead us. They give us an undifferentiated dichotomy of order versus disorder, when social life is really composed of some orderly situations, some disorderly ones, and many in between. These works stress that the very logic of collective life—interdependence among autonomous actors—often calls forth "disorderly" outcomes. Social institutions sometimes overcome

Review Essay: Is a Social Science of Politics Possible?

Political Society: A Macrosociology of Politics. By Edward W. Lehman. New York: Columbia University Press, 1977. Pp. xi+247. $15.00.

What Does the Ruling Class Do When It Rules? By Göran Therborn. London: New Left Books; New York: Schocken Books, 1978. Pp. 290. $14.95 (cloth); $8.75 (paper).

Anthony M. Orum
University of Texas at Austin

Is a science of politics, more particularly, a social science of politics, possible? That is to say, is there a method whose systematic application would permit one to accumulate and to refine one's knowledge of politics? Or is politics so tinged and tainted with human emotions that a science of it simply is unattainable? Many scholars believe the study of politics to be nothing more than the rantings and ravings of violent people, people who cannot seem to agree with one another on what they are talking about. So caught up in their own fancies are these combatants, it is claimed, that they are unable to say anything intelligible about politics. Instead, they sit and recite meaningless rubbish—and, what's more, they often get it published.

There is, in fact, a great deal of nonsense written about politics, and a great deal of it is published by those who call themselves social scientists. It is nonsense because it says nothing and it leads nowhere. I do not know how to draw the line easily and clearly between the sense and the nonsense, though once I thought it merely a matter of empirical validity; and I fear, as a result, that occasionally I am not fulfilling my duty to my students who are not even aware that such a difference exists. But, for all my own confusion—and my feeling sometimes that I could use a good dose of Comtean cerebral hygiene to rid myself of the accumulated cobwebs which surround my meager knowledge of political life—I think I have arrived at a few principles to guide my judgment.

It seems to me that a social science of politics must begin simply by trying to understand that which has happened, and that which may happen, in the world of politics. To understand is to put the pieces of a puzzle together in such a way that we who engage in the study, and frequently even they who are so studied, can find the solution satisfying. If we accept the premise that to be human is to confront a variety of dilemmas—about the worth of our own lives compared with those close to us, for example, or about the value of the state as compared to the value of the lives of its citizens—then to understand is to figure out how real

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human beings, at a particular time and place, confront and solve their dilemmas. To understand politics is to render the meanings that people attach to their political actions—to obtain power, to protect positions, to secure regimes, to overthrow decadent dictators—into a veritable model of the times and situations we are studying. To understand politics is not only to render motives—the understandings of the actors themselves—lucid, but also to portray them as part of a drama whose undercurrents contain the lingerings of history and the fine particles of culture. To understand politics, by the same token, is not to create needless hypotheses, or to employ useless technical apparatus, or to fabricate particular tests of significance; all these are merely the trappings that are only infrequently important to achieving our principal ambition, that of a fundamental set of insights into political life.

Much as the reader might think I have furnished standards that could be satisfied only by the works of the Great Author Above, there are a handful of writings that fit my bill. Hannah Arendt's *The Origins of Totalitarianism* is very high on my list, for this is a work that gives me considerable insight into the motives of people, on many sides and in various quarters, who were caught up in one of the great cataclysms of this, or any, century. Within this book there are threads of law, of history, of specific individuals, of political institutions, and, perhaps most important, of certain moral dilemmas and issues, all of them ingeniously woven together into a masterly interpretation of totalitarian regimes. Somehow Arendt managed to capture on paper a world whose profundity and simplicity, clarity and obscurity, passion and indifference are exceeded only by the phenomenon—Nazi Germany—itself.

There are some other works that come close to this exemplar. The *Middletown* studies, by the Lynds, which sought to create a certain kind of portrait of politics and other forms of life in Muncie, Indiana, also seem to display some of the same feel for the actors and the events, and thus to approximate criteria for a sensible statement about politics. So, too, the study of New Haven by Robert Dahl provides a kind of solution, composed of historical events and special interest groups, of thick social bonds and prominent individuals, of many interests and few clashes, that occupied that particular political arena. Granted, Dahl's sketch is more than a touch on the mild side; it renders politics virtually innocuous, and it fails even to explore the apparent difference of citizens and the unique importance of Mayor Lee to any substantial degree. But, like all significant scientific renderings, it establishes a certain frame of interpretation, and it insists, even to the point of exaggeration, that this frame furnish a fair representation of the reality itself. Consequently, we now understand, better than before, how it was that politics in New Haven worked through the 1950s—how a mayor, some concerned citizens, businesses, and other groups came to reach specific agreements on how their community would be run and their collective problems solved.

Another work of equal, if not greater, caliber and merit is *The Power Elite* by C. Wright Mills. It is a work that I have consulted time and

again because of its sensitive rendition of the political struggles in mid-20th-century America. Like Arendt, Mills succeeds in creating a portrait of those times and people which seems real. In rereading Mills, moreover, one discovers a master of clear prose, a stylist who lacks great elegance yet writes smoothly. But beyond that, for Arendt herself was no great writer, Mills manages to carry on self-conscious dialogues with past masters like Pareto and Weber, Marx and Mosca, to draw from them just the proper number and the right mixture of ideas, and to apply these notions effectively in order to fashion his compelling portrait.

As a social scientist, Mills tried to comprehend the actors and the institutions that constituted the American polity, and he tried to do so in a fashion as concrete and as faithful to the particular concatenation of cultural and historical circumstances as possible. In his portrait there were neither general laws nor universal principles at work, but rather men and women caught in the flow of their positions in "society" and in government, fulfilling as best they could the claims put upon them. By and large, the struggle between the individual and his niche is missing; but what is there, in Mills's work, is the great hiatus that separated the large majority of citizens from those places where their destinies often were indifferently calculated. In a sense, we might even say his was a picture of the world much the same as Arendt's, but it was from a different angle and cast in a softer light. Mills also depicted the capture of the human spirit by big money and big power—indeed, not just the capture but the outright victory. And it was a victory of which Arendt had forewarned us; ironically, however, we were told it was to occur only in Europe, not in America as well.

As in any and all such works, Mills engaged in partial truths, in extreme statements about certain apparent facts. While he did not quite speak of the "expropriation of the expropriators," he did confine the actors involved in the struggles for power in urban areas to a relatively minor place on the American political stage, and he was more than a little preoccupied with the rise of the defense establishment and the military vision of reality. Yet, on matters having to do with the power of the president and his advisors, with the comparatively trivial amount of influence of all but the most senior congressmen, and with the often limited and ineffectual role of pressure groups in national politics, Mills seems to have rendered accurately the human motives and central events of these times. Indeed, like Marx and Weber, he seems to have deciphered the latent tendencies as much as the actual facts, tendencies that after his death resulted in a major war, a crisis of presidential authority, a defense establishment seemingly gone haywire, and a sense of estrangement among citizens which turned eventually from simple apathy into a profound disbelief in the sacredness of those institutions and their leaders which Mills had so clearly depicted.

What has made for the enduring success and widespread appeal of Mills's achievement? If my general guidelines are right, Mills succeeded because he made it possible for us to comprehend, in the very widest

sense, the world of his human subjects. Somehow he was able to demonstrate the manner whereby the institutions of American society had gradually given birth to positions of considerable power, positions whose occupants shared a similar world view and social background. That such officials did not act immediately in concert really was of little moment; that their motives were shaped by their current slots in society, and, by implication, by the settings that had trained them to fill those places, was of critical importance. Mills's account, in effect, was a subjectivist one, based on structural developments in the world; it thereby achieved the blend of two views—subjectivism and structuralism—which we often take to be fundamentally opposed to one another. That it did succeed, and that it did so with a blend of the two frameworks, should remind us to be wary of those who would urge us to rely exclusively on one theoretical scheme or the other.

Like all great works, *The Power Elite* has had to pay a price for its success. It has done so in terms of the very weak, if nevertheless often very popular, imitations it has spawned. While it is true that we now possess more evidence about the corporate backgrounds of people in government, and the governmental backgrounds of people in corporations, we have not truly advanced our understanding of this particular world—of what moves people like you and me, for instance, to complain only infrequently, and then just to pollsters, that as citizens we are more like slaves than free people.

There are, fortunately, some works that at least aspire to take us forward. Although many of them do not possess the ambitions of the works of Arendt or Mills, or even Dahl, they still aim to give us some better feel for what is going on. One such book is *Political Society,* by Edward Lehman. Lehman informs us that he intends to create a language which will help to guide us through the thickets of sometimes painful and often obstinate political realities. At the outset, such an aim could be self-defeating, or, rather, it could be self-defeating depending upon the skills of the person who attempts it. While Arendt, and Mills, did, in fact, leave us with a legacy of rich conceptual language, as in Arendt's conception of totalitarianism or Mills's notion of the power elite, they both began from concrete sets of circumstances. Their language was, in effect, a by-product of their attempts to explain some specific events. Lehman's is not. He simply wants us to adopt a uniform frame of reference whose focus will be the nature of politics in any and all societies.

What will this language entail? It is introduced with two denials, a hint possibly that it will turn out to be less than fulfilling. It will not deal with human actors à la Weber, but with collective ones, à la Parsons; and it will not deal with culture in a broad—need one say important?—fashion, but will relegate it to the realm of circumstances that simply create variations among political systems. Lehman dismisses culture as a useful idea chiefly because, in his eyes, it is fundamentally too inflexible a component of the world to include in a program whose aim, in part, will be to furnish the basis for practical action. The reader, of course, might wonder

at this point how wise it will be to create a program for practical action, yet omit human beings. Granted they do interfere with "collectivities"; but if they do not act, who will?

Having so established the general limits to our sensibilities and expectations, Lehman proceeds, in a very careful and systematic fashion, to produce the elements that will constitute his map of the world. Naturally, power is an important element here because Lehman, like virtually all social scientists who establish this sort of agenda, believes that Weber was on the right track: we must establish some clear definition of power. So power, in roughly the Weberian sense, is introduced, and Lehman then proceeds to tell us about the resources for power, including material and normative ones, and its bases. Bases come in two handy sorts, tangible ones, such as property, and symbolic ones, such as reputation. Furthermore, if we speak of power, we must naturally think as well of goals, because no one rational would use clout just for its own sake. In *Political Society* goals, as we might anticipate, are not the unenlightened purposes of acting individuals; instead, they are the *collective* goals of *collective* actors.

All of this represents the mere beginnings of the program. Power, Lehman argues, can be broken down further into "systemic" and "intermember" power. The former term identifies the efforts and resources which can be brought to bear on the attainment, naturally, of collective goals, whereas intermember power refers to the variations in the amount of power distributed among the collective members. After many further refinements in much the same vein, Lehman proceeds to introduce us to the players themselves. He tells us that there are collective actors in society, which he will call macrosolidary groups (others have called them solidary groups or collectivities), and that there exist differential privileges and life-styles as well. And, in the political arena, he speaks of the state and the polity, populating them each with slightly different actors.

These, then, are virtually the basic elements of Lehman's language. Presumably, if we take the time to master them, we will be better equipped to arrive at the understanding of politics so essential to the progress of knowledge. But will we? Here several points must be made in rebuttal to Lehman's claims. All this language, by itself, cannot create a scheme to study the real world of politics, let alone contribute to the deep sense of understanding attained by works like those of Arendt and of Mills. Lehman essentially borrows en masse the insights of Parsons, of William Gamson, of Amitai Etzioni, without also incorporating the specific realities, even the specific problems, to which these scholars addressed themselves. Lehman, moreover, has not fashioned these elements of language into a coherent and dynamic scheme for laying bare the quality of political life. Inasmuch as there exists no special question to which the language will be addressed, or a motive force that could lend energy to propel it along, say, like the notion of system, the language just falls apart into meaningless words, words, and more words. To make it even worse, Lehman thwarts his own ambitions almost entirely when he claims that, apart

from its fruitfulness for practical action, the utility of his particular language awaits confirmation through results manufactured by analyses of variance. Here, indeed, lies his basic misconception. He wants us to forgo the criteria of understanding, and to submit instead to the readings of technical instruments, of mere statistics. Quite apart from the problem of ever testing a language (remember the ultimate failure of the logical positivists), we must by now realize that statistics themselves never provide any final guarantee for an understanding, no matter how robust they may be.

Besides Lehman, there are some Marxist scholars who, themselves unhappy with such characterizations of politics as those of Arendt and Mills, want to picture the world of politics from their own vantage point. Some people, generally those who believe that there can never be a science of politics, are definitely of the opinion that no Marxist writer could ever make heads or tails of the world. I think they are simply mistaken. From Marx himself to Antonio Gramsci, the attempts by Marxist scholars to enlighten us about how the world works have frequently been very successful, even by the criteria I established at the outset of this essay. Did not Marx, in *The Civil War in France*, in *The Class Struggles in France*, and in *The Eighteenth Brumaire of Louis Bonaparte*, serve to deepen our appreciation of 18th- and 19th-century France? And among contemporary Marxist scholars there are those who, like the late Nicos Poulantzas, in a slightly different fashion manage to make a certain amount of sense of particular historical settings. Just because one is a Marxist, in other words, one is not condemned to be unintelligible.

One young Marxist scholar who aspires to provide us with a greater feel for the world of politics is Göran Therborn, author of *What Does the Ruling Class Do When It Rules?* Like many other Marxist writers today, Therborn wants to explain the nature of the state from within the basic paradigm of Marxist scholarship. But he also wants to move Marxist analysis from the preoccupation with philosophical questions demonstrated by members of the Frankfurt School, for example, to a concern with the scientific generation of political facts.

His book consists of three essays, all of which attempt to clarify the role of the state. The first of them provides a framework for examining the nature of the organizations and institutions of the state (state apparatus), and for defining the essence of state power. Unlike some other contemporary Marxists who have come to emphasize the "relative autonomy" of the state, among them Ralph Miliband, Therborn argues that the power of the state is grounded in the stage of development of the class struggle, and in the fundamental forms of contradiction in society. In characteristically difficult, often even intensely opaque prose, he writes: "The dynamic of our analytic model of the state apparatus is provided by the developmental logic of the class struggle and the various modes of production. The state apparatus feeds back into society a contribution to the regeneration of the class relations that formed it. It does this by reproducing the state-society relationship inscribed within it. . . . Both the state

apparatus and the class relations that formed it are reproduced or transformed by the interventions of the state—by that state power which is the central focus of the class struggle and its changing relationship of forces" (p. 45). Therborn then proceeds to throw fresh light, so he claims, on the nature of the state apparatus and state power by urging us to think of the state as a system, an entity that processes inputs and outputs. Among the inputs are the actual forms of class and class contradictions in a particular place and time, while the outputs include the policies and regulations of a specific state. He makes our effort to grasp his analysis and his purported insights supremely difficult, not only because of his rather insistent usage of a certain style of Marxist language (to which I do not object if, as by Poulantzas, for instance, it is employed to highlight some political events in the real world), but also by considering the state in three eras, not simply one—feudalism, capitalism, and socialism.

In the second essay, Therborn turns to consider the ways in which the state exercises its power. Here he is concerned, or so it seems to me, with making better sense of Poulantzas's claims about how states come to embody class contradictions, and also how in certain historical instances (conjunctures) and settings (social formations) they actually serve to represent the domination not of single classes, but of fractions or alliances of classes. As part of his argument, he maintains that the conceptions of the elite, as in the work of Mills, and of pluralism, as in that of Dahl, should be abandoned because they are much too concerned with the intentions and aims of actors, much too little preoccupied with the nature of structures (e.g., that of class) and politics.

How, he then asks, does the state intervene in society, and what effects does such intervention have on the relations of production and on the class character of the state institutions? To answer, he considers the ways in which specific types of political systems of representation, for instance, the bourgeois notable format characteristic of Great Britain in the 18th and 19th centuries, and specific illustrations of state intervention, for example, outright repression, alter the character of society and of the state. Once again, a large part of the difficulty involved in genuinely appreciating his insights, assuming they exist, comes from his overwhelming attachment to certain terms, an affection that serves to diminish, rather than to enhance, our understanding of politics. Finally, his third essay is concerned exclusively with practical action—though, obviously, of a different sort of praxis than that envisioned by Lehman—and, in particular, with "the taking of state power." And here Therborn simply discusses and briefly evaluates the various formulas that have been employed by particular groups of dissidents in their revolutionary struggles, such as that of the Bolshevik Party in Russia.

Therborn's work, as the reader must acknowledge, is like Lehman's inasmuch as it provides a program for restructuring our thinking about politics, not a demonstration of how live human beings, or even groups, act in concrete settings and places. To his credit he does, at least, provide a mechanism which helps to make his program more than merely a series

Review Essay: On Quentin Skinner

The Foundations of Modern Political Thought. Vol. 1: *The Renaissance.* Vol. 2: *The Reformation.* By Quentin Skinner. Cambridge: Cambridge University Press, 1978. Pp. xxiv+305; 405. $29.50 each (cloth); $9.50 each (paper).

Robert Alun Jones
University of Illinois at Urbana-Champaign

Traditional practice in the history of political thought seems much the same as that in the history of sociological theory.[1] Practitioners of both such "histories," for example, typically operate from departments of political science or sociology instead of history per se; both by training and by inclination, therefore, they are more apt to identify themselves as political or sociological "theorists" than as historians, and to offer a rationale for their scholarly investigations which is greatly at odds with anything historians might supply in the effort to justify their own research. Rather than attempt to understand and explain the development of past ideas, therefore, such theorists typically view themselves as supplying the normative framework within which the more empirical study of politics and society ought to be conducted, and from whose increasingly profane encroachments their own more "humanistic" pursuits are to be protected. Ultimately, such theorists have viewed themselves as the guardians of a sacred canon of "classic texts," whose pages yield great and timeless truths, of deep and continuing relevance to social and political theory—at least to those granted the wisdom and insight to discover them. Within such a perspective, the thorough and concerted effort to reconstruct the historical context of such classic texts is viewed, not simply as irrelevant, but occasionally as dangerous;[2] for the argument that such texts can be understood only within the broader social and intellectual history of *their own* time contains, however implicitly, the suggestion that they may lack timeless truths altogether; and this in turn undermines the traditional justification for the investigation itself. The result is that the history of both political and social theory has often been less a serious scholarly pursuit than, as one wit has put it, that activity which fills the years between genuine achievement and retirement (Collini 1978, p. 47).

Of course there are interesting differences as well. Political theorists, for

[1] These introductory remarks were suggested to me by some observations on the history of political theory made by Quentin Skinner (1979, pp. 11–13). The suggestion that the same observations might easily be made concerning the history of sociological theory is entirely my own.

[2] See, for example, the somewhat alarmist reaction to proposals of this kind found in Johnson (1978). See also Jones (1978).

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example, have typically been more thorough and scholarly than sociological ones in simply reading and guarding their sacred canon; more important, they have also been more methodologically self-conscious in openly defending and justifying the activity itself. It is in political theory, therefore, that we have accounts of *Leviathan* like those of Howard Warrender (1957) and F. C. Hood (1964), which have been formulated simply by reading the classic text repeatedly "until its argument assumed some coherence"; still further, we have overt statements like that of the late John Plamenatz who, discussing Machiavelli and Montesquieu, argued that "we learn more about their arguments by weighing them over and over again than by extending our knowledge of the circumstances in which they wrote" (1963, 1:x). Nonetheless, no one familiar with the traditional literature in the history of sociological thought will question the fact that its authors have viewed themselves as "theorists" rather than historians, as providing a normative framework for present thought rather than a historical interpretation of past ideas, as students and even guardians of classic texts rather than scholars reconstructing earlier contexts; and if pressed, as they almost never are, it is doubtful that such practitioners would defend or justify their activities in terms very different from those of Warrender, Hood, and Plamenatz.

This is particularly interesting in that these traditional practices were attacked rather convincingly by Robert Merton almost 15 years ago. Referring to the "very different functions" of the history and systematics of sociological theory as "obvious enough to be embarrassing" (1967, p. 4)—after all, departments of biology do not confuse the history of biology with current biological knowledge—he went on to lament the admittedly "attractive but fatal" confusion of the two, which has produced neither history *nor* theory but a "poorly thought-out hybrid." The result, Merton objected, was a "most parochial, almost Pickwickian conception of the history of sociological theory as a collection of critical summaries of past theories spiced with short biographies of major theorists" (1967, p. 2).[3] And the irony, Merton concluded, was that sociologists had retained this "narrow and shallow" concept of the history of ideas at the very time when "a new breed of specialized historians of science [e.g., Kuhn, Gillispie, Hall, Mendelsohn, Price] is drawing widely and deeply upon the sociology, psychology and politics of science for theoretical guides to their interpretations of the development of science" (1967, pp. 2–3).

Merton's assessment was not quite totally disheartening, for in the inception of the *Journal of the History of the Behavioral Sciences* two years earlier, he saw one of several indications of the emergence of "authentic histories," genuinely concerned with "the complex filiation of sociological ideas" (1967, p. 2). Other signs warranting most cautious optimism have

[3] This, Merton adds parenthetically, helps to explain why almost all sociologists feel themselves qualified to teach and to write the "history" of sociological theory; for the guiding assumption is that all that is required is some acquaintance with the classical writings of an earlier day (1967, p. 2).

followed.[4] Nonetheless, in its current state the history of sociology not only lags far behind the history of the natural sciences but continues to foster and reward precisely those egregious practices described in the first paragraph of this essay—albeit, once again, without the elevated methodological self-consciousness evident among political theorists. If we cannot learn from Merton and the historians of natural science, therefore, perhaps it is time to turn our attention to the field of political theory; for there, during the same period in which we ignored the arguments of Merton, a far more relentless and devastating assault upon the same practices has been led by Quentin Skinner of Cambridge University.[5]

Beginning with a review essay on Hood (Skinner 1964) and progressing through a series of brilliantly argued papers based on seemingly endless research into the writings of Hobbes's contemporaries, Skinner first discounted two of the most common assumptions about Hobbes's political theory—that it was a unique and isolated phenomenon, and that it encountered only intense opposition from its initial audience. This intellectual context firmly in hand, Skinner next argued that the views of Hobbes's contemporaries necessarily implied by the "deontological" interpretations of Warrender and Hood were patently absurd, for they required the assumptions that (1) every contemporary, whether follower or opponent, missed this point in Hobbes's theory; (2) every contemporary was mistaken in precisely the same way; and (3) despite this, Hobbes never bothered to inform them of their error (Skinner 1965, 1972*a*, 1972*b*). In other words, by placing *Leviathan* within its social and intellectual context, Skinner did not merely provide "background" for a subsequent interpretation; the force of his argument is that the rigorous study of the historical context is a part of the interpretive act itself, and that indifference to this injunction can only result in what one reviewer has called "howlingly anachronistic interpretations."

While no one in sociological theory has even approached the depth of commitment which Skinner brings to this task, sociologists will at least recognize his appeal to the context of past thought as falling within a familiar tradition in the history and sociology of science—specifically, that concentrated and sustained attack on "presentist" or "Whiggish" histories of ideas begun by Butterfield (1931), applied to the history of the natural

[4] One thinks immediately of the fledgling *Journal of the History of Sociology,* the Research Committee on the History of Sociology (ISA), George Stocking's Chicago Group for the History of the Social Sciences, and the appearance of more historically minded works such as Lewis Coser's *Masters of Sociological Thought* and Steven Lukes's *Durkheim.* Perhaps most promising, however, is the emergence of a young, highly critical generation of specialized scholars—e.g., Stefan Collini, Henrika Kuklick, Wolf Lepenies, Robert Nye, David Watson, W. Paul Vogt, and the Durkheimians, Philippe Besnard, Mohamed Cherkaoui, et al., in Paris—for whom the history of sociology is not a passing fancy but a full-time commitment.

[5] Skinner is exceptionally generous concerning his debt to other writers in the development of his views on interpretation. In addition to his obvious reliance on R. G. Collingwood and J. L. Austin, he has expressed admiration for the work of Alasdair MacIntyre, Martin Hollis, J. G. A. Pocock, and especially John Dunn.

sciences by Kuhn (1962), and extended to the history of the social sciences by Stocking (1968). Indeed, in "Meaning and Understanding in the History of Ideas" (1969), Skinner launched a withering attack on the lamentable influence of the historian-theorist's "paradigmatic preconceptions," accompanied by a barrage of illustrative anachronistic interpretations which . . . well, make one howl.

Nonetheless, it would be a mistake to regard Skinner as just another of the happily increasing number of antipresentist historians of ideas. Not only is he more circumspect in his "historicism" than are some of similar commitment,[6] he has also cast a highly skeptical eye on most of the devices whereby writers with truly honorable historical intentions have sought to tie the subtle knot between text and context. When antecedent or contemporary intellectual "influences" on the classic writer are alleged, for example, both Skinner (1969) and his Cambridge colleague John Dunn (1968) have repeatedly questioned whether the minimal conditions of demonstrable similarity, awareness, and dependence required by such "explanations" are ever, in fact, met; the result, Skinner observes, is that the entire repertoire of *einflüss* studies might easily be reduced to a question of which ideas in some great books remind us of other ideas in other great books. And when the appeal has been to a broader intellectual spectrum, whether in Marxian or Namierite form, Skinner and Dunn have again questioned whether such transformations of highly complex intellectual performances into either epiphenomenal effects or self-justifying political tracts are in fact sufficient to, or even consistent with, the primary task of an intellectual historian—the *understanding* of the ideas themselves.

In the face of such unrelieved skepticism, one might reasonably ask what positive recommendations Skinner has had to make, and it is precisely here that he has been most subtle and original. In a highly sophisticated blend of the idealism of R. G. Collingwood and the speech-acts theories of J. L. Austin, Skinner has argued that both anachronism and vulgarism can be avoided only if we concentrate on the *intentions* of the author in question. Classic works, Skinner has argued, should be viewed not as the objectification of disembodied abstractions, but as speech-acts, possessing what Austin (1962) called "illocutionary force"—that is, the act performed *in* saying something. The means to recovering such intentions, Skinner continues, is to reconstruct the linguistic conventions governing the performance of *those* actions in a society of *that* kind—in short, to read virtually everything written by the author's contemporaries, and then to set the work of special concern firmly within that context. Only in this way, Skinner concludes, can we hope to distinguish between the original and the commonplace in writings of the past, and thus to discern what their authors were *doing* in writing what they wrote.

Such an unrelenting assault upon the most precious assumptions of political theorists could not go unanswered, and Skinner has faced con-

[6] For the particular caveats which he has entered on this subject, see Skinner (1969, pp. 5–6; 1974*b*, p. 281); see also Dunn (1968, pp. 97–98).

siderable opposition. It has included a critical assessment of his contribution to the literature on Hobbes (Wiener 1974), several criticisms of his methodology (Tarlton 1973; Parekh and Berki 1973; Schochet 1974) and its analytic foundations (Mew 1971), and a rather interesting defense of the anachronistic reading of classic texts themselves (Leslie 1970). More recently, however, the appearance of Skinner's splendid two-volume study of late medieval and early modern political theory has raised what remains of this controversy to new and more intoxicating heights. Indeed, for those who watched this project grow, applauded its successive installments, and eagerly awaited a stunning result, the arrival of *The Foundations of Modern Political Thought*, amid most favorable and authoritative reviews, conjoined with its author's election to the prestigious chair of politics at Cambridge University, has marked not only a culmination but a vindication as well.

The book has three main goals. The first is to offer an "outline account" of the principal texts of late medieval and early modern political thought, including the chief political writings of Dante; Marsiglio of Padua; Machiavelli; Guicciardini; Erasmus and More; Luther, Calvin, and their disciples; Vitoria and Suárez; and the French constitutionalists including Beza, Hotman, Mornay, and especially Bodin. No such survey of the transition from medieval to modern political theory has been attempted since Pierre Mesnard's *L'Essor de la philosophie politique au XVIe siècle* (1936); and while Skinner modestly disclaims any effort to match Mesnard's range or depth of scholarship, he has taken full advantage of more recent scholarly discoveries and the growth of a large and important secondary literature. The result, in the authoritative judgment of Keith Thomas (1979), is the best history of early modern political theory yet written.

Skinner's second goal is to use these texts to illuminate a more general historical theme—the process by which the modern concept of the state was formed. This in turn explains the chronological boundaries of the work itself; for while Skinner began his investigations with the 17th century, he increasingly found that it was between the late 13th and the end of the 16th centuries that the decisive shift was made from the idea of the ruler "maintaining his state"—this simply meant upholding his own position—to the idea that there was a separate legal and constitutional order, that of "the State," which the ruler had a duty to maintain. When the power of the state rather than that of the ruler thus came to be seen as the basis of government, there emerged the modern Weberian conception of the state as the sole source of law and legitimate force within its own territory and the sole appropriate object of its citizens' allegiances.

Skinner's third goal, not unexpectedly, is to exemplify a particular way of approaching the study and interpretation of historical texts. While Mesnard, for example, concentrated almost exclusively on leading theorists of the period, Skinner has focused instead on the more general social and intellectual matrix from which their ideas emerged. Thus he begins most sections by discussing what he regards as the most relevant characteristics

of the societies in and for which the theorists originally wrote; he then proceeds to consider the intellectual context—earlier writings and inherited assumptions about political society, as well as the more ephemeral contemporary contributions to social and political thought—on the ground that these writings set the nature and limits of the "normative vocabulary" available for the discussion of those topics with which the classic texts deal.

All this is quite consistent with the methodological program Skinner had already set out, and also with one of the most general and recurrent themes in recent historiography—that if we wish to understand earlier societies, we need to recover their different *mentalités* in as broadly sympathetic a fashion as possible. But if this injunction is justified, and Skinner clearly thinks it is, then we can no longer "focus our main attention on those who discussed the problems of political life at a level of abstraction and intelligence unmatched by any of their contemporaries. If on the other hand," Skinner adds, "we attempt to surround these classic texts with their appropriate ideological context, we may be able to build up a more realistic picture of how political thinking in all its various forms was in fact conducted in earlier periods" (1:xi).

While Skinner thus calls for a history of political theory which is genuinely *historical*, his most ambitious desire is that his approach might eventually illuminate some of the connections between political thought and action. Within the traditional academic division of labor, he observes, political historians attach a somewhat marginal role to political thought; inversely, historians of political theory, as we have seen, focus on a canon of classic texts and thus largely ignore any close ties between political thought and political life. But if the latter were to think of themselves as historians of more broadly based ideologies, Skinner suggests, they might uncover at least one way in which the explanation of political behavior depends upon the study of political ideas. For in recovering the terms of the normative vocabulary available to any given agent for the description of his or her political behavior, we are also indicating one of the constraints upon the behavior itself. It follows that if we are to explain why such agents act as they do, we are bound to make some reference to this vocabulary. The study of such normative vocabularies, Skinner therefore concludes, is at least one area where the history of political thought can contribute to the study of political behavior.

Potential rewards from the study of thought and action thus entered, however, it should not be forgotten that the main reason for Skinner's focus on the study of ideologies (like some aspects of his methodology) is really quite traditional—the goal of understanding the classic texts themselves. Through such an approach, he argues, we begin to see not merely what arguments their authors were presenting but "what questions they were addressing and trying to answer, and how far they were accepting and endorsing, or questioning and repudiating, or perhaps even polemically ignoring, the prevailing assumptions and conventions of political debate" (1:xiii)—in other words, what they were *doing* in writing what they wrote.

Worthy goals indeed. But in the face of the extraordinary tasks which Skinner himself has set, does *The Foundations of Modern Political Thought* succeed? The answer, thus far testified to in varying superlatives by Keith Thomas, John Dunn, Alan Ryan, and Anthony Quinton, is an unequivocal yes. Even leaving aside the question of a new methodology, reading Skinner's *Foundations* is an exhilarating experience, a fascinating tour through the myriad ideas of the Renaissance and Reformation, led by a young historian in total command of the skills of his discipline while still enamored of its beauties. Indeed, a rather querulous caveat which might be entered against the premature vindication of Skinner's method is the fact that some of the finest qualities of *The Foundations* are surely due to no new method at all but instead to its author's exemplification of those skills which have always made for superb histories—for example, a deep and thorough immersion in primary sources; a complete command of the secondary literature; a facility for the languages (particularly Latin, Italian, and French) in which both are written; an almost staggering erudition, combined with a most lucid, transparent literary style; and an effort to distinguish the important from the trivial and the past from the present, together with a willingness, in each case, to concentrate on the former.

Nonetheless, after 15 years of methodological debate, the real test of *Foundations* will be its capacity to cast new light on the classic texts, and here Skinner has offered two general observations. The first is that, to a large extent, the vocabulary of Renaissance moral and political thought was derived from Roman Stoic sources. Skinner acknowledges the work of Eugenio Garin on the Platonic origins of Renaissance political philosophy and that of Hans Baron and J. G. A. Pocock on the Aristotelian influence on "civic" humanism; but he extends still further the view that the political theorists of Renaissance Italy, and of early modern Europe generally, were strongly influenced by Stoic values and beliefs. In particular, this results in an altered conception of Machiavelli's relationship with his predecessors, and thus of his aims and intentions as a political theorist.

Of more substantive interest to sociologists, however, will be Skinner's discussion of Reformation political thought, in which he emphasizes the "almost paradoxical extent" to which both Lutherans and radical Calvinists relied upon concepts derived from the study of Roman law and scholastic moral philosophy. The traditional Weberian view, of course, insists upon a sharp distinction between the social and political theories of the Catholics, on the one hand, and their more radical 16th-century Calvinist counterparts, on the other; and this in turn results, in works like Michael Walzer's *Revolution of the Saints*, in discussion of a "Calvinist theory of revolution." But if Skinner is right, then strictly speaking, no such entity exists. This is not to deny that the revolutionaries of early modern Europe were, in general, professed Calvinists; it is simply to argue that the ideas espoused by the Huguenots, for example, were not specifically "Calvinist" ideas at all, but represent the adoption and consolidation of the arguments of their more radical Catholic adversaries, including

jurists like Bartolus and Salominio and theologians like Ockham, Gerson, Almain, and Mair. These observations will be debated by specialists for years, but it is important to note that Skinner's somewhat rarefied methodological intelligence has already yielded at least two major reinterpretations of Renaissance and early modern political thought for our critical scrutiny.

Sociologists, however, will quite rightly expect answers to at least two other questions—What has Skinner to say to historians of sociology? What (if any) contribution has he to make to the sociology of knowledge? These are large questions, impossible to answer in this limited space; but some tentative indications might at least be attempted.

The most self-conscious effort to grapple with methodological difficulties in the history of sociology has been the Merton essay[7] referred to above. Despite the laudable features of that argument, however, one might question whether the longed-for "revision" in traditional practices sought therein would not be merely a variation on the same old theme. One question already raised (see Jones 1977, pp. 289–92), for example, concerns the basis for the distinction between the "history" and the "systematics" of sociological theory itself. The latter category, Merton suggests, "represents the highly selective accumulation of those small parts of earlier theory which have thus far survived the tests of empirical research"; the former, on the other hand, "includes also the far greater mass of conceptions which fell to bits when confronted with empirical test. It includes also the false starts, the archaic doctrines and the fruitless errors of the past" (1957, p. 5). The criterion of the distinction would thus be the inclusiveness of one's account, and the criterion of inclusion would be the empirical validity of the theory in question.

This distinction certainly has a descriptive appeal, for a truly "historical" account of past theory *is* likely to be more inclusive than its "systematic" counterpart; and the distinction is frequently applied in this way (see Parsons 1968, p. xiii). But it is a distinction which no historian would accept. A statement about the *Elementary Forms*, for example, does not cease being historical simply because it refers to one of Durkheim's substantively validated propositions. So long as we limit our referents to Durkheim's highly confirmed propositions, therefore, we have no basis whatever for distinguishing between history and systematics. Inversely, of course, a statement about the *Elementary Forms* could hardly *become* historical simply because it refers to a subsequently disproven assertion. Finally, the distinction ignores the fact that a large proportion of our statements about the *Elementary Forms*, and many of the most interesting ones, do not in fact refer to Durkheim's *propositions* at all, but to ideas less amenable to the criteria of truth and falsity. In fact, one of the most interesting facts about Merton's distinction is that it remains purely "descriptive"—that

[7] Merton's concern with the distinction between the "history" and "systematics" of sociological theory actually began in 1948, was continued in *Social Theory and Social Structure* (1957, pp. 4–5), and culminated in the opening essay of *On Theoretical Sociology* (1967, pp. 1–37). This essay draws upon the last two in particular.

is, he does not defend "inclusiveness," or indeed, any other criterion, as the distinguishing characteristic of the history of sociological theory—and the description itself is dropped from the otherwise enlarged version of Merton's argument in 1967.

What, then, *is* the defining characteristic of an authentic history of sociology? Briefly, I think that what is distinctive about history, as opposed to other academic disciplines, is the fact that its statements refer to events and objects in the past; for it is this reference, and the problems arising from it, which requires special methods, techniques, training, and a special relation of theory and data (see Murphey 1973, p. 6; Jones 1977, p. 291). In writing about Durkheim, therefore, the question we should ask is whether our statements refer to actions he could have performed and meanings he might have intended. If they do not, it is difficult to see how they can be statements about Durkheim at all; nor would they be historical statements, because they would not be in any meaningful sense about the past.

But if this distinction is well founded, we must adopt a rather skeptical view of Merton's more positive recommendations for the *history* of sociological thought. Having concluded his (quite justified) criticisms, for example, Merton moves on to discuss the "occupational hazard" of the historian of ideas as one of claiming to find a "continuity" where it did not exist, or failing to find it where it did, with the probability of error decidedly greater in the former case as a consequence of the "incremental" conception of scientific development. The problem of identifying *real* continuities, Merton suggests, is always great; but it takes on a special character when historians ignore the complex interplay between ideas and social structure, place the alleged linkage between earlier and later ideas on center stage, and confine themselves to "distinguishing the extent of similarity between earlier and later ideas, the range of differences being embraced by the terms rediscovery, anticipations, adumbrations and, at the extreme, adumbration*ism*" (1967, p. 9).

From this point on, however, Merton is remarkably uncritical, confining himself to the more precise determination of the conditions under which the historian might say, with confidence, that a *genuine* "prediscovery," "rediscovery," "anticipation," or "adumbration" has occurred. For example, when "substantively identical or functionally equivalent ideas and empirical findings" are set forth by "two or more scientists, each unaware of the other's work" within the span of a few years, we refer to "simultaneous independent discoveries"; when longer periods separate them, the later is a "rediscovery" and the earlier a "prediscovery" (1967, pp. 9–10); when there is "somewhat less of a resemblance, in which earlier formulations overlap the later ones but do not focus upon or draw out the same set of implications," we have an "anticipation"; and when there is an "even smaller resemblance, in which earlier formulations have, quite literally, merely fore-shadowed later ones," we may speak of an "adumbration" (1967, p. 13).

The difficulty with Merton's argument does not lie in the standards thus

imposed. It does not even lie in his largely unexamined assumption that the traditional terminology employed in the history of the natural sciences can be applied uncritically to the history of sociology.[8] The real difficulty, if Skinner's achievement is to carry any force, is that the questions themselves are wrong. A vocabulary like that employed by Merton is simply incapable of rendering an account of anything we might recognize as the act of sociological thinking—for example, of things Durkheim might have been *doing* in saying what he said, or of meanings he might have *intended* in saying them. What we need, *pace* Merton, is not a set of criteria for determining the "substantive identity," "functional equivalence," or "degree of overlap" between earlier and later disembodied abstractions, but the painstaking reconstruction of the real, social activity of sociological thinking.

Our second question—that of Skinner's contribution to the sociology of knowledge—has been treated with considerable sophistication in A. P. Simonds's *Karl Mannheim's Sociology of Knowledge* (1978). Simonds's general position is that Mannheim's readers have largely misunderstood the nature of his work, which was less reductive than hermenuetic in intent. Far from seeking some sort of causal relationship between thought and its existential base, Mannheim consistently recommended the sociology of knowledge as a tool for *understanding*, as a hermeneutic or interpretative method (1978, pp. 26, 30). This is particularly evident, Simonds argues, in the works produced in the decade before *Ideology and Utopia*, which are concerned with the "mediating functions of culture"—that is, mediation between the activity of subjective consciousness and the objective embodiment of this activity in cultural products. How is it possible, Mannheim asks, for such an objectification to serve as a vehicle of meaning? What would be required of a claim to "understand" that meaning authentically?

The most complete answers to these questions are to be found in Mannheim's essay, "On the Interpretation of *Weltanschauung*" ([1921–22] 1952). Objectifications of culture, he argues, are distinctive in the sense that they are presented not only "objectively" or "immediately," in the evidence of direct experience (e.g., the objects studied by the natural sciences), but also "mediately," in the sense that what is there in its own right must also be conceived as standing for something else. Two modes of mediation are particularly important: (1) the object as an *expression* of meaning, in which our attention is referred back to its subjective source; and (2) the object as a *document* of meaning, in which we are directed to some more comprehensive context. The interpretation of *expressive* meaning is highly problematic, Mannheim admits, for frequently a given expression becomes so closely identified with a particular "objective" content that the latter is taken automatically as the equivalent of the former. In the case of the highly standardized connotations attached to artistic symbols in the Middle Ages, for example, the audience "could not help con-

[8] The term "invention" rather than "discovery," for example, would seem a more apt characterization of what sociologists have done, at least until the very recent past.

sidering certain expressive meanings as objectively inherent in certain contents. That this cannot be the case in any *absolute* sense," Mannheim interjects, however, "is clear from the fact that the same events and figures were called upon in the course of history to support many different expressive meanings" ([1921–22] 1952, p. 49).

The expressive dimension of an object's meaning, Simonds summarizes, necessarily takes us beyond the boundaries of the work as it exists "objectively" to the larger context of experience from which it emanates in the conscious activity of a subject: "To grasp the meaning of the intentional act which conferred meaning, it is necessary to make reference to more than the properties of the object in which it is conferred" (1978, p. 45). This is even more true of the second, more comprehensive level of documentary meaning, which "conveys to the interpreter not merely an understanding of its author, but also an understanding of the age, the culture, the 'spirit' of which he partook" (1978, p. 45). And this level of meaning is "intentional" only from the point of view of the recipient or spectator; from the author's or artist's standpoint, it is a wholly unintentional, unconscious by-product.

The difficulty for Mannheim's argument, Simonds continues, arises when we move from his early concentration on objects of art to the interpretation of written texts. Here it might be agreed that there is a distinction between the meaning of a natural object and the meaning of a text, but only in the sense that the distinction concerns the "frame of reference" brought to bear upon the objective evidence presented in each type of object. By such a view, "expression" is distinguished from other instances of objective meaning only insofar as a special frame of reference—the rules of the grammatical and semantic use of language—is shared by the writer and the reader of the text. Once the writer has employed these rules to produce the text, its meaning is to be found "objectively"—and exclusively—therein (1978, pp. 52–53). The advantage of such an argument, Simonds observes, is that it confines interpretation to an object which is immediate, palpable, before us—"the poem," Leavis argues, "is a determinate thing; it is *there*" (1953, p. 174)—and the assumption is that this, and this alone, makes it possible to speak of a "determinate meaning"—for example, "if interpretation is not grounded in the objective evidence of the text itself, . . . then how are we going to adjudicate among conflicting accounts of what it means? The meanings of a text will become as disparate as the external sources in terms of which it is interpreted" (Simonds 1978, p. 56).

This, of course, is the theory of the "autonomous text," espoused with considerable warmth by the New Critics and tacitly assumed by defenders of the Great Books approach to the study of Western civilization. Simonds even suggests that it underlies many of the programmatic efforts of social scientists to purge data of all interpretative reference by defining them in formal and quantitative terms; and of course it is manifestly present in the lamentable practices described in the first paragraph of this essay. Simonds's problem, however, is that Mannheim himself did not develop the case against such a doctrine of textual autonomy, for the simple reason

that the prejudice in favor of such a case was not as pronounced or influential in his day as in ours. Convinced that the ideal of the autonomous text is specious, however, Simonds wishes to defend the need for expressive and documentary reference in the interpretation of cultural objects. He wishes to do so, moreover, without sacrificing the principle of the determinacy of meaning; for, unlike some of the proponents of a relativist hermeneutic, Simonds urges that if expressive meaning is to be a suitable source of evidence in the social sciences, determinacy is an inescapable requirement ("indeterminate evidence" being a contradiction in terms). The problem with the doctrine of the autonomy of the text, Simonds must thus argue, is not that it insists upon the identification of determinate meaning, but that it is in principle incapable of attaining this goal. Extratextual reference, Simonds must prove, is in fact a prerequisite to the determination of expressive meaning. How is he to make his case?

It is here that Simonds turns to Skinner, summarizing Skinner's arguments under three general observations. First, the rules of language as they are provided in dictionaries and grammars simply do not suffice to establish any determinate meaning; they merely indicate "meaning possibilities" which show continuous historical variation, and among which we must adjudicate through reference to extratextual sources. Even the proponents of textual autonomy, Simonds observes, are found to make such interpretative reference (as, indeed, they must) in order to fix the boundaries of the allegedly unproblematic "text." Second, in order to understand a text, it is necessary to grasp not only what its words and sentences refer to but also the nature of the act performed in that expression.[9] In expressing oneself, Simonds points out, a person is also *doing something,* and knowing what it is makes a difference in our understanding of what the expression means; but once again, there is nothing in the words or grammatical arrangement of the "determinate text" which can answer this question (1978, p. 58). Third, the more complex an expressive object, the more important it is that we construe its *point* correctly—that is, we must be able to assign appropriate weight to various passages, to distinguish what is and is not implied, to specify the relations of various parts of an argument to one another, etc. Judgments of this kind, however, are possible only if the project as a whole is taken in view, something difficult if not impossible from a position within the text itself. "Against the figure of the objective content of an expression," Simonds argues, "there always lies a ground in terms of which the problem has been conceived and defined" (1978, p. 60). And, like Skinner, Simonds finds the classic expression of this argument in Collingwood's *Autobiography* (1970, p. 31).

Relying heavily on Skinner, therefore, Simonds concludes that "the objective meaning of the autonomous textual object must be judged insufficient to determine [respectively] the sense, the force, or the point of an expression . . . and therefore we must conclude that the interpretative in-

[9] This is the classic argument of J. L. Austin's *How to Do Things with Words* (1962), on which Skinner places so much emphasis.

junction to respect the autonomy of the text cannot be justified by the goal of securing determinate meaning, and that in fact it must be rejected precisely for the reason that it precludes the achievement of this goal" (1978, p. 61). Skinner thus provides support for Mannheim's theory of the "mediate" interpretation of cultural objects—at a point, moreover, at which Simonds himself admits that Mannheim's arguments were not fully developed. But Simonds's more general argument goes quite beyond a mere reinterpretation or defense of Mannheim's sociology of knowledge: "The arguments that Mannheim offered (tentatively and not always very coherently) during the twenties," Simonds concludes on his last page, "often seem to deliver us directly to the doorstep of the most interesting and suggestive contemporary efforts to reappraise and restructure social theory" (1978, p. 187). These efforts, recently summarized in Bernstein (1976), include not only those of Skinner but also those of Gadamer, Toulmin, Habermas, MacIntyre, Winch, Kuhn, and many others; and if successful, there seems little question that the task of the sociology of knowledge—and sociological theory generally—will be permanently transformed.

Finally, these arguments provide us with some straightforward answers to two of the more authoritative reviewers of the *Foundations of Modern Political Thought*. At the conclusion of what was otherwise an excellent and highly laudatory essay, for example, Keith Thomas added: "The truth is that historical texts can have a valuable meaning for modern readers, even if it is not the meaning which their authors intended. Indeed, it is out of their misreadings of past texts that many thinkers construct a future" (1979, p. 29). From what has been said above, it should be clear that this almost platitudinous remark rests on a failure to distinguish clearly between the "expressive" and "documentary" levels of mediate interpretation. Clearly, it in no way relieves the historian of the responsibility for the recovery of past intentions. "I see no impropriety," Skinner has emphasized, "in speaking of a work having a meaning for me which the writer could not have intended. Nor does my thesis conflict with this possibility. I have been concerned only with the converse point that whatever a writer is *doing in* writing what he writes must be relevant to interpretation, and thus with the claim that *amongst* the interpreter's tasks must be the recovery of the writer's intentions *in* writing what he writes" (1972*c*, p. 405). Thus Thomas's remark in no way contradicts or even touches upon anything Skinner has argued, though in less skillful hands it might be construed as doing so.

A second, quite different criticism has been advanced by Anthony Quinton. After observing that the *Foundations* provides but austere reference to the conventional political history of the period, even less information about the socioeconomic positions of the theorists, and less still about their personal biographies, Quinton concludes that the book has "no tincture of the sociology of knowledge about it" (1979, p. 37). Once again, however, it seems that the critic is encumbered with precisely that "mechanical reductionist" version of the sociology of knowledge which, according to Simonds, Mannheim and Skinner (among others) would quickly dismiss.

Therefore, even though one might occasionally wish for more mundane information to balance Skinner's focus on *mentalités*, there is no question that ideas, beliefs, values, and language are just as much "social facts" as economic development or class structure—and both categories, after all, require abstraction.[10]

While one reads the *Foundations of Modern Political Thought*, however, such wishes are occasional indeed; for it is precisely when Skinner has completed his more traditional obligations, and sets about the history of ideologies, that the effect is most dazzling—not unlike that of watching a highly skilled athlete and being so seduced by the grace and skill of the performance that the score itself lapses into momentary insignificance. But in this case, the achievement can hardly be overestimated. One can only hope that sociologists and historians will take Skinner's example to heart, so that the sociology of knowledge might reclaim its task and the "history of sociological theory" might become the *history* of sociological theory after all.

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Book Reviews

On the Margins of Science: The Social Construction of Rejected Knowledge. Edited by Roy Wallis. Keele: University of Keele, 1979. Pp. 337. $12.00 (paper).

Bruce Stephenson
University of Chicago

The essays in *On the Margins of Science* concern two themes: the stresses produced in a scientific community by persistent division in its ranks, and the role of social interests in the evaluation of theories advanced as scientific. As Roy Wallis points out in his introduction, research in these topics has been stimulated by the inability of philosophers to determine objective criteria that might account for the acceptance or rejection of scientific theories. We must now recognize the evaluation of a new theory as a nontrivial act of the scientific community, one which cannot be reduced to a formula. R. G. A. Dolby, in the initial essay, reviews the philosophical problems and proposes a framework for sociological analysis of "deviant" science.

The second theme is the more ambitious, for it makes the truth of scientific theory dependent upon social processes, and hence subject to sociological analysis. Whether this is an interesting thing to do depends upon what one considers as the "social interests" affecting scientific evaluation. The essay by B. Barnes and D. MacKenzie presents the most aggressive theoretical claims for this kind of sociology of science. Chiding earlier studies for their all-too-common inclination to smooth over seemingly irrational elements in the history of science, they urge a "naturalistic" appraisal of struggles between adherents of competing scientific theories. They accept almost anything, from a shared desire to solve a problem (p. 53) to direct expression of social issues external to science (p. 54), as a social interest. As a case study they take the controversy at the beginning of this century between the two statisticians Karl Pearson and George Yule. These men understood one another well enough and yet disagreed. Barnes and MacKenzie take the disagreement as a puzzle and suggest that it relates to the situations of Pearson the eugenicist, allied with the professional middle class, and of Yule the conservative member of the Royal Statistical Society. These are sweeping statements and not very closely argued, but article format after all allows little space.

The greater problem lies in their objective: to study "naturalistically" the evaluation of scientific theories. One will never understand this sort of complex process by approaching it from outside, as a naturalist, with

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the ready-made residual category "social interests" to patch up whatever holes one finds in a naive account. Naturalistic study may be appropriate for the controversy itself, but the scientific evaluations involved must first be understood internally. Without such understanding it is premature, indeed irresponsible, to infer social interests merely because one finds ambiguity.

Articles by P. Wright, on astrology and medicine in 17th-century England, and A. J. Webster, on acupuncture, are still more vague. Wright attempts to attribute the successful establishment of medicine to its compatibility with the ideology of capitalist production. Webster suggests, ponderously and largely in the impersonal passive, that professional interests have accompanied the scientific evaluation of acupuncture in recent years. He introduces the concept of "socio-cognitive metonymy" to denote "a contiguous but non-reducible relationship between cognitive and social processes whereby the constitutive features of scientific cognition are effectively signified by and in part genetically linked to the social processes of the scientific institution" (pp. 121–22). His analysis is about as penetrating as one would expect from so shapeless a tool.

S. Shapin's account of anatomical studies surrounding the 19th-century debate over phrenology contains a great deal of information in an argument that is hard to follow. He finds social interests, broadly defined, underlying four increasingly esoteric points of anatomical contention. (Twice he seems to oppose social interests to "idle curiosity" as possible motivation for scientific investigations [pp. 147, 152].) Some of his conclusions are undoubtedly sound. Controversies are fueled by professional interests, and they often do generate new knowledge.

The other studies in the volume concentrate, with more success, on the peculiar tensions raised by unresolved controversy in science. The ideal of objective knowledge, particularly in the simplistic popular ideology of science, is dirtied by persistent controversy. B. Wynne's account of the way in which Nobel prizewinner C. G. Barkla was ever-so-gently isolated, and quietly tolerated, is exceedingly interesting. More perplexing is a tactical analysis of the debate on parapsychology by H. M. Collins and T. J. Pinch. They firmly and repeatedly reject a distinction—between "constitutive" and "contingent" forums in science—around which their entire paper is structured. Noting that a principal tactic of parapsychologists is that of "physical metamorphosis—of changing themselves into scientists," they take it as evidence that the content of science is socially defined. Otherwise, "the appropriate strategy would surely, be simply to perform and report the 'definitive' experiment!" (p. 241). This verges on parody.

E. Barker has contributed a sensitive and thoughtful study of the "scientific creationist" challenge to the theory of evolution. Other examinations of tensions and tactics deal with 19th-century mesmerism (T. Parssinen) and spiritualism (J. Palfreman), parapsychology (P. Allison), sea-serpent sightings (R. Westrum), and UFOs (J. Blake). Less ambitious in their theses, these articles are more successful in execution.

Contemporary Hermeneutics: Hermeneutics as Method, Philosophy and Critique. By Josef Bleicher. London and Boston: Routledge & Kegan Paul, 1980. Pp. xi+288. $28.00 (cloth); $14.00 (paper).

Alan Sica
University of Kansas

Still another specter haunts sociology, "hermeneutics" by name. In *Contemporary Hermeneutics* Josef Bleicher has provided the first account of nontheological hermeneutics written expressly for students of social theory. It is therefore by definition an important event, particularly for sociologists interested in European social and philosophical thought. The book's real strength, however, comes less from Bleicher's own writing than from the four translated essays around which his introductions and commentary are built. But what is hermeneutics, and why should contemporary sociologists interest themselves in a scholarly field which originated during the Reformation?

Modern hermeneutics was first formed during the late 18th century by German philologists who required "rule books" to interpret confidently ancient or foreign texts. Biblical hermeneutics is the most famous of these philological activities, but juristic hermeneutics is perhaps the most important, since jurisprudence is by definition a hermeneutical (interpretive) discipline. This type of interpretive theory does not concern itself as much with the substance of a text at its ultimate, meaningful level, as with providing readers guidance in avoiding gross errors of interpretation due to the faulty understanding of words, phrases, sentences, and so on. Hence its name, "grammatical hermeneutics." Friedrich Schleiermacher, who contributed to this part of the field, also broadened the scope of interpretive theory tremendously by introducing the notion—embodied for sociologists in the concept *Verstehen*—that the hermeneuticist cannot rest content after understanding words on the page, but must in addition penetrate the creative process which gave rise to them. And to do this one must, of course, "understand," in the most radical sense, the author.

Today two branches of hermeneutics are flourishing as direct descendants of Schleiermacher's work. On the one hand, Emilio Betti and E. D. Hirsch have called for "objective" hermeneutics, capable of distinguishing apodictically a "true" from a "false" reading. On the other, Hans-Georg Gadamer and his many followers (all influenced by Heidegger) postulate a "philosophical hermeneutics," which exceeds even Schleiermacher's expectations in that it pursues not only texts and their authors but also the "movement of Being" as defined in *Sein und Zeit* and subsequently elaborated by Gadamer himself. Paul Ricoeur, another central figure in today's hermeneutics, has tried to bridge the gap between these two warring camps.

The reason Bleicher's book is so very useful is that within a fairly short (though mercilessly dense) monograph, one can follow, through his re-

marks and the translated essays, the entire history of the field during the past 25 years. Bleicher has translated Betti's important essay, "Hermeneutics as the General Methodology of the *Geisteswissenschaften,*" into English for the first time, which corrects the previous omission of Betti from debate, and is a great service. Gadamer's famous essay on the "hermeneutic claim to universality" also is reprinted, as is Habermas's equally famous "rebuttal" (not entirely successful). And a synthetic effort by Ricoeur, which extends hermeneutics into phenomenology, rounds out the set of translations. Bleicher also manages to discuss Dilthey, Heidegger, Bultmann, Karl-Otto Apel, "materialist hermeneutics" (an exotic variety), and phenomenological hermeneutics beyond Ricoeur.

Bleicher has only about 150 pages to himself, and because he tries to cover so much the density of his commentary is at points excessive. An additional problem with the book, though reasonably minor, is the omission of Schleiermacher himself. He is discussed very little, and it would have been truly useful to have reprinted some passages from his recently translated *Hermeneutic Manuscripts* (F. D. E. Schleiermacher, *Hermeneutics: The Handwritten Manuscripts,* ed. Heinze Kimmerle, trans. James Duke and Jack Forstman [Missoula, Mont.: Scholars, 1977]), since they so strongly influenced Dilthey. It would also have been illuminating to have reproduced Dilthey's brief history of the field, which might have helped relieve the reader of strain upon meeting the first translated essay, by Betti. This 44-page work is Betti's own condensation of his magnum opus, some 800 pages long. It would have been wise, perhaps, to have placed Gadamer's essay first, since he is by far the clearer writer of the two. But this would have violated Bleicher's own obvious preference for Betti's version of "objective" hermeneutics, a preference which I find difficult to understand.

What are sociologists to make of this field, then? The first, most obvious application is to a reliable appropriation of our own theoretical and substantive legacy—simply put, to the accurate reading of texts. If one reflects on how many versions of Marx, Durkheim, and Weber flourish in the journals, the utility of a disciplined theory of interpretation becomes obvious. Another, more adventurous path (which Bleicher identifies as a third type, "critical hermeneutics") was first pointed out by Jürgen Habermas over a dozen years ago. It involves his notions of the "ideal speech situation," "rational consensus," "distorted communication," and his "theory of communicative competence." This approach has been vigorously taken up by Apel and many other philosophers and social scientists, and the results of their continuing work may well be the "dialectical-hermeneutical science" Habermas insists should be developed as the culmination of "critical theory."

While there are other treatments of general hermeneutics available today, only Bleicher's invites immediate use by social scientists. With it hermeneutics may pass beyond its arrested stage as a mere buzzword.

Communication and the Evolution of Society. By Jürgen Habermas. Translated by Thomas McCarthy. Boston: Beacon Press, 1979. Pp. xxiv+239. $11.95 (cloth); $5.95 (paper).

David Zaret
Indiana University at Bloomington

Communication and the Evolution of Science should be an especially satisfying book for readers familiar with the problems and projected solutions that have preoccupied Jürgen Habermas over the past two decades. Few surprises are in store; continuity and change seem equally to characterize recent trends in critical theory. The five essays collected in this volume focus on ideas that previous work identified as crucial to the progress of critical theory. These ideas were undeveloped, existing only as tantalizing remarks intended for future elaboration. Now, after several major books that posed critical problems, Habermas offers us some substantive solutions, although they are still, as he observes, somewhat provisional. Perhaps it is the fate of critical theory, given the enormity of its tasks, to remain forever in a frustratingly provisional form.

These essays admirably display the ambition and breadth of Habermas's critical theory, and they appear in order of decreasing level of abstraction. The first, pivotal essay outlines a theory of "universal pragmatics." This theory of communication seeks to establish universal conditions of possible understanding. It complements two well-established fields of study: analytic and structuralist philosophies of language and sociolinguistics. Unlike the former, it does not sever formal analysis of semantic and syntactical rules from pragmatic use of language. It studies necessary conditions for establishing a reciprocal consensus through communicative action, a consensus that presupposes the satisfaction of several types of validity claims. Unlike sociolinguistics, universal pragmatics is concerned with *formal* determinants of meaning, with universal and unavoidable presuppositions of communication, and not with empirical strategies that reflect specific social situations.

According to Habermas, universal pragmatics is a framework for analyzing cognitive and moral aspects of communication, for ascertaining the rational basis of norms, and for incorporating a concern with normative structures at the core of a revitalized historical materialism. In fact, four of these essays originally appeared in Germany in a book whose title explicitly announced this concern with reconstructing historical materialism (*Zur Rekonstrucktion des historischen Materialismus* [Frankfurt: Suhrkamp, 1976]). The title given to this translation is, however, equally acceptable, inasmuch as this reconstructed doctrine is a theory of social evolution that stresses cumulative aspects of normative as well as technical changes.

Building on the theory of universal pragmatics, the next essay borrows heavily from work by Kohlberg and Piaget to provide a developmental

scheme of the individual's acquisition of communicative competence, a competence that has both cognitive and moral dimensions. This essay in turn prepares for the next two, which argue that the genesis of different types of cognitive and moral truth claims in individual development also occurs in the evolution of legal and moral institutions.

Here Habermas follows up on earlier work in which he suggested that Marxism incorporate interaction along with labor as a fundamental category of theory. He pursues this task by using his theory of communicative competence to show how normative structures undergo rationalization in social evolution. Communicative action establishes understanding about mutual expectations which, over time, increasingly acknowledge that different validity claims could in fact be satisfied as the justification of these expectations. In this sphere, rationalization means the extinction of force, and an end to systematically distorted communication so that reciprocally raised validity claims could be applied to test normative expectations. Moral and legal institutions evolve toward this state of affairs just as individuals do in arriving at more adult stages of moral consciousness. A parallel ontogeny of individual and society is, of course, highly questionable.

But this departure in Habermas's work clearly follows from theoretical priorities that define critical theory. His earlier flirtation with psychoanalysis was motivated by the conviction that an autonomous ego—the therapeutic ideal—is inextricably linked to an emancipated social order. For the same reason, the founders of critical theory—Adorno, Horkheimer, Marcuse—often presented it as the heir to the philosophic tradition of Kantian rationalism, in which the autonomous mental activity of subjects imposes order on raw experience. Currently, the critical theory of Habermas relies on these sources, as well as on analytic and structural philosophies of language, which are also compatible with this concern with the autonomous ego.

The last essay discusses legitimation problems of the state in capitalist society. Its focus on a motivational crisis as the root of this problem is reminiscent of Daniel Bell's analysis of the antinomian strain in advanced capitalist culture. Habermas does not go beyond the ideas contained in his *Legitimation Crisis* (Boston: Beacon, 1975). However, readers will be more than satisfied with the new developments in the first four essays.

The Making of Symbolic Interactionism. By Paul Rock. Totowa, N.J.: Rowman & Littlefield, 1979. Pp. xiii+268. $19.50.

Barry Glassner
Syracuse University

Paul Rock comes across as the sort of participant observer whom participant observers distrust. As a British sociologist visiting the United States and sympathetically researching symbolic interactionists, he had the op-

portunity for both active participation with the group and the critical distance of an outsider who will be among the group only temporarily. At places in his book, *The Making of Symbolic Interactionism,* this balance is achieved, and Rock successfully portrays the symbolic interactionists as actors whose behaviors are extensions of their methods and assumptions. In much the same way as quantitative practice leads to considering the world in increasingly mathematical ways and linguistic analysis results in unending extensions of rhetoric, the pragmatism underlying symbolic interactionism leads to an emphasis on immediate, irreproducible experience (pp. 178–85).

Rock correctly accuses symbolic interactionists of multiplying the number of small fieldwork studies they conduct without concurrently analyzing these studies to identify common findings or grounds for generalization. Because symbolic interactionists learn to be impressed by the uniqueness of events, phenomena, settings, and symbolism, they hesitate to do much more than describe them. In addition to the problems shared with kindred descriptivists, symbolic interactionists also suffer from an inability to evaluate contradictions. When one explanation contradicts another, the symbolic interactionist begs off an investigation into the evidence for the competing claims by treating the opposing views as genuine given where they come from: "The interpretations of others become ethnographic data or distractions rather than competing or complementary sociologies" (p. 91). If one person in a field site says something is *A* and the other says it is *not A,* or if one sociological theory provides an *A* explanation and another a *not A* explanation, the symbolic interactionist seeks to understand how each side came to hold its views. Such a response is fruitful in explaining the development of interpretations, but it conveniently neglects the fact that *A* and *not A* cannot both be true.

At the same time, the human mind in symbolic interactionism extends almost infinitely. As Herbert Blumer has argued, mind makes the world. He has gone so far as to claim that "the nature of an object—of any and every object—consists of the meaning that it has for the person for whom it is an object" (*Symbolic Interaction* [Englewood Cliffs, N.J.: Prentice-Hall, 1969], p. 11). Rock points out that with such an ontology some of sociology's most inconvenient problems evaporate. No one can ever really be mistaken, no one is alienated, there is no false consciousness, and society can be observed directly.

Put simply, symbolic interactionism "maintains an aversion to protracted philosophical disquisition [and] upholds the alleged affinity between lay and scientific reasoning" (p. 167). In revealing just how problematic these twin positions turn out to be, Rock's text excels any I have read. For that reason it should be required reading for symbolic interactionists. As I suggested initially, however, those who are active symbolic interactionist researchers are likely to distrust much of his account.

One gets the sense that Rock has not drawn himself very close to the actual practices of symbolic interactionists. He utilizes solely their written commentaries about their work while claiming that the significant discus-

sions are oral (p. 5). There is no evidence that he himself has been very active as an empirical researcher, or that he attended symbolic interactionist gatherings in preparing this book. Most fundamentally, he does not display an appreciation for the unique strength of symbolic interactionism, namely, the ability of some of its adherents to uncover how particular groups of people collectively form and maintain the social organization and interpretation of their worlds: how persons can engage in homosexual activities and be considered heterosexuals by their own and others' definitions, how a group of patrolmen can become an elite police corps, how group interactions produce drug effects, and so forth.

Some of the discussions are correspondingly distant from actual practices of symbolic interactionists. Rock compliments symbolic interactionism for "its acceptance of uncertainty and [for adopting] a modest scope" (p. 2). But many symbolic interactionist papers and books make grand claims and read as if their authors are quite certain that they are correct. When he contends that symbolic interactionism is "conveyed by imitating the actions of its adherents, not by reading and discussing their works" and that "fieldwork cannot become a topic for extensive teaching and examination" (pp. 183–84) he has somehow missed the many conference hours, running mail correspondence, and published debates between symbolic interactionists and others regarding their research and theory, as well as the dozens of fieldwork courses taught every semester (at my university, at the graduate level alone, there are at least four in most years and a great deal of debate among the instructors).

Similarly, although Rock's chapter on the intellectual sources of symbolic interactionism is an important contribution to the history of social thought, he fails to appreciate which of these sources actually affect contemporary practice. Thus, Simmel, Hume, Hegel, and Kant receive prominent discussion, but, fortunately or not, their influence is difficult to find in contemporary symbolic interactionism. Rock blames "the emerging union of pragmatism and formalism" for the symbolic interactionists' unwillingness to produce much abstract theory, deduction, or a clear position on the dualistic problem of the observer's relationship to the observed (p. 79). Since neither pragmatists nor formalists are quoted or much studied by contemporary interactionists, I would suggest that these problems result from the *absence* of a philosophical tradition and dialogue, and from an emphasis on one basic method of inquiry.

Rock also spends a good deal of space repeating the standard criticisms leveled at symbolic interactionists—they ignore macrostructure; they accept everyday accounts and other surface appearances as true; and some researchers betray the confidence of those they study ("there can be few professional sociologists who do not know the identity of 'Middletown' for instance" [p. 204]).

The Making of Symbolic Interactionism is like many observer accounts. Its strengths come when it reveals important features within or beyond what the participants see of their own world. Its weaknesses result from misconstruals or simplifications of what actually goes on in that world.

Social Interaction and Consciousness. By P. D. Ashworth. New York: John Wiley & Sons, 1979. Pp. 227. $34.95 (cloth); $17.95 (paper).

Bernard N. Meltzer
Central Michigan University

One of the central issues of social psychology is the relationship between social interaction and consciousness. This issue concerns both the social sources of individual awareness of the world and the patterning of human association by conscious social actors. P. D. Ashworth, principal lecturer in social psychology in the Department of Applied Social Studies, Sheffield City Polytechnic, has written a highly compendious monograph on the subject. *Social Interaction and Consciousness* includes a 17-page list of references (over 500 items) for a text of only 196 pages. Each chapter abounds with such a tightly compressed profusion of research findings, judicial appraisals of old and new ideas, and reconceptualizations as to render a brief review quite difficult.

Ashworth adopts the theoretical framework of symbolic interactionism, which holds that human interaction requires conscious, symbolic behavior but that the very capacity for such behavior is the product of a more rudimentary form of interaction, gestural communication. Starting from this premise, the book mines a vast British and American literature from both the psychological and sociological "wings" of social psychology in an effort to elaborate the interaction-consciousness nexus. Along the way, the reader encounters numerous pithy insights, for example: "Society is a precondition for the emergence of mind and self, so interactionism cannot be reductionist, even if (on the other hand) social processes are possible only insofar as individuals engage in significant communication" (p. 15).

Appraising G. H. Mead's position, Ashworth treats such topics as: empirical work in developmental psychology that fills in details of Mead's sketchy account of the transformation from gestural to symbolic interaction (e.g., the case for certain innate capacities as preconditions for human interaction); the ambiguity of interactionist views on determinism, as evidenced in treatments of the important notion of "definition of the situation"; and the debate over whether Mead's "social behaviorism" constitutes a behavioristic psychology. On this latter topic, he argues the negative side fairly persuasively.

In each of the subsequent chapters, Ashworth treats an analytically separable component of both interaction and consciousness: language, perception of self and others, emotions, motivation, and attitudes. That these topics constitute an organic unity becomes increasingly clear as the book progresses. For example, the discussion of language considers both mind and communication—hence, interaction—as symbolic processes. Further, it reviews the topic of linguistic relativity—that is, the determination of thought by language, and the function of language as the vehicle of thought—as well as addressing the question whether symbols are species-

specific to humans. The intimate connection between language and all the topics treated after it is first evident in Ashworth's consideration of the place of linguistic categories in perception. The key idea here is that it is misleading to conceive of the perceptual process as a sequence in which meaningless sensations are later interpreted. To become aware of objects is to become aware of them as already symbolized, already meaningful. This idea emerges out of a comprehensive treatment that includes such topics as proxemics, kinesics, and eye movements, as well as the issue of whether emotional expression is universal or culturally relative.

In chapter 5, "Perceiving Oneself," Ashworth clarifies Mead's account of the formation of the self as a product of social interaction. He properly points out that if the self were completely determined by the views of others, such human capabilities as "role distancing" could not occur. Similar insights illuminate the treatment of the relationships between emotions and social interaction. One dimension of these relationships is seen in the cultural relativity of emotional experience. Diverse vocabularies of emotion are reflected, for example, by "shame cultures" and "guilt cultures."

A chapter on motivation (chap. 7) discusses the attribution of motives to oneself and to others, the conditions under which motives are attributed, and the vocabularies of motives available to people in different times and places. Ashworth then considers the related topic of attitudes (and, inescapably, cognitive dissonance). Their relationship to behavior, he asserts, reemphasizes the importance of the definition of the situation, for attitudes can be seen as "motivating" and "suggesting a content for" situational definitions. Thus, like motives, attitudes are *reasons* for—not causes of—behavior.

In his conclusion, Ashworth mentions approvingly a recently noted convergence between symbolic interactionism and psychoanalysis. Psychoanalysis can be viewed as "a linguistically-based procedure aimed at the interpretation of the [individual's] communicative behavior" (p. 196).

Given the broad scope of the book and the overall high quality of Ashworth's coverage, it may appear captious to make the following points: (*a*) the significance of the self in social interaction is afforded skimpy treatment; (*b*) the character of human interaction as *constructed* is underemphasized in the discussions of motivation and of the relationship between attitudes and behavior; and (*c*) Ashworth overlooks several important, relevant American works, for example, those by A. R. Hochschild and T. D. Kemper on emotion and by I. Deutscher and A. E. Liska on the attitude-behavior controversy.

American Journal of Sociology

The Invisible Children: School Integration in American Society. By Ray C. Rist. Cambridge, Mass.: Harvard University Press, 1978. Pp. xii+289. $14.00.

Morton Weinfeld
McGill University

In *The Invisible Children* Ray Rist describes the first year of a voluntary integration program at the Brush (Elementary) School in Portland, Oregon, in 1973–74. Some 30 black students in grades 1–4 were bused to this suburban, all-white, middle-class school, with just one or two black children allocated per classroom. Drawing on year-long participant and nonparticipant observations, as well as on interviews and discussions with teachers, students, the principal, and parents, Rist presents a chronology of that first year.

In an introductory chapter he presents four models of school integration, of which two are of interest in this study. The first, racial assimilation, is the model implicitly and explicitly operative at Brush—black children are to be treated in color-blind fashion, rendered invisible, with no emphasis on cultural or racial specificity. Rist's preference is for the second model, racial pluralism. He acknowledges this at the end of the book, and that preference may have subtly influenced the tone and style of presentation of the data, accentuating the apparent failings of the program.

The book is exceptionally well written, and the recorded observations are uniformly topical and interesting; they bear not only on the integration issue but also on the general nature of contemporary schooling in America. We see again the pursuit of order and discipline, the misuse of IQ scores, and the harmful effects of teacher expectations and peer pressures on learning. While the data are of clear sociological relevance, they also help flesh out real characters about whom the reader cares, notably the young black students and their well-meaning teachers. This valuable study exemplifies a major advantage of good qualitative research—the provision of insight and a dynamic understanding so often lacking in quantitative studies of the effects of integration, notably one-shot surveys with an input-output (black box) paradigm.

In Rist's view, the Brush experiment was unsuccessful. After a year, most of the black students had settled near the bottom of the ability groupings in their respective classrooms. Skill deficits which they brought to Brush were not appreciably overcome.

Why did the black students perform poorly? In the narrative portion of the book several possible explanations, anticipated in the literature, are culled from the data: cultural bias in the curriculum, unconscious racism and paternalism on the part of the white staff, poor home environments typified by lack of sleep or nourishment for some of the black students, low parental participation in the school's affairs, social ostracism by peers and eventually by teachers. Yet the root cause of the failure lies in the

underlying model of racial assimilation which characterized the Brush program. Rist argues further that any evaluation of the success or failure of an integration effort must consider the degree to which a program affirms "the diversity of cultural life in America" regardless of any hypothetical increase in scholastic achievement.

There are obvious limits to the generalizability of this case study, as Rist is aware. Blacks were only 5.6% of Portland's population in 1970, and the racial climate there was generally calmer than in most northern cities. The numbers of bused children were particularly low, leaving the classrooms without a critical mass of black students to sustain the identity of the individual black child and making the achievement of some form of racial and cultural pluralism practically impossible. Moreover, the program was voluntary, and the receiving school and the white parents were not hostile to the incoming blacks (perhaps because of their small number). Many of the good-willed white teachers had never had any contact with black students. A tragicomic example of this unfamiliarity with black cultural sensibilities was the principal's directive that henceforth chocolate milk was to be called "black milk."

In the final two chapters of the book Rist moves beyond his data to argue the case for pluralistic education in America, not only between schools and within schools, but even within classrooms. He advocates a pluralism of teaching styles within the classroom, but does not specify exactly what such a "new classroom approach" might be, other than a shift away from hierarchy and competitive evaluation. He also advocates curriculum revision, "a rewriting of history" that would stress the minority role in America and boost the self-esteem of minority students. Rist emphasizes the importance of making schools more enjoyable and humane for all students through forms of "social engineering" designed to break the status hierarchy based on and reinforced by race, social class, and academic performance which is found in integrated settings. Efforts at integration might be more successful through massive residential desegregation that could reestablish an integrated neighborhood school as an alternative to busing.

The reader may choose to accept or reject Rist's description of the Brush experiment as a failure, as well as his advocacy of a pluralistic model of education. We might remember that there are certain limits to cultural pluralism in *public* education, if we accept some minimal, uniform socialization as a legitimate objective of schooling (Nathan Glazer, "Ethnicity and the Schools," *Commentary* [September 1974], pp. 55–59). As to whether the Brush experiment failed the bused black students, it is of interest to note that black parents were uniformly positive about the experience, and all wished to send their children back to Brush (pp. 241–42). Rist speculates that these parents may not have been fully aware of everything that transpired in the school, or may have thought that mixing with whites was "ipso facto" beneficial, or, like most white parents, tended to accept as valuable any education afforded their children. These hypotheses are not tested.

An alternative hypothesis—also not tested—is that the black parents did have a sound knowledge of their children and their previous schooling compared with the Brush experience, and of social and cognitive benefits that Brush might bring to their children. If we grant these black parents the validity of their own construction and understanding of their world, the integration program at Brush, Rist's objections notwithstanding, was a success.

Summer Learning and the Effects of Schooling. By Barbara Heyns. New York: Academic Press, 1978. Pp. xviii+326. $21.00.

Paul Lindsay
University of North Carolina at Greensboro

As Barbara Heyns tells us in this study, negative findings on the effects of schooling during the past two decades have made educational research "a prime contender for the century's most dismal science" (p. 186). Perhaps the most often quoted statements are, "Compensatory education has been tried and it apparently has failed" (Arthur R. Jensen, "How Much Can We Boost I.Q. and Scholastic Achievement?" *Harvard Educational Review* 39 [Winter 1969]: 2) and "Schools bring little influence to bear on a child's achievement that is independent of his background and general social context" (James S. Coleman et al., *Equality of Educational Opportunity* [Washington, D.C.: Government Printing Office, 1966], 1: 325). Using an imaginative longitudinal research design and sophisticated analytic methods, Heyns shows clearly that such dismal conclusions were premature.

The research reported in *Summer Learning and the Effects of Schooling* succeeds in overcoming some serious limitations of earlier work. Academic achievement of approximately 3,000 sixth- and seventh-grade students in Atlanta, Georgia, was measured in fall, spring, and the following fall. Gains from fall to spring are taken as indicators of learning during the school year. Gains (or losses) from spring to fall are indicators of summer learning. Thus, the summer presents a natural experimental situation, with some children exposed to schooling and other children not exposed. This overcomes one of the problems that affected previous results—the lack of an independent variable that varies. This study design also makes it possible to disentangle the effects of family and schooling, which are both operative during the school year. Data on the cognitive development of children not in school can be used to measure the effect of family environment on learning by comparing children of different socioeconomic status (SES) and race.

Among the central findings in part 1 of this book are: (1) Children learn more when they are in school than when they are not. This pattern is found for both whites and blacks at all levels of SES. (2) The gains in

learning during the school year are substantively the same for different SES and racial categories. While schools do not eliminate the absolute gaps in achievement between groups, the gaps do not become much larger while children are in school. (3) In contrast, socioeconomic differences in achievement become larger when children are not in school. High-SES children of both races continue to learn in the summer (although not as much as during the school year), but low-SES children learn less during the summer, and in some categories there is a loss in achievement. Family differences, not school experiences, account for the increasing inequality in achievement as children grow older. These findings are presented in graphs and tables using both grade equivalent and raw scores. A path analysis in the tradition of status-attainment research gives further empirical confirmation of the findings.

Summer activities and their effects on learning are assessed in part 2. Summer schools seem to have limited effects on cognitive growth, perhaps, the author suggests, because of their short duration or lack of academic objectives. Most nonschool activities, structured or unstructured, and vacation trips also seem to have little effect on achievement. Reading and the use of libraries, however, are highly related to learning during the summer. Using number of books read and number of hours spent reading on a typical day as measures of reading, Heyns shows, through a series of regression equations and cross tabulations, that reading has independent positive effects on learning for all groups. Next, whether a child uses a library regularly is a major determinant of the amount of reading the child does. For white children, the effects of library use are almost as strong as the effects of family income and parental education. For blacks, the importance of library use is even greater than that of family SES. Finally, the distance a child lives from the library is an important factor in whether he or she uses the library regularly, irrespective of race or SES. "More than any other public institution, including the schools," Heyns concludes, "the public library contributed to the intellectual growth of children during the summer" (p. 177). Like schools, libraries do not outweigh the powerful effects of family background on cognitive inequality. Without libraries, however, the inequality would be even greater.

The author harbors no liberal illusions that schools by themselves can eradicate cognitive inequality, much less social and economic inequality. "Educational institutions are unlikely ever to redress the prevalent patterns of inequality in society as a whole. Yet it does not follow that these institutions are ineffective for poor children or that poor children would be well served in their absence. This is the fundamental point" (p. 183). Heyns does not address directly the radical critique of schooling which claims that dominant groups in capitalist societies prefer for schools to reproduce and legitimate inequality. Implicitly, however, her findings provide evidence that such dominant groups are less than completely successful. This study also does not deal with the broader humanistic aims and outcomes of education, how such outcomes might be conceived of and evaluated, or how the organization of schools and the structure of societies

may affect the realization of such goals. However, within its clearly defined limits, Heyns's study provides some eminently usable knowledge, both for public policy and for sociological research on education.

The central findings should prove useful for decision makers, particularly in regard to the distribution of public library resources (e.g., through mobile or branch libraries). More generally, for educational policy, this study firmly establishes that, however strong the effects of families (and to whatever extent this can be attributed to environmental or genetic causes), schooling does, indeed, make a difference in cognitive skills. For sociologists doing research in education, this book is a rich resource. Conceptual, theoretical, and methodological issues are discussed, and results are thoroughly documented. An appendix deals with problems in using standardized test scores, designed to measure relative position, as indicators of learning growth. Other appendixes present supplementary tables and correlation matrices, sampling design and data collection procedures, and the survey questionnaire. This volume is a model of thoughtful theorizing and careful empirical analysis.

Organization without Authority: Dilemmas of Social Control in Free Schools. By Ann Swidler. Cambridge, Mass.: Harvard University Press, 1979. Pp. x+199. $12.50.

Carl Milofsky
Yale University

Organization without Authority is an observational study of two free schools in Berkeley, California, which focuses on the question of how social control is maintained in schools which forgo the adult-centered, discipline-oriented, formalized control arrangements typical of American public education. To show how control is maintained, Ann Swidler offers detailed accounts of the schools and of relations between teachers and students. While this observational material is, on the whole, convincingly presented, it is not the heart of the book, which is an eclectic theoretical analysis of the changing role and structure of organizations in postindustrial society. The central thesis is that control in free schools comes not so much from organizational mechanisms based on administrative structures or on some technology of teaching. Rather, a changing social and cultural context makes the private, symbolic lives of students and teachers an important source of shared commitment, integration, and order in the free schools. Evidence supporting this contention is to be found in the social science literature as well as in direct observations.

The argument is supported in two ways. On one hand the author draws heavily on a growing literature of organizational analyses directed at a variety of alternative institutions, such as free clinics, communes, and worker cooperatives, which point to a widespread, upper-middle-class anti-

authoritarian movement. She also contrasts her two schools to show how the alternative organization model may be applied to free schools.

The contrast is between one school, which she calls Ethnic High, committed to raising the cultural awareness of a primarily minority student population, and a second, called Group High, which attracts a middle-class clientele responsive to the countercultural atmosphere in Berkeley. Ethnic High is relatively unsuccessful as a free school because it fails to generate among its students feelings of community centered on the school, owing to the students' weak academic skills and lack of commitment to the idea of education or to the institution of the school. Without making programs more structured, teachers find it hard to retain student interest and to convince them that supporting and participating in school activities is important.

This is less difficult at Group High, Swidler argues, because the movement toward alternative organizations is a reflection of currently emerging middle-class values of antiauthoritarianism and community control of institutions. These emergent values have led to the creation of a variety of alternative institutions. It is relatively easy to involve students in free school activities and to generate a feeling of community focused on learning and self-exploration if they believe that learning is important, that it should be personally motivated, and that schools should be egalitarian and based on feelings of solidarity and communal support for school activities. These were attitudes consistent with a countercultural ideology that was shared by Group High students and staff.

Swidler does an elegant job in showing how her organizational analysis builds on other studies of alternative institutions as well as on theoretical writings on postindustrial society. The theoretical portions of the book are comprehensive, well argued, and useful as source materials. I felt, however, that she did not give enough weight to the contribution of the youth culture of the late 1960s and early 1970s to the value shift she describes. Much of her organizational description documents the arduousness of working in free schools where formal authority has been dispensed with and replaced by the personal charisma of teachers. Having to be warm, energetic, constantly imaginative, and willing to work under the impoverished conditions which tend to prevail in free schools can quickly produce burnout.

Looking back from some distance, it seems that social experiments like those Swidler describes were period pieces. This is not to say that there is no contemporary interest in alternative institutions or that there is not a new, deep-rooted opposition to traditional institutions, to government bureaucracies, and to large corporations. But Swidler presents her free schools as though they were vanguards of a coming age in which new cultural values would sweep away old institutional forms. Looking at the free schools I know about, what seems more true is that, as do the members of the counterculture age, they become more and more willing to accept and support the comforts of existing arrangements in school systems. The old forms are powerful not just because they are instruments of the economy, the state, and elites but also because they are comfortable

for people who work within them. Perhaps their ability to coopt and seduce radicals is their most unsavory quality, but before one can suggest that the counterculture is writing the epitaph for old, bureaucratic, authoritarian styles of organization, it is important to think about how alternative organizations can be built which will be more comfortable to live with. Swidler's book would have been strengthened if it had offered more of a longitudinal view.

Power and Politics in Organizations: The Social Psychology of Conflict, Coalitions, and Bargaining. By Samuel B. Bacharach and Edward J. Lawler. San Francisco: Jossey-Bass, Inc., 1980. Pp. xiii+249. $15.95.

William A. Gamson
University of Michigan

There is a sound premise here. It is useful to look at organizations as involving interest groups that form coalitions and vie with each other for power. This is not a particularly new perspective. As the authors acknowledge, it is well represented in such classics as Cyert and March's *A Behavioral Theory of the Firm* (Englewood Cliffs, N.J.: Prentice-Hall, 1963) and James Thompson's *Organizations in Action* (New York: McGraw-Hill, 1967).

Samuel Bacharach and Edward Lawler's purpose in *Power and Politics in Organizations* is to tidy up this area, collecting the different strands of relevant literature and organizing them systematically. They recognize that much of the relevant literature remains at a general level of analysis, requiring some specification before one can apply it to the politics of organizations. Presumably, part of their purpose is to show us how work on coalition formation, bargaining, influence networks, and the like can aid us in understanding organizations.

Their strategy of achieving these objectives has one curious quality. No real organizations are ever mentioned at any time. Although the authors claim that their discussion is especially fruitful for understanding collective bargaining processes, you will search the index in vain for such bargainers as General Motors and the United Automobile Workers—or *any* company or union that has ever engaged in collective bargaining. I am not quite certain what they hoped to accomplish by this abstemiousness, but there is something heroic in writing a whole book about organizations in which none ever appear.

Their chosen strategy is to generate as many propositions or hypotheses as they can think of about power and politics in organizations. They succeed in breaking 100, topping off at 103. These propositions are, of course, clustered around particular processes or topics. What we end up with is a number of small piles, not a large pyramid of propositions.

A proposition can distill an important insight, but sometimes a summary

statement of a complex argument loses most of what calls for distillation. How well do Bacharach and Lawler's propositions do on such a criterion? An example suggests unevenly at best.

Their hypothesis number 37 (p. 116) suggests that "bargaining in the context of formal well-developed lines of communication will be more explicit than bargaining in the context of informal, undeveloped lines of communication." It is useful to recognize, as the authors do, that the rules governing tacit bargaining are not necessarily the same as those governing explicit bargaining. Thomas Schelling's rich essays on tacit bargaining (*The Strategy of Conflict* [New York: Oxford University Press, 1960]) have shown the importance of this distinction.

It is hard to see much of comparable interest distilled in hypothesis number 37 above. It is certainly reasonable to expect that when bargaining is conducted in well-developed channels it will be more explicit than when it is not. Indeed, established lines of communication seem almost a defining characteristic of explicit bargaining and the hypothesis approaches tautology.

The case for the 100-odd propositions rests on plausible theoretical arguments drawn from the existing literature on the topic. There are not many of them that one would choose to dispute, but the authors invest no effort in showing their empirical plausibility. They do not present new data that demonstrate the empirical fruitfulness of these hypotheses nor do they show how they can be used to reinterpret existing studies of organizations. Short of a systematic examination of new or existing data, they might persuade us with insightful case studies, but, in fact, they never illustrate their propositions with specific instances of organizational behavior. Indeed, the reader is even left to guess how the authors might measure the variables used, and all the thorny problems of clarity that one encounters in operationalizing concepts are avoided.

I share the authors' premise that it is useful to look at organizations as arenas in which conflict and bargaining processes are operating among competing groups. But I lack their faith in the value of propositional inventories and see their purposes better served by a strategy that places less emphasis on general, context-free propositions and more emphasis on the analysis of observable phenomena in real organizations.

The Investigators: Managing FBI and Narcotics Agents. By James Q. Wilson. New York: Basic Books, 1978. Pp. xi+248. $10.00.

Peter K. Manning
Michigan State University

The Investigators is a useful comparative study of the Federal Bureau of Investigation (FBI) and the Drug Enforcement Administration (DEA) that should be read in part because it analyzes well an important segment

of the American regulatory structure, in part because it is written by the current spokesman for the conventional wisdom about crime, and in part because it is misleading. James Q. Wilson, as we have learned to expect, neatly integrates empirical materials, a nicely drawn theoretical position, and his own well-mannered biases. The book is rich in insights, partial truths, and implications and is consistently stimulating. It rings true in my experience.

In this study of two Justice Department bureaus, Wilson has accomplished another coup, as he did in *Varieties of Police Behavior* (Cambridge, Mass.: Harvard University Press, 1968), by holding constant certain features of the organizations studied and then comparing variations in internal dimensions as they bear on environment-organizational transactions. It is based on a variety of kinds of materials, principal of which are official records and interviews with higher executives and managers in the two organizations. In spite of the fact that Wilson espouses and for the most part hews to a "view from the bottom up" of these bureaus, his data came from the top. Few people could have written this book. Wilson is an erstwhile advisor to the Justice Department.

Wilson's thesis is that governmental organizations deal with complex, vague, and controversial matters which are viewed as not profitable; they are characterized by "inadequate technology" or "vague goals" and are therefore forcefully shaped by the environment, or by features of the situation that are tangential to or independent of stated goals. The "critical tasks" of the agency, those carried out by the operators in the field, are not controlled by managerial strategies (and vice versa, according to Wilson). Because agencies of this type are shaped by the political environment, they are sensitive both to external pressures and to internal ones (predilections of the operators, formal and informal rewards, and constraints with which the operator must cope). They cannot develop autonomy, and therefore the executives seek to manage the political environment more than the internal one, while, on the other hand, the internal environment is sensitive to subunit autonomy wishes at each level (executives, managers, and operators). These matters coalesce with the difficulty of obtaining full and objective measures of performance. Rather than concentrating on performance, agencies such as these attempt to manage the appearance of effectiveness and to avoid being caught out for their errors.

The insights derived from this general perspective, combined with a sensitive portrait of the operating styles and dilemmas of both organizations, cohere to form an appealing veneer. The contrasts between the two agencies are neatly drawn: whereas the FBI is an *investigative* agency which looks into crimes that have been committed, is means oriented (attempts to evaluate the process of investigation and to reward procedural integrity), and has the benefit of public confidence and a monopolistic control over its domain, DEA (formed by Nixon in 1973 by combining the old Bureau of Narcotics and Dangerous Drugs with portions of Customs) is an *instigative* agency which attempts to make crime happen and

then intervene, is goals oriented (attempts to evaluate the extent to which cases meet programmatic objectives and goals and to reward gaining control over drug markets), and lacks a strong public mandate and monopoly within its domain. These structural features, when combined with the critical tasks agents must accomplish, produce the organizational character of the agencies, their defining contradictions and resolutions. In Wilson's view, conversely, in a world without political pressures (the external environment), the managerial strategy of the agencies would be based on the critical tasks of the operators (interviewing and informant work with the FBI; the inducement of crime with DEA).

In many ways, this is an intriguing and masterful book. Like all such books, it conceals and reveals artfully, elevating key assertions to factual status and demoting other facts to assertions. The critical reader might find objections to the hypothecated syllogisms repeatedly advanced, question the causal links, and entertain, even if it should be bad form, some unanswered empirical questions. Wilson asserts, for example, that business firms display an awareness of their critical tasks on pain of failure, but of course the managerial trickery that passes for accomplishment involves precisely the powerful and sometimes arbitrary redefinition of "critical tasks." A critical task is not a matter of platonic essence, but of symbolic construction and negotiation. It is said that the mission of the FBI under Hoover was "fully consistent with public beliefs, expectations and values" (p. 179). No evidence is cited for this claim, about which there could be at least some controversy. There are other claims about the nature of organizations, about the activities of the DEA, and analytic distinctions between the "external" and "internal" environment which could be questioned. The claim that tasks can be neatly isolated and described apart from the political environment, and that critical tasks are themselves unchanging, leads Wilson to discount the objective contours of the environment about which he writes. In Wilson's view, the FBI succeeds because it is means oriented, and evaluates performance using internal management criteria. It does not on the whole respond to the environment; it redefines it, enacts it, and posits the variability in the internal system as the source of management concern and the basis for determining agent "success." The DEA, ironically, according to Wilson, is too sensitive to the environment and fails by becoming too constraint oriented, not task oriented.

There is an alternative interpretation of the problems of these agencies. Begin—rather than with political pressures in the environment, vague goals, and technology yielding an irrelevant management separated from critical tasks—with constraints of the market, of the law, of the crime itself, and of resources, information, and skills. See these as being shaped and modified by cynical and expedient managers seeking power and control in their domain; and see these constraints as determining what defines "critical tasks." See also organizations as the locus of an internal struggle for control and definition of these tasks between segments of the organization loosely coupled internally and to the "environment." Thus, a contradiction results

between the power goals of appearance management and manipulation and the investigators' perspective mediated by praxis. The results may be the same, but the argument is quite different in its implications. Once the organization becomes the source of definitions *both* of the "environment" it enacts and makes real *and* of the "critical tasks," the dilemmas become those of mediating and transforming the objective and material features of the external world through symbolic codes and replicating that meaning system. If we were to turn Wilson's argument around, we would conclude that if there were no political pressures, the "critical tasks" would remain the determinants of the organization. Critical tasks are politically defined in the first instance, but even if they were not, the contradiction between doing and administrating would remain: that is, the schism residing in all organizations and the relative power of the agents in coopting the center of power and definition alter the internal structure of such agencies.

Union Rule in the Schools: Big-City Politics in Transformation. By William J. Grimshaw. Lexington, Mass.: D. C. Heath & Co., 1979. Pp. xv+157. $18.50.

Daniel B. Cornfield
Vanderbilt University

An unprecedented surge in public employee unionization occurred during the 1970s. While labor union membership in manufacturing declined, union membership of state and local government employees grew by 180%, over 20 times the rate of union membership growth in the American labor force. Yet little attention has been given to the effects of public employee unionization on the balance of power in city government and on the effectiveness of city government. William Grimshaw, who teaches political science at Illinois Institute of Technology, begins to fill this void in *Union Rule in the Schools*.

Union rule is a new model of urban government developed by Grimshaw. The main thesis of the study, that "union rule constitutes a major change in big-city government" (p. 3), is argued with a historical case study of the Chicago school system and the public elementary and secondary school teachers' union. The author claims that union rule differs in three ways from machine rule and reform rule—the two models of urban government that preceded public employee unionization.

First, union rule is characterized by "intrainstitutional disarticulation," in that the management of a city agency loses some of its influence over labor. Second, union rule differs from reform rule because unionized bureaucrats express a high self-interest in their behavior and objectives. Third, union-rule bureaucrats are more partisan politically than reform-rule bureaucrats and more inclined to regard unilateral managerial influence as illegitimate.

With the emergence of intrainstitutional disarticulation under union rule, maintains Grimshaw, "cities are becoming not only ungovernable but unmanageable and undemocratic as well" (p. 2). Managers are reduced to labor negotiators, and the union is accountable to its membership, not to a political constituency.

The study includes historical analyses of: (1) the changing beneficiaries under different school system regimes (chap. 2), (2) teachers' employment conditions (chap. 3), and (3) the decline of the Chicago machine and the rise of organized labor (chap. 4). It also includes an analysis of contemporary patterns of influence exerted by the union on the school administration (chap. 5).

The diverse analysis of the causes of intrainstitutional disarticulation between the teachers and the school management in Chicago is a major asset of the study, and it demonstrates the author's sophisticated and detailed historical understanding of Chicago school system politics. His sophistication is undoubtedly due, in part, to his experience in the Chicago schools, as a teacher and as an aide to Illinois Governor Dan Walker.

But Grimshaw does not explicitly define intrainstitutional disarticulation, a central concept in the book. (The reader must infer that it is a process by which a formerly dependent or subordinate group within the agency achieves authority and autonomy from a powerful group in the agency.) By not defining this central concept, the author assesses the formal and informal influence of the teachers' union on the school administration arbitrarily and thereby distorts the influence of the union in four ways.

First, he relies on records of school board voting on collective bargaining settlements to determine the degree of board sympathy with organized labor. The board always votes in favor of the contract. But is management "pro-labor," as Grimshaw suggests, if it accepts a contract with the union?

Second, he maintains that the most recent (at the time of the study) school superintendent appointed by the mayor believed that "the basic threat to the school system comes from the teachers, who have so leveled the school system through collective bargaining" (p. 129). Although Grimshaw cites this as evidence of the growth of union rule, it may also be taken as clear evidence of antilabor sentiment in the school administration.

Third, the study focuses mainly on collective bargaining issues before the board. But the union's sphere of control is limited or absent in such critical policy areas—which are neglected in the study—as facility planning and construction, hiring of teachers, hiring of all non-bargaining-unit personnel, and the racially and politically sensitive decisions regarding school attendance boundaries. Indeed, such decisions by the school administration recently jeopardized federal funding of the Chicago school system, because the federal government threatened to withhold funds until an acceptable school desegregation plan was established.

Fourth, the study fails to mention that on the vital, system-wide issue of finance, the Chicago teachers' union and the school board have united in concerted action. With the continuous loss of middle-class population

and manufacturing jobs in Chicago during the post–World War II era, and the subsequent erosion of the tax base, the Chicago school system has become increasingly difficult to finance. Recently, when the bond rating of the school system dropped, the teachers' union joined the school administration and the mayor in Springfield to lobby for more state aid. The teachers' union in New York City behaved similarly during that city's recent financial problems.

Union Rule in the Schools is an attempt to create a new model of urban government in response to a recent empirical trend: public employee unionization. Although the model—including its name, "union rule"—greatly overstates the magnitude of public employee union power, the study raises provocative questions for a budding field of research.

The Breaking of the Image: A Sociology of Christian Theory and Practice. By David Martin. New York: St. Martin's Press, 1980. Pp. x+211. $22.50.

Martin E. Marty
University of Chicago

Sociologists need not confine their expertise to formal sociology. One of England's two foremost sociologists of religion, Bryan Wilson, is "something of a connoisseur of claret . . . [and] acknowledges an 'untutored enthusiasm' for Chinese ceramics, eighteenth century music, Lieder and English watercolours" (from author's identification before the title page in Bryan Wilson, *Religion in Secular Society: A Sociological Comment* [Baltimore: Penguin, 1966], n.p.). His peer, David Martin, is not only an expert who in 1978 could publish *A General Theory of Secularization* but also someone who now shows himself to be something of a connoisseur of cadences. He brings a reasonably tutored enthusiasm to high-toned church music and high-church liturgies.

Like Jacques Ellul, who intersperses works of social thought with theological polemics, or Peter Berger, who slips in works of theology between books on social theory, Martin is capable of venting aesthetic and theological opinion under the category of sociology, as he does in this new book. At worst, and it does have some worst moments, this work's effect matches Martin's definition of "so much preaching . . . pious opinion glossed by sociology" (p. 103). At best, Martin displays connoisseurship in aesthetics. In the middle range he does draw on sociological sources. Weber receives passing mention, but Durkheim and Parsons and the other sociologists who have dealt with these themes are absent. Martin has been carrying on conversation more with the informing likes of anthropologists Victor Turner and Mary Douglas.

From one point of view, *The Breaking of the Image* can be read as a quasi-sociological polemic in support of the better, older ways in the Church

of England. Martin is the leader of a group of British intellectuals and litératteurs who are resisting revisions of the Book of Common Prayer. Such a cause is, of course, his prerogative. I have empathy for him in his love of the archaic as a bearer of the sacred and his distaste for compulsive "mod-ness" in forms of worship. It is when he moves from expression of opinion or preference to attempts at grounding the outlook in reliable social theory that one grows uneasy. Whereas his American fellow-conservative Peter Berger looks for signals of transcendence in the gestures and speech of common folk, Martin comes on as a culture-bound snob.

Rejecting worship "in the round," where the mystery is in the center of the gathered people, he comes out for the pew-behind-pew arrangement with the clerics' back to the congregation as the better way for imaging the sacral. Better for whom, and where and when and why? Early Christians in their domiciled worship had enough sense of the sacred in casual face-to-face relations to sustain them while they kept the church alive. Or were there pews in their living rooms? Many "primitive" cultures did better than the English at keeping the sacred vivid by gathering around a holy hill.

Martin shuns liturgical language that sets out to communicate clearly, implying that more people would worship and all would worship more profoundly were there more of the archaic and arcane. Adults should cherish "bounden" and "heavenly benediction" just as once they favored "yum yum" and "fe fi fo fum." Well and good, but all this implies a judgment that the millions of Southern Baptists who use *The Living Bible* and straight speech are missing out on the *mysterium tremendum et fascinosum*. How does Martin know? Certainly the Baptists are at least engrossed in worship, while the English have had three centuries of the Prayer Book but have deserted the sanctuaries in droves and have left bare ruined choirs. Its polemics and snobbery aside, there is much in this book that will resonate in the mind and heart of anthropologists, high-church worshipers. and lovers of mystery. Martin, like Wilson and Berger, cannot fail to be lively, and many of his polarities ("Holy Virgin and Holy City," "Embodiment and Plenitude," "Human Sound and Sublime Vision," "Secular Communication and Holy Communion") show that he has a fine intuitive sense about where the mysterious and the sacred break into life. He also ventures with the beginning of theories to account for them, to the degree that human speech can do this accounting at all. His clues await development in a formal, less polemical sociological work. I, for one, will be an eager reader thereof.

American Journal of Sociology

Islam and Capitalism. By Maxime Rodinson. Translated by Brian Pearce. Austin: University of Texas Press, 1978. Pp. xxvi+308. $6.95 (paper).

Leonard Binder
University of Chicago

The central question for understanding the events known as the Islamic revival is whether this revival is the result of a spiritual reinvigoration of religious faith and commitment or the consequence of admittedly significant material change. Some 15 years ago, when Maxime Rodinson wrote *Islam and Capitalism* (originally published as *Islam et le capitalisme* [Paris: Editions du Seuil, 1966]), the issue was to explain the backwardness of the Islamic world; but the explanations were of a similar nature. Presumably, the intervening transformative events, occurring in the full view of historically conscious observers, should have allowed these questions to be decisively set at rest and thus disposed of one of the most persistent intellectual conundrums of all time. Rodinson claimed that the truth of the Marxist materialist conception of ideology had been more than amply demonstrated by the history of his times, even if that truth were a little distorted by the excesses of his dogmatic Marxist friends. Despite his confidence in this matter, there are still many moralizing and intellectualizing idealists around and about who are inclined to read the most recent history of Islam differently, and there are also not a few phenomenological monists who insist on the absolute unity of matter and spirit. Rodinson, himself, is outspokenly committed to the values of political freedom and material equality; furthermore, he argues that Muslims and others are in a position (are free?) to choose between capitalism and socialism at this historical juncture.

Actually, Rodinson's explicitly theoretical, philosophical, and methodological statements are somewhat confused, rambling, and incoherent. But then the value of this book lies neither in the fragments of polemic against dogmatic Marxism and panstructuralism which it contains nor in its oversimplified populist materialism. Rodinson is an orientalist, and throughout most of this work he remains faithful to that calling despite his apparent belief that the enlightenment evidenced by his abandonment of orthodox Marxism was an entitlement to a social scientific pretension. The true value of the book is to be found in its relevance to two more limited questions, one arising in the context of Marxist theory and the other in the context of Weberian theory, and both related to the general issue of development or modernization.

The predominant theories of development, liberal and Marxist alike, hold the Hegelian position that modernization, by whatever definition, is a singular phenomenon of world historical significance which has already occurred. Though modernization, whether as a cultural or an economic phenomenon or both, first emerged in Europe, the significance of that

emergence is worldwide. Hence, as far as all non-European societies and cultures are concerned, modernization or development must in some sense be westernization and therefore alienative. For Third World countries, the possibility of an authentic development (i.e., one that truly reflects the existence of a given historical sociocultural entity) depends on the theoretical possibility of two or more entirely independent processes of development and, at our present stage of world history, on the practical possibility of reconstructing what an alternative and originally authentic process of development might look like. The complete statement of the issue for present purposes requires the further question whether there might have been an authentic Islamic alternative of development which was preempted by European imperialism and capitalism and which might now be recovered by a revolutionary return to Islamic authenticity—as some might argue is occurring in Iran under the leadership of the Ayatollah Khomeini.

Rodinson does not approach this general issue in the way in which I have posed it. Instead he deals with the two more limited questions to which I have alluded. He rejects the validity and applicability of the Marxist thesis of the nondevelopmental consequences of the Asiatic mode of production. He also rejects the argument attributed to Weber that the Islamic ethic is too antipathetic to rationality to have permitted the indigenous development of capitalism. In this argument, capitalism quite justifiably functions as might the concept of modernization or development.

In arguing against the dogmatic Marxists, Rodinson is at pains to point out that he distinguishes between the prevalence of certain capitalistic practices which he takes as evidence of the potential historical evolution of a capitalist mode of production or a capitalist socioeconomic order, and the actual existence of a socioeconomic order whose mode of reproduction is capitalist. The existence of capitalistic enclaves or economic subsectors in medieval Islam is enough to refute the Asiatic mode thesis. Given the proper political, social, and economic circumstances, Rodinson is certain that a full-fledged capitalism would have emerged.

The refutation of Weber is accomplished in two stages. First Rodinson asserts that Islam as presented in the Koran is more rational than either Judaism or Christianity of the Old and New Testaments. Then he goes on to explain that insofar as later Muslim practices, especially those related to fatalism and to the application of qadi law, were inimical to economic rationality they are to be attributed to historical circumstances and not to any essential of Islam.

In his insistence upon the detachment of Islamic essences from historical circumstances and in his refusal to validate any conclusions regarding Islam which have been drawn from historical events, Rodinson appears, paradoxically, to agree with a number of idealistic Muslim apologists. In point of fact, he criticizes both would-be orientalists and Muslim apologists. He does so by demonstrating that even the Islamic prohibition of usury and of certain kinds of transactions was no barrier to the continuation and

elaboration of pre-Islamic capitalistic practices and by similarly demonstrating that the Koranic revelation did not bring into being an exemplary moral economic system which was fundamentally different from any other existing economic system.

Indeed, this is the theme to which Rodinson returns at the end of his essay. He flatly states that there is no third way. There is only a choice between capitalism and socialism. Islam, like any other ideology, is essentially epiphenomenal even if in the short run it might have some political significance. Moreover, at the moment the symbols of Islam are fairly firmly under the control of the conservatives. Hence, if Muslims desire to achieve greater political freedom and greater material equality in accordance with what most people now believe to be possible, they must turn from traditional Islam and reject the chimerical notion that there is an authentic, original, Islamic third way to development. Of course, this is not to argue that Islam is a barrier to socialism any more than that it is a barrier to capitalism or even that it is essentially socialistic, capitalistic, fatalistic, or militaristic. As mere ideology, Islam may under appropriate circumstances serve any political design, whether that of Khomeini, or Saddam Hussein, or President Sadat, or King Khalid, or Mu'ammar al-Qaddafi, or General Zia al-Haq, or King Hassan II, or . . .

Even though this book was first published in 1966 it is manifestly not dated except in the sense that it does not refer to some obviously relevant recent examples of efforts to achieve development without either adopting socialism or allowing an unbridled capitalism to overwhelm traditional social and political structures. While falling considerably short of being a major intellectual achievement, the book does have a seminal quality. Although we cannot be certain of its direct influence, there exist a small number of related studies which have further amplified the issues here introduced. The most closely related are the two essays by Bryan Turner, *Weber and Islam: A Critical Study* (London: Routledge & Kegan Paul, 1974) and *Marx and the End of Orientalism* (Winchester, Mass.: Allen & Unwin, 1979). Edward Said, the author of the celebrated *Orientalism* (New York: Pantheon, 1978), has expressed his admiration of Rodinson's work, and he, like Rodinson, has defended Islam against its critics without at the same time telling us anything substantive about Islam except for its identitive function. Of greater relevance to Marxist polemics is the little book by Samir Amin called *The Arab Nation: Nationalism and Class Struggle* (Belfast, Me.: Porter, 1979) in which he attempts to apply his "law of unequal development" to Arab history. Amin is well known for his earlier denial of the validity of the argument that development is possible within the framework of dependency. Those who hold the contrary view are, according to him, constrained to "accept the Trotskyist theses according to which socialism can only come from the developed capitalist world" (p. 111). But he rejected those theses, asserting instead that "there are thus several evolutionary paths, as opposed to a single road mapped out in advance for the whole of human history. . . . If capitalism had not

appeared in Europe, humanity would doubtlessly have discovered it anyhow" (p. 91). Suggestions of this sort, based at least on the same problems which presented themselves to Rodinson on his emergence from orthodox Marxism, may give rise to a new trend in the study of Islamic history—a trend which may be inspired by the search for an emergent indigenous capitalism which was crushed by European imperialism. Peter Gran's *Islamic Roots of Capitalism: Egypt 1760–1840* (Austin: University of Texas Press, 1979) is oriented to this theme and we may expect it to be followed by other works with a similar *parti pris*. It may be noted that, like Rodinson's, these works are critical of traditional European-oriented Marxism, which is said to be tainted with orientalism. Gran, for example, as a good academic trade unionist argues that the Islamic world does have a history of its own which is meaningful independent of European history and worthy of study. Amin has larger aspirations. He believes that "all the conditions are gathered for the appearance of an invincible revolutionary upsurge in the Arab world" (p. 114). This upsurge will accompany a profound restructuring of the international division of labor, producing a new framework within which "one can already see the broad outlines of the outcome appearing in the form of an Arab renaissance, an affirmation of Arab power" (p. 105). The Arab nation, according to Amin, may form an alliance with the capitalist West or with the statist East, or it may choose the path of genuine social revolution. In this choice, however, Egypt will play the key role, as it has always done.

In this manner the views of the Marxist orientalist, whose very arguments seemed to question the validity of studying the orient, have been refuted by a complex reasoning which restores the Islamic world to the center of history as the most likely locus for the occurrence of a truly socialist revolution which will, finally, achieve that liberation and equality for which man has so long striven during that career which we call history.

In Amin's book we find at best one possible response to Rodinson's apparent defense of Islam against both Marxian and Weberian-inspired orientalism—a defense which, however, attributes no authentic influential role to Islam in the shaping of the future socioeconomic order of the Muslim world.

Peter Gran, though, is openly critical of what he takes to be the nationalist excesses of Amin's thesis, and emphasizes instead the *Islamic* roots of capitalism; that is to say, he would go beyond Rodinson's argument that Islam was neither ideologically nor in practice opposed to capitalism and show in what particulars there was and may yet again be a distinctively Islamic form of this classic mode of production. To the extent that this demonstration is successful, it may be argued that the view that there can be a distinctively Islamic force of modernization has been sustained by means of a demystified or nonromantic discourse similar to the one employed by Rodinson himself.

Ethnic Plurality in India. By R. A. Schermerhorn. Tucson: University of Arizona Press, 1978. Pp. xii+369. $12.95 (cloth); $6.45 (paper).

A. R. Saiyed
Jamia Millia Islamia, New Delhi

By the time the decade of the seventies had drawn to a close, it had become apparent that modernization was yet another god that had failed—especially in the so-called developing societies. Instead of the unity and homogenization expected of modernization, today all these societies are experiencing ethnically inspired dissonance and conflict. India, with its extreme cultural diversity, is no exception. Notwithstanding its efforts to forge a common sociopolitical identity, religious rioting continues with amazing savagery. The northeastern tribal regions are in a ferment bordering on revolt. And caste antagonisms, in rural areas, are being manifested in the form of peasant uprisings. Thus, as the expected universalistic outlook associated with modernity becomes a fading dream, the salience of ethnic differentiation and antagonisms is increasingly becoming a persistent nightmare. Accordingly, *Ethnic Plurality in India* is a most timely volume. Its value is further enhanced by the fact that R. A. Schermerhorn has attempted a macro-level analysis instead of engaging in a micro study of limited value and application.

The book not only describes the sociopolitical situation of India's prominent ethnic minorities but also analyzes in each case their problems of adjustment and integration. Schermerhorn has accomplished both tasks with commendable insight and scholarship. Hence (even without always agreeing with the author) it may be asserted that this pioneering effort will remain a lasting contribution to the currently underdeveloped, and uncared for, field of ethnic relations in Indian sociology.

To those familiar with India's sociocultural complexities and pluralities, any macro-level study would seem to be an impossible task. However, with the help of imaginative paradigms and diligent data collection, Schermerhorn has surmounted most difficulties. Compromises, of course, there are, and selectivity too is present. But in an exploratory study of the magnitude of this one, such lapses are not unknown; indeed, they are inescapable.

Schermerhorn's theoretical scaffolding will be familiar to those acquainted with his earlier work in this field. According to him, ethnic groups take their rise from recurrent historical patterns that link the groups to the broader society. He calls these patterns "intergroup sequences." In the Indian context the following patterns (and the minorities pertaining to each) are identified: (1) emergence of pariahs: scheduled (depressed) castes; (2) emergence of indigenous isolates: scheduled tribes; (3) migrations: Jews, Parsis, and Chinese; (4) colonization and conquest: Muslims, Christians, and Anglo-Indians; and finally (5) religious cleavage: Jains and Sikhs. After presenting this identification the author attempts

to examine the extent to which these 10 ethnic minorities are integrated into the national mainstream. This examination is one of the main purposes of the book.

Rightly emphasizing that integration is a two-way relationship involving agreement and disagreement over collective goals, on the part of both minorities and majority, Schermerhorn points out that centripetal or centrifugal tendencies, on the part of both groups, help to illuminate the exact nature of these relationships. Combining these two tendencies for both dominant and subordinate ethnic groups, the author has developed a four-celled paradigm. Cell *A* represents assimilation/incorporation; cell *B*, cultural pluralism/sanctioned autonomy; cell *C*, forced segregation with resistance; and cell *D*, forced assimilation with resistance. In terms of this fourfold typology, Schermerhorn assesses the chances of integration with the dominant group for each of the 10 ethnic minorities.

Constraints of space will not allow a detailed discussion of his analysis. Suffice it to say here that Schermerhorn gives the least chance for integration to the three ethnic minorities that are salient in terms of size and enclosure, namely, the scheduled castes, scheduled tribes, and Muslims. Differing reasons are adduced in each case. The author's treatment of the problems and dilemmas of integration faced by the first two groups is cogent and well balanced. But in the case of Muslims he appears to have erred in overemphasizing the role of historical antagonisms. Perhaps the kind of informants on whom he relied have been responsible for this skewness.

Special mention must be made of the fact that the author has amassed a wealth of information on each ethnic group, and has very competently intermeshed a vast array of historical, demographic, political, and socioeconomic data in his description and analysis of the condition and problems of each group. Of special relevance in these discussions is the application of a paradigm of the modes of mobility. This paradigm has been developed by combining the variables of culture on the one hand, and power and control on the other, and relating them to collectivistic and individualistic channels of mobility. This strategy draws our attention to the fact that while minority group members everywhere are confronted with the dilemma of choosing between individual ambitions and collectivistic loyalty, cultural and political factors impinge on the alternatives selected, making integration a complex and difficult process.

Ethnic Plurality in India is a book which no student of ethnic relations or Indian society should miss. Apart from its other merits, Schermerhorn writes in a style, and with insights, that infuse his book with an artistic quality as well.

American Journal of Sociology

We Eat the Mines and the Mines Eat Us: Dependency and Exploitation in Bolivian Tin Mines. By June Nash. New York: Columbia University Press, 1979. Pp. xxi+363. $22.50.

Jean R. Barstow
University of Minnesota

June Nash's thought-provoking monograph is important reading for anyone seeking to understand the cultural and ethnic influences which shape political consciousness in the Third World. *We Eat the Mines and the Mines Eat Us* is a valuable study of Bolivia's Aymara and Quechua "Indian proletariat" employed in the mining enterprises nationalized in 1952 by the Nationalist Revolutionary Movement. Although mine workers constitute only 2% of Bolivia's work force (and 28,000 of the country's roughly 5.5 million inhabitants), they include the working class's most outspoken left-wing elements, inheritors of nearly a century of union activity and staunch resistance to antilabor governments. In Nash's study, this militant history is painstakingly chronicled within a more encompassing analysis of the international economic and political pressures which have made Bolivia a vivid example of the dependency which typifies many Third World nations. For despite its wealth in natural resources, Bolivia has the lowest indices of per capita income, literacy, and life span of any country in South America.

To the rapidly growing anthropological literature on class, this book adds a fresh and highly original perspective, drawn from Latin American dependency theory, Marxian analysis of ideology and class consciousness, and the interpretation of symbolism and ritual processes. A basic premise, linking Nash's chapters on such diverse topics as kinship, community structure, and labor relations, asserts that the *cholo* culture of the mines, a transformation of belief and behavior typical of the *ayllu* or indigenous rural community, supplies workers with a shared cosmology and a sense of solidarity. Profoundly rooted Andean symbolism and ritual, kinship-based systems of mutual assistance, and the annual ceremonial cycle all come under Nash's scrutiny as wellsprings of resistance by miners to their exploitation—the low returns for their labor and their dehumanizing working conditions. The major accomplishment of this study, however, lies in Nash's well-sustained demonstration that *cholo* culture also works to vitiate the unity of mine workers, in their families, on the job, and in political action. This is because native symbolic themes and ceremonial frequently convey and reinforce the deep feelings of dependency which workers harbor toward the mine as their sole source of material survival. Most of the book is devoted to an investigation of the contradictions arising from the miners' awareness of their exploitation, which inspires collective protest, and their dependency feelings, which encourage an acceptance of existing conditions.

In a short preface, Nash supplies a background for the involvement of miners in national politics since 1971, when she completed 16 months of

fieldwork in the mining centers of Oruro. After documenting the Banzer government's repression of political opposition—particularly of miners and peasants—between 1971 and 1977, her narrative details the pivotal role played by miners, factory workers, human rights activists, and the clergy in bringing about the short-lived "opening toward democracy" which began in January 1978. The introduction (chap. 1) then develops the argument that Bolivia serves as an ideal case study for the overall impact of industrialization on Latin American nations. In chapter 2, the analysis of worker consciousness begins with accounts of the history of the mines, as narrated by laborers themselves, in myths as well as moving personal recollections of the strikes and massacres which preceded nationalization. As an anthropological perspective on class consciousness, this chapter's greatest strength lies in a thoughtful weighing of the impact of generation, ethnicity, and gender in fomenting or inhibiting the emergence of a classwide unity among miners. Chapter 3, "Belief and Behavior in Family Life," presents an analysis of kinship, including the ties of *compadrazgo* (ritual co-parenthood), as an adaptation of rural institutions to the industrialized mining context. Nash reveals systematically how family structures, like other dimensions of the daily lives of workers, encourage or thwart protest. In this section, the goal of clarifying how qualitatively different forms and levels of dependency are articulated receives special attention. In family life, Nash argues, privileges based on age, "whiteness," and gender are inculcated and contribute eventually to reinforcing "authoritarian" economic and political structures outside the domestic sphere. Continuing the analysis of strains toward acquiescence and discontent in the mine camps, chapter 4 examines the attitudes of workers toward housing, health care, and other services supplied by the administration. With the exception of the educational system, such benefits are often seen by laborers as inefficient and self-serving bureaucratic structures. On the other hand, traditional Andean patterns of reciprocity enduring in the mines, such as *aini* (the reciprocal exchange of money, goods, or services) and *pasanaku* (a collective saving and distribution pool), encourage cooperation among workers. "The Natural and the Supernatural Order" (chap. 5) delves into the miners' highly syncretistic "world view," elucidating how pre-Conquest cosmology, Catholic doctrine, and "Western" political ideologies have been integrated without dissonance into the same belief system. In chapter 6, "Conditions of Work in the Mine," Nash offers a creative contribution to the ethnography of work, reporting on the day-to-day tasks performed by workers in the mine shafts and delineating the cultural, ethnic, and occupational differences which set laborers apart from technicians and supervisors. Chapters 7 and 8 situate the history of the miners' struggles for higher wages and for worker control against the background of Bolivia's economic and political dependency, substantiating in full Nash's earlier claim that the Bolivian nation is unable to resolve its internal problems "because of the controls operating on it from beyond its borders" (p. 16).

After a valuable discussion of the process of "cholification," Nash's conclusion turns to a theoretical synthesis concerning dependency and ex-

ploitation as distinct forms of "alienation." At this juncture, a final summary of the book's innovative contributions to distinguishing and interrelating forms and levels of dependency would have greatly strengthened the study. And given the emergence of political coalitions of miners, peasants, and urban dwellers since the 1950s, the analysis might well have directed more attention to the types of linkages between miners and nearby rural populations, particularly in those regions where peasants have successfully allied themselves with miners and contrasting zones where miners are less known for political activism.

Moving a Mountain: Cultural Change in China. Edited by Godwin C. Chu and Francis L. K. Hsu. Honolulu: University Press of Hawaii, 1979. Pp. ix+446. $17.50.

Chong-chor Lau
Chinese University of Hong Kong

Moving a Mountain is the product of the conference on "Communication and Cultural Change in China" held in 1978 at the East-West Center. Though grouped into five parts, the 17 papers collected and edited by Godwin C. Chu and Francis L. K. Hsu are variations on the same theme: the role communication has played in processes of change in Chinese society under the Communists. Most of the observations are based on empirical studies, and since China has been isolated from the world community during a large part of the past three decades, they contribute to bridging a knowledge gap which still separates that country from outsiders.

The editors, separately and together, contribute three articles. They attempt to construct an analytical framework for the phenomena being studied so that the collective work will become more coherent. The framework looks to an individual's relationships with three cultural elements: significant others, materials and objects, and ideas. The unique character of these relations is the core of culture. The individual also has perceptions about relations among the three cultural elements. Such perceptions about one's social and physical worlds vary in their impact on cultural patterns. Cultural change, then, means the alteration of these actual and perceived relations. Communication becomes a key factor in understanding cultural change, for any process of change involves exchange of information and feelings.

From this perspective, the main proposition advanced in the chapters written by the editors is that Chinese social relations, especially those between an individual and his significant others, are dominated by "affect," a concept they use to contrast with "role" or instrumentality. Affect relations are personal in nature and are generated and maintained by par-

ticularistic group ties. The authors argue that affect-dominated cultures respond less readily to technological change than role-dominated ones. In the former, changes in ideology are necessary if the patterns of social relations are to be altered. Communication, then, becomes an important mechanism for social change in a society like the Chinese.

Five papers are devoted to analyzing China's communication system. Together, they present a picture of the system with regard to its historical development, its structure and functions, and its relationship to change in local communities. The main impression conveyed by these chapters is that the current communication network in China is an effective one with little advanced technology. Sociologists will be particularly interested in the papers by Martin King Whyte and Lau Siu-kai, which discuss the use of small groups and formal organizations in the communication process.

Four chapters discuss the relationships between China's political culture and the communication media. Lucian W. Pye addresses himself to an interesting phenomenon he calls "esoteric communication," that is, the use of ideological code words and historical allegories in the Chinese press. This form of communication requires the readers to be more intelligent and resourceful than their Western counterparts. Reading newspapers in China, especially during the Cultural Revolution, became an intellectual exercise. The reasons, according to Pye, are both political and cultural. In Chinese politics, personnel questions are more important than policy issues. And culturally, the Chinese are "more restricted in expressing disagreeable or aggressive views in face-to-face encounters, less restrained when speaking to an audience, and willing to be highly emotional in print" (p. 157). Thus personnel changes are announced through secret channels of communication, and political consensus is achieved through face-to-face exchange of information and views. Until these procedures are completed, the mass media remain a forum for attacking nameless targets; hence the great guessing game. Merle Goldman's analysis of two political campaigns in the mid-1970s illustrates the purpose and mechanisms of such a communication process. The other two chapters add enlightening information about the role of communication in changing China's political culture.

Five other papers measure value change in contemporary China. Their authors use a variety of methods of data collection: from personal observation and content analysis to refugee interviews. They cover such topics as value themes in short stories, attitudinal change among the Chinese peasantry, the effect of communication on value change among the urban youths who were sent to settle in the countryside, and the role of communication media in rural social change. A concluding chapter by A. Doak Barnett summarizes the findings of most papers collected here, and points to some future problems of research. The central finding seems to be that communication can effect more changes in social structure than in culture. However, following the editors' framework, cultural change is bound to take place if social relations are altered significantly. Therefore, the mountain will eventually be moved, though it takes time.

The Political Economy of Distribution: Equality versus Inequality. By Michael Don Ward. New York: Elsevier–North Holland Publishing Co., 1978. Pp. 188. $16.95.

Kenneth A. Bollen
General Motors Research Laboratories

The Political Economy of Distribution is a cross-national empirical analysis of the *intra-* and *inter*national determinants of inequality. As such it is relevant to the research of many comparative sociologists. Before commenting on the major strengths and weaknesses of the work, I will briefly summarize its contents.

The overview of the work in chapter 1 is followed by a chapter devoted to a theoretical and operational definition of inequality. Michael Don Ward expands the usual theoretical definition to include other social goods in addition to income. His measure of inequality is also broader than that used in most previous research. It includes (1) a Gini coefficient of individual income inequality; (2) a "social immobility" index; (3) an index based on the infant mortality rate, physicians per capita, and caloric and protein consumption per capita per diem; (4) a ratio of affluence to poverty, with affluence measured by the number of universities and colleges, Hilton hotels, and motor vehicle deaths, and poverty indexed by a set of nutritional variables; and (5) a second affluence/poverty ratio measured by telephones per 100 total population divided by telephones per 100 population in the capital city.

Chapter 3 examines the effects of the level and rate of economic development on inequality. The level of development's effect on inequality is evaluated by comparing three functional forms: linear, quadratic, and a logarithmic transformation of the development measure. Like Robert Jackman (*Politics and Social Equality* [New York: Wiley, 1975]), Ward chooses the logarithmic transformation of development as the most appropriate form. He also presents evidence that the rate of economic development, when considered alone, increases inequality but that it reduces inequality when examined in interaction with the diversity of the economy.

In chapter 4, Ward introduces a set of intervening variables which he argues mediate the effects of economic development on inequality. Educational policy effort and general welfare policy effort are estimated to be important domestic factors that reduce inequality. A test to determine whether political mobilization affects these two domestic factors reveals no significant effects.

The international determinants of inequality are the subject of chapter 5. Ward presents arguments from dependency theory to suggest that the position of a nation within the international hierarchy has both direct and indirect effects on inequality. He also interprets his results as showing that the international dependency factors have greater effects than the domestic

ones discussed in chapter 4. Finally, chapter 6 summarizes the major findings and draws out some of the implications of the research.

One strong point of Ward's work is his attempt to derive empirically testable hypotheses from theories of dependency. Given the highly abstract and sometimes ambiguous nature of dependency theories, this is not an easy task. Although readers may question his interpretation of these theories, his attempt should be commended.

Another desirable aspect of Ward's research is his efforts to integrate domestic and international determinants of inequality. Although his model is far from complete, his comparison between these determinants is relevant to recent research in sociology. Indeed the relative effect of the domestic and dependency factors on inequality is still a subject of controversy (see, e.g., Steven Stack, "Internal Political Organization and the World Economy of Income Inequality," *American Sociological Review,* vol. 43 [1978]).

In my opinion the major strength of Ward's book is his extension of the concept and measurement of inequality beyond income statistics. Students of inequality and international development have nearly always examined income distributions to the exclusion of the distribution of other social goods such as health, nutrition, and shelter. By calling attention to other aspects of inequality, Ward performs a valuable service.

On the other hand, his work exhibits a number of weaknesses. One recurrent problem is the questionable validity of the variables he uses to measure his concepts. For instance, the inequality measure contains several indicators lacking even face validity. Specifically, the use of the number of universities and colleges, Hilton hotels, and motor vehicle deaths as measures of affluence presents some problems. Motor vehicle deaths, for example, measure economic development and vehicle miles traveled more than they measure affluence. Indeed the wider the distribution of motor vehicle ownership and the more miles driven, the greater the number of deaths that would be expected. This variable may be more closely related to equality of distribution than to affluence.

An additional problem of the inequality measure is that some of its components are also used later as independent variables. In chapter 4, for instance, education is measured by the number of universities and colleges. The reader cannot help wondering whether the use of this measure in both the dependent and independent variables affects the analysis. This is even less understandable given the variety of educational measures that could be used alone or in some form of index.

A second recurring problem is Ward's inadequate tests of the hypotheses he examines. One important example concerns the relationship between economic development and inequality. Although he reviews some of the well-known work of World Bank researchers (pp. 59–61), he fails to use or test the functional form of the development/inequality relationship that they derived. That is, he does not test the "logarithmic term plus logarithmic squared-term" form of the model which is recommended by the World

Bank sources that he quotes. As Erich Weede ("Beyond Misspecification in Sociological Analyses of Income Inequality," *American Sociological Review,* vol. 45 [1980]) has recently pointed out, such a misspecified development/inequality relationship may radically influence the results of an analysis.

Another example of incomplete testing occurs in chapter 4. Here Ward argues that the effects of economic development are mediated by educational and welfare policies. Yet he does not test whether development's *direct* effects on inequality drop to zero when these variables are controlled. In fact some of his results suggest that these variables do not mediate all of development's effects: on page 64 he reports an adjusted $\bar{R}^2$ of .57 when inequality is regressed on development alone, whereas on page 108 inequality regressed on the indices of educational policy, welfare policy, and short-term economic growth, but not on the level of development, results in an adjusted $\bar{R}^2$ of .46. In other words, the equation with economic development alone has an $\bar{R}^2$ nearly 25% greater than the equation with the variables that are claimed to mediate development's impact on inequality!

Another set of problems stems from Ward's misunderstanding of some of the statistical techniques he uses. One example appears on page 70, where he compares the multiple correlation coefficients and standardized regression coefficients of his analysis to those of Jackman's work. The problems of comparing standardized coefficients rather than unstandardized ones across different time points and populations are well known. And the fact that Jackman uses a measure of inequality completely different from Ward's makes any comparison more questionable.

Another example of faulty methodological analysis occurs on pages 156–57, where the variance-covariance matrix of the *regression coefficients* is examined as a means to detect heteroscedasticity and autocorrelation. "Examination of this $[\sigma_\mu^2 (X'X)^{-1}]$ matrix lends information about several of the OLS assumptions. Specifically, the variances of the estimated coefficients . . . should be roughly equal. . . . Second, the OLS model assumes that the nondiagonal element on the variance-covariance matrix will be roughly equal to zero. This is the assumption of uncorrelated regression disturbances, i.e., nonautoregression" (pp. 156–57). Such misunderstandings of regression techniques leave me uncomfortable about the empirical analysis in the book.

In short, although Ward has chosen an important topic of analysis and has expanded the usual conception of inequality, flaws in his analysis and measures raise questions about the validity of his findings.

Political Action: Mass Participation in Five Western Democracies. By Samuel H. Barnes, Max Kaase, and Klaus R. Allerback, Barbara Farah, Felix Heunks, Ronald Inglehart, M. Kent Jennings, Hans D. Klingemann, Allan Marsh, and Leopold Rosenmayr. Beverly Hills, Calif.: Sage Publications, 1979. Pp. 607. $25.00.

G. Bingham Powell, Jr.
University of Rochester

This work is the first major publication of results of a large cross-national study of citizens' attitudes toward unconventional political action. The project was stimulated by the waves of protest experienced by many Western industrialized nations in the late 1960s. Between 1,800 and 3,200 citizens aged 16 or older were interviewed in 1974 in each of the following: the United States, Britain, Austria, West Germany, and the Netherlands.

Political Action, by Samuel H. Barnes, Max Kaase, and Klaus R. Allerback, et al., is divided into four major sections. The first develops description and initial analysis of the different forms of citizens' political action, particularly comparing conventional and unconventional activities. The authors demonstrate in many ways the degree to which awareness of unconventional participation tactics has become part of the political consciousness of the population in these countries.

The second major section, entitled "Ideology and Values," first examines the levels of ideological conceptualization that emerge from analysis of citizens' evaluations of political parties in the five nations. The analyses build directly on the work on ideological sophistication originally presented by Angus Campbell et al. in *The American Voter* (New York: Wiley, 1960). Ideological conceptualization levels and value priorities of citizens are then related to their views of political action.

The third section analyzes personal and political dissatisfactions, their origins and their relationship to various forms of political action. "Conventional participation was largely a function of system responsiveness, or support for the rules of the game, while unconventional activity was attributable to unhappiness with the existing political agenda" (p. 444). The fourth section analyzes relations between attitudes of youths (aged 16–20) and their parents. A special sampling procedure was used to add some 200 additional respondents to each sample in order to create pairs of youths and parents. The analysis compares both the average differences and similarities across generations in each country, and linkages between individual parents and their children.

Despite differences in focus and analysis from section to section, the concern with the forms and bases of political action provides a unifying theme. The authors build scales of two major types of political action—conventional participation and protest participation—and use them throughout. (They also present an interesting scale of attitudes toward political repression.) The conventional participation scale is a Guttman scale of the

actual frequency with which citizens engage in various political acts. The authors are aware of the multidimensional conceptualization of conventional participation developed by Sidney Verba, Norman Nie, and their associates, but opt for a unidimensional scale themselves. They justify their decision primarily by the largely non-American context, and note various differences between European and American settings. As Verba, Nie, et al. find similar multiple dimensions in seven nations, including two of the countries analyzed here (Austria and the Netherlands), the justification is clearly inappropriate. (See the summary in the appendices to Sidney Verba, Norman H. Nie, and Jae-on Kim, *Participation and Political Equality* [New York: Cambridge University Press, 1978].) In part the differences probably stem from the choice of items in the participation scale: voting is excluded, discussing politics is included, and the item about contacting officials does not specify the issue content. Future analysis will have to determine whether or not there are real differences in findings and what their implications may be.

More significantly, because proportionately few citizens had actually engaged in most protest acts, the authors chose to focus their analysis on a "protest potential" scale. This scale combines citizens' degree of approval of various "unconventional" activities with their responses to questions as to whether they had done or might do such acts. The acts themselves included signing petitions, participating in lawful demonstrations, joining boycotts, occupying buildings or factories, withholding rent or taxes, joining unofficial strikes, and blocking traffic. Few citizens approved of more violent or damaging activities, and these were excluded from the analysis. The limitations in making inferences about the actual use of protest are fully discussed by the authors. To some extent, however, the importance of this book to the study of protest depends on the credibility of such inferences or on further research establishing the linkages.

Among the many specific findings of interest, only a few can be mentioned here. The relationship between conventional participation and the potential for protest participation is notable. As we might expect, younger people in each country are likely to have higher protest potential, as are the politically dissatisfied. Yet, in each country, those who were more conventionally active were also likely to have greater protest potential. The common effects of education, income, greater ideological sophistication, and participatory values in creating resources for both forms of political action outweigh differences (pp. 93, 372–76).

A second particularly interesting point involves a cross-national double comparison. On the one hand, the comparison of partisan orientations and many of the values and attitudes examined suggests that Germany has the widest generation gap of any of the five countries. On the other hand, the "lineage" linkages between parents and children are on the average stronger in Germany than in any other country. (Moreover, in all the countries, and especially in Germany and Austria, open-ended questions show the youths feeling quite close to their parents.) Hence, as the authors point out, we need to be wary of attributing the appearance of generation

gaps to a breakdown in value transmission between parents and children (chap. 16).

As we might expect from a group of distinguished and experienced survey research analysts, the book is filled with fascinating descriptive findings and pieces of analysis. These are presented clearly and placed in the context of the (largely American) literature on attitude formation and political activity. The methodology of analysis is presented with great clarity in the appendices. There is no doubt that all students of individual behavior and attitudes will find results of consequence here, and that *Political Action* will remain an important point of reference.

The Status of Women in Preindustrial Societies. By Martin King Whyte. Princeton, N.J.: Princeton University Press, 1978. Pp. xi+222. $15.00.

Women and National Development: The Complexities of Change. Edited by the Wellesley Editorial Committee. Chicago: University of Chicago Press, 1977. Pp. xiv+346. $4.95 (paper).

Women United, Women Divided: Comparative Studies of Ten Contemporary Cultures. Edited by Patricia Caplan and Janet M. Bujra. Bloomington: Indiana University Press, 1979. Pp. 288. $15.00

Jane F. Collier
Stanford University

The three books reviewed here have little in common beyond the fact that all contain the word "women" in their titles and so are presumably aimed at a supposed women's studies market. Martin King Whyte's *The Status of Women in Preindustrial Societies* is a cross-cultural study using the Human Relations Area Files. *Women and National Development,* edited by the Wellesley Editorial Committee, reprints 22 of the 80 papers presented at an international conference on women and development held at Wellesley College in June 1976. Though also a collection of papers, *Women United, Women Divided,* edited by Patricia Caplan and Janet M. Bujra, was produced by a group of women who met together frequently, shared a common theoretical framework, and criticized each other's work.

Although the three books have little in common beyond their supposed market, they can, with a little imagination, be viewed as representing three points in the development of scholarly thought about relations between the sexes. They can, for example, be arranged so that the conclusions of one form the starting point for the next. Whyte's book on *The Status of Women in Preindustrial Societies* ends with the cheerful conclusion that industrialization promises greater equality for women. *Women and National Development,* which presents studies analyzing the effect of "development" on women in Third World countries, not only comes to a

largely opposite conclusion, but actually grew out of the fact that study after study has revealed the adverse effects of development on women who do not belong to elite minorities. This book concludes with a suggestion by Eleanor Leacock that the direction for women's studies "is to emphasize women's organizations, broadly defined, of the past, present, and future" (p. 322). Her implication is that women, by working together, can improve or have improved their lot and that of their sisters. The task of analyzing women's organizations is taken up by the authors of *Women United, Women Divided.* The London Women's Anthropology Group focused on "female solidarity" in the hope of understanding "sisterhood" and the genesis of "feminist consciousness," which they defined as "an awareness of gender inequality *and* the desire to end it" (p. 14). Their published case studies, however, record only one example of feminist consciousness (China), leading the authors to realize that "in considering women co-operating and supporting each other, [they] were facing a different analytical issue, which had more to do with the social and ideological manifestations of various forms of the sexual division of labor, than with feminist activity *per se*" (p. 14). In short, they found that women's groups often reinforced women's oppression or asserted class privileges instead of rejecting gender subordination (p. 15). Janet Bujra's introductory essay records the conclusions of the group. She rejects the assumption that "women, as women, can be treated as a uniform analytical category" (p. 18) and advocates instead beginning with the concept of mode of production, a concept that necessarily involves attention to class, history, and—for the authors of this book—the fact that humanity is composed of two sexes instead of only one. We have yet to see the results of studies beginning with this conclusion, but I, for one, have high hopes because it is a conclusion with which I concur.

Whyte's *The Status of Women in Preindustrial Societies* performs the invaluable service of debunking the idea that there is any such thing as a "general 'status of women' complex that varies consistently from culture to culture" (p. 116) and can be used to rank cultures or historical periods according to whether women's "status" is high or low, better or worse. Whyte does not state this conclusion until page 116, however, because to do so would be to obviate the purpose of his study, which was to test hypotheses about variables affecting the "status" of women (p. 27). He uses data from 93 societies to test 18 hypotheses, operationalized as 46 independent variables, 12 control variables, and 52 dependent variables. Although he concludes that there is no "general status of women" that can be measured, he does note that "the most consistent pattern to emerge [from his analysis was a] series of associations with measures of societal complexity, a concept that . . . serves as [his] 'proxy' in dealing with questions of social evolution" (p. 172). This is, of course, a conclusion that pleases scholars like me who believe that a sociological analysis of anything must be grounded in some concept of modes of production. Not every reader, however, draws from this book the moral I do. In scanning

other reviews, I noticed that other reviewers, like me, seemed to take from the book the preconceptions they brought to it. Reviewers who believe in a "general status of women" argued that Whyte's book showed we need a more complex formulation of it, rampant empiricists argued that it showed each society was unique, and reviewers who hate computers and the cross-cultural method concluded it was all bunk anyway.

Women and National Development can be read on two levels: either as findings or as data. On the first level, the book offers an excellent collection of scholarly papers about what is happening to women and about what women are doing in several "developing" countries. The 22 substantive papers are organized under seven headings: "Toward Models of Development," "Changing Modes of Production," "Women and Symbolic Systems," "Migrants and Women Who Wait," "Politics and Institutions," "Women, Education, and Labor Force Participation," and "Methodology and Data Collection." Because the volume editors had many conference papers to choose among, each published paper is of high quality, although neither within nor between topics do the papers have much in common with one another. This lack of commonality, in fact, is what establishes the book as data for anyone interested in analyzing the social production of knowledge. Any scholarly book can, of course, provide data for such an analysis, but this book is particularly useful because conference participants were partly aware of their role in producing knowledge. According to the five "Reflections" on the conference printed at the end of the book, conference participants, who came from several countries, were deeply divided over such issues as who should do research on Third World women, what research should be done, how results should be disseminated, what theoretical frameworks should be used, etc. (p. 313). These deep differences, however, are not reflected in the conference papers which, except for an occasional diatribe against Western scholarship, are all safely within one or another Western theoretical framework. The papers were, of course, written before the conference took place but, even so, their lack of agreement reveals the role of disagreements within an institutionally anchored tradition of staving off challenges to the unstated shared assumptions that make those disagreements possible.

Women United, Women Divided contains nine essays by nine authors who analyze women's organizations, or their absence, in 10 contemporary cultures, including two Western ones: Britain and Australia. The essays are longer than those in *Women and National Development,* and they share a common theoretical framework. The essays are a pleasure to read. They are not only well written, but also combine ethnographic richness with theoretical sophistication. In the end, however, the nine essays remain nine separate case studies. Their impact is not cumulative. As a result, Janet Bujra's introductory essay is less an introduction to this volume than a programmatic outline of the theoretical considerations that should guide future research. Her essay, in fact, has been receiving considerable attention from feminist scholars, both for its penetrating criti-

cisms of previous research frameworks and for its suggestion that future studies of relations between the sexes should be grounded in an understanding of modes of production: "Rather than analyzing 'the position/role/status of women,' attention should be diverted towards understanding the articulation of domestic labour with differing modes of production" (p. 20).

Sexual Harassment of Working Women: A Case of Sex Discrimination. By Catharine A. MacKinnon. New Haven, Conn.: Yale University Press, 1979. Pp. xiv+312. $22.50 (cloth); $6.95 (paper).

Mark E. Beatt
Columbia University

The sexual harassment of working women has only recently been recognized as a pervasive social problem. The tacit assumption that the practice is legitimate for men to engage in, and taboo for women to confront, even to themselves, has come under increasing attack from feminists and their supporters. As awareness of the problem has grown, women have begun to turn to the courts for redress of their grievances. Catharine A. MacKinnon's timely study is the first comprehensive treatment of both the social and the legal issues raised by sexual harassment.

MacKinnon makes several valuable contributions in *Sexual Harassment of Working Women.* First of all, she advances our understanding of the scope and nature of sexual harassment. Through an analysis of newspaper and magazine accounts, as well as the few social surveys that have been done, she makes a convincing case that it is extremely widespread. Moreover, her in-depth interviews with working women sensitize us, particularly the males among us, to how abusive, humiliating, and oppressive the experience is for those on the receiving end. MacKinnon is quick to point out, however, that sexual harassment is not simply a personal injury, but a social injury that occurs on a personal level. She argues that sexual harassment is the result of a confluence of authority relations and sexual interest in a society stratified by gender; that it is a practice which expresses and also reinforces women's inferior labor market position and the traditional social roles of men and women.

After defining the meaning and detrimental impact of sexual harassment, MacKinnon examines the few legal cases that have been decided. Most opinions are found to be uncompelling, inconsistent, and ideologically sexist. The general response of judges to women's complaints of sexual harassment has been "to consider them 'personal' incidents, 'natural' expressions, or both" (p. 83). These labels respectively trivialize and monumentalize sexual harassment, simultaneously making its eradication seem impossible and undesirable. Both characterizations not only rationalize legal noninvolvement, they "make sexual harassment socially and culturally

permissible by locating its determinants beyond the social and cultural sphere" (p. 83).

MacKinnon's aim is to change judicial thinking about the matter. Her thesis is that the sexual harassment of women at work constitutes sex discrimination in employment under the Equal Protection Clause of the Constitution and related statutes. In supporting this thesis, she offers a new interpretation of the clause. Traditional equal protection theory, which MacKinnon calls the "differences" approach, is based on the principle that similar persons should be treated similarly. Because women and men are rarely comparably circumstanced, the courts have frequently upheld laws and official practices which disadvantage women. At the same time, they have often overruled regulations designed to overcome the effects of past discrimination on the ground that they make arbitrary distinctions. Thus, despite its prohibition of overt discrimination, equal protection law has not significantly altered the subordinate social position of women.

MacKinnon contends that equal protection law should focus on the issue of inequality rather than on equal treatment. She argues that the courts should concentrate on whether the legal treatment of women participates in their systematic social deprivation. Accordingly, the only question for litigation should be "whether the policy or practice in question integrally contributes to the maintenance of an underclass or a deprived position because of gender status" (p. 117). This "inequality" approach merits serious consideration. It would force the courts to take account of the fact that many laws, although neutral on their face, have the effect of perpetuating societal discrimination. It would also direct their attention to the group-based nature of inequality. Most important, it lends support to those affirmative action programs which may be essential to the achievement of true equality.

MacKinnon goes on to maintain that sexual harassment is sex discrimination under both approaches. It is sex discrimination per se under the differences approach because it differentially injures women in a sphere—sexuality in employment—in which men and women can be viewed as similarly situated. And it is sex discrimination under the inequality approach because it disadvantages women at a point where their sexuality and material survival are joined. Legal recognition of these perspectives, particularly the latter, could significantly advance women's drive for economic equality and sexual self-determination.

Although MacKinnon's arguments are very persuasive, they are not flawless. Throughout the book she strongly implies that the perpetrators of sexual harassment are hierarchically superordinate men and that the victims are subordinate women. A recent study of federal workers by the Merit Systems Protection Board, however, found that females were more likely to be sexually harassed by their co-workers than their superiors, and that 15% of the males surveyed had been recently harassed. While these findings do not represent a serious challenge to MacKinnon's conceptual scheme, they underscore the need for rigorous social scientific research. I hope sociologists will build on this fine work.

The Reception of Unconventional Science. Edited by Seymour H. Mauskopf. Boulder, Colo: Westview Press, 1979. Pp. x+137. $13.25.

Lawrence H. Stern
Barnard College/Columbia University

Significant departures from previously accepted claims to knowledge are, in part, structurally induced via the reward system of science, and they pose a constant and formidable problem for science. How are they to be received by the relevant scientific community? Often, such new claims are initially perceived as being "unconventional" or "unorthodox" (terms in dire need of explication). Nevertheless, some eventually become accepted and may even restructure existing cognitive areas, while others may be partially incorporated into the existing corpus of knowledge. A good many, however, are either ignored, actively resisted, rejected, or possibly banished from the scientific community. Investigations attempting to identify and analyze systematically those social and cognitive factors and processes affecting the type of reception accorded to this generic type of knowledge claim—claims perceived as unconventional *and* as potentially of high impact—have appeared over the past few years in the history and sociology of science. Four such studies, originally presented at the 1978 meetings of the American Association for the Advancement of Science, are contained in *The Reception of Unconventional Science,* edited by Seymour Mauskopf. An introduction by the editor and a general discussion by sociologist Marcello Truzzi are also included.

The first two cases presented, Paul Forman's analysis of the differential reception accorded to quantum mechanics in Germany and Britain and Henry Frankel's analysis of the initial and later reception of the theory of continental drift by the geological community, both deal with knowledge claims that eventually won widespread acceptance; the former very quickly, the latter after a period of nearly four decades. The final two cases, in contrast, deal with knowledge claims which have still not been accepted or granted widespread legitimacy in their respective domains. John Bowers gives a brief historical account of the origins of acupuncture and the way it has been received by Western science, while Mauskopf and Michael McVaugh focus on one episode in the controversy surrounding the emergence of parapsychology in the 1930s—the debate over the validity of the statistical analyses used by Rhine and his co-workers to substantiate their claims.

In his introduction, Mauskopf, a historian, discusses these cases in the context of an issue which has preoccupied many historians and philosophers of science for years: the so-called internalist-externalist debate. Can the development of scientific knowledge be best explained through the operation of rational intellectual processes and antecedent ideas, or is it best explained by extrascientific factors found in the broader social and cultural context? Or can the two approaches be profitably combined and lead to

a better understanding of scientific development and change? The issue is certainly not resolved here. Indeed, the relative emphasis given to elements of each approach varies considerably in these papers.

Forman, a specialist in the history of physics, sets out to demonstrate that the broader intellectual environment of Weimar Germany, in contrast to the one found in Britain, facilitated the creation of, the receptivity to, and the immediate acceptance of quantum mechanics, the essential mathematical tools for studying atomic and subatomic physics introduced by Werner Heisenberg in 1925. Drawing on historical accounts of this period, Forman demonstrates that after Germany's unexpected military and industrial collapse at the end of 1918 a new Zeitgeist emerged, one emphasizing "intuition," a celebration of and "experiencing" of life. The very notion of "causality," an underlying assumption thought to be basic to the pursuit of science, came under attack, as did the scientific enterprise more generally. Physicists, Forman argues, were neither oblivious to nor unaffected by this change, and as a response to their loss of prestige in Weimar culture they actively sought to adapt the content of their science to the values of their intellectual environment. Thus, the creation and reception of quantum mechanics, with its implicit indeterminacy, was facilitated by this "climate of opinion" which impinged on the scientific community. British physicists, however, working in a more hospitable intellectual environment, one where anticausal inclinations were not characteristic, were, for the most part, oblivious to the broad epistemic implications of quantum mechanics and thus received this innovation with more reserve.

In direct contrast to Forman's approach, Frankel, a philosopher of science, turns to factors internal to science to account for the initial rejection and, four decades later, the acceptance of the theory of continental drift. Guided by Larry Laudan's theory of scientific development, Frankel argues that *both* the initial resistance to and rejection of drift theory *and* its subsequent acceptance were entirely appropriate and the results of a process of rational appraisal within the discipline itself. He derives three very general theses from his reading of Laudan and purports to test their applicability to his present case (in reality, two cases). Here, however, difficulties arise. Most scholars trying to test the theories of philosophers of science have stumbled over the problem of finding adequate indicators for the essential component parts of these theories. Frankel, it appears, is no exception.

Neither of these first two cases should be seen as an exemplar or even a representative specimen of a purely externalist or internalist approach. Each account tacitly incorporates factors from each approach. Both of the final cases—Bowers's description of the reception accorded to acupuncture by Western science and Mauskopf and McVaugh's analysis of the debate surrounding Rhine's use of statistics in the presentation of parapsychological claims—*explicitly* utilize a combination of internal and external factors. Although this is a step in the proper direction and one that should be encouraged, neither paper appears to employ this method in a systematic

way. Agreement has yet to be reached on precisely which social and/or cognitive factors or processes affect the reception of knowledge claims, unconventional or not, to say nothing of what the relative impact of these factors might be or how they might interact. Nor have the conditions that facilitate or constrain the impact of these factors been specified.

Perhaps enough case studies have accumulated in the literature so that a broad conceptual scheme can be developed to deal with the preceding questions and also to generate new ones. It appears too that the very notion of "reception" must be further specified so as to identify its many facets and capture its variability and dynamic nature. The reception *process* must then be examined from various vantage points. How is the knowledge claim, as it changes and becomes modified over time, differentially received by individual scientists, competing research groups, and the broader scientific community? How, too, does the *type* of knowledge claim introduced, as well as the one being challenged, affect this process? Are we dealing with an isolated anomalous observation or a new theoretical orientation? Work touching on various aspects of this problem area comes in many guises and from a variety of theoretical perspectives. The growing literatures on consensus formation in science, the workings of the evaluation system, the negotiation of standards of evidence within a specialty, theory choice, the resistance of scientists to scientific discoveries, and the conduct of scientific disputes and controversy all deal in some part with the more general problem of reception and are especially pertinent. The cases reported in this volume may provide source materials for the task that remains.

American Academics: Then and Now. By Logan Wilson. New York: Oxford University Press, 1979. Pp. 309. $13.95.

Richard Hagman
University of Chicago

In the 19th century, essays and books on higher education often were exhortations. When they were not, they were elegant essays on a variety of topics having to do with higher learning. Sometimes, as in the case of John Stuart Mill's *Inaugural Address at St. Andrew's University,* they were both. Early in the 20th century we abandoned lofty tones and labored styles. Perhaps Thorstein Veblen, in his *Higher Learning in America: A Memorandum on the Conduct of Universities by Businessmen,* ushered in a new way of discussing higher education. Whatever the cause, with a few notable exceptions, most of our literature on higher education bears the stamp of sociology, political science, or economics. We are clearly more interested in description than in advice. In *American Academics: Then and Now* by Logan Wilson, we have a comprehensive introduction to the academic way of life by a sociologist whose 40 years of observations and research are condensed into an immensely informative book.

The value of *American Academics* lies in its treatment of several unwieldy subject matters: the academic profession, the sociology of scholarship and scientific research, and the hierarchy within and among institutions of higher education. Wilson draws on his own research and on studies he has read closely over his many years as an administrator. The synthesis of disparate observations on the nature of higher education in the United States, the interpretation of not-so-obvious research on the academic profession, and the summarizing of sizable bodies of information are activities that could be produced only by someone of Wilson's ability and erudition. In short, were one assigning a single book to a class eager to learn something of the character of the academic profession, one would now have a good work with which to start intelligent discussions.

What I should like to discuss in this brief essay is Mr. Wilson's use of information. Take, for instance, the use of documents sufficient to produce tables with the following titles: (1) "Professed Reasons of Being in Graduate School"; (2) "All Current Sources of Financial Support for Advanced Study"; (3) "Rank Order of Top Twenty Fields by Doctorates Conferred"; (4) "Average Time Reported by 180 Presidents on 18 Activities"; (5) "The Over-All Power Structure of American Universities"; (6) "Importance of Various Factors in Evaluating Faculty for Promotion, Salary, or Tenure"; (7) "Reported Faculty Hours per Week in Teaching Activities and in Research and Scholarly Writing"; (8) "Faculty Unionization; Average Salaries and Average Compensations by Category (Affiliation, and Academic Rank)"; (9) "Attitudes of Faculty Members, by Age"; (10) "Periodicals Ranked by Size of Faculty Readership"; (11) "Comparison of the Physical Resources of the Thirty Leading Graduate Centers"; (12) "Institutional Standings of Graduate Faculty in Leading Universities." These are representative of the tables used throughout the text. They are supplemented by appendices containing such information as a summary of the 47 recommendations of the Commission on Academic Tenure in Higher Education and the Carnegie Commission's 26 recommendations on academic governance.

Wilson makes extensive use of many of the important documents in the recent history of higher education and offers this explanation in the first page of his introduction:

> This book about American academics is in some ways a belated follow-up of an inquiry a good many years ago into the sociology of a profession. When I wrote *The Academic Man* (New York, Oxford University Press, 1942), I was a fledgling academic and sociologist, curious about the folkways of my chosen occupation. In the early states of study, I discovered that answers to many of the questions I had in mind were not between the covers of any single volume and that some topics were relatively unexplored. Knowledge about the profession as a whole seemingly could be acquired only through life experience in the college and university world, or by reference to widely scattered and unorganized sources of information.
>
> One of my purposes in writing *The Academic Man* was to bring together the most significant observations, organize them within a pragmatic frame

> of reference, and present an ordered and objective view of professionals engaged in the higher learning.

The same purposes are at work in *American Academics*. In fact, there is a constant reference to *The Academic Man,* which serves as the basis for the "then and now" comparison mentioned in the title.

Wilson is without equal in assembling answers to questions only he, with his laudable curiosity about the nature of academic life and institutions, has endeavored to ask in systematic ways over the past 40 years. He is successful in making interesting reading of his many summaries of studies conducted everywhere in American higher education over the past several decades. That we have no other, single, readable text on the academic profession—at least none so full of information as this one—will become obvious to the reader. The book is not a discussion; it is a dictionary.

As a reference tool, *American Academics* will prove useful for many years to come. If I have a single complaint, it has to do with the scarcity of Wilson's own excellent thought. That thought shows itself in his masterly treatment of data, in his style, and even, occasionally, in such statements as: "All of the competitors [academics] in this system [higher education] are afforded the satisfaction of participation in a collective undertaking of unbounded social significance. And regardless of their creative accomplishments as advancers of knowledge, all academics can experience the rewards of cultivating the minds of others, and thus also make their contributions to the higher learning. It is no wonder, then, that most academics find both challenges and rewards in their chosen profession." This is the concluding statement in *American Academics,* and it raises interesting questions: (1) What is the "unbounded social significance" of higher education? (2) What are the "rewards of cultivating minds of others"? (3) Why has Wilson, whose lifetime experience in the academic profession qualifies him to hazard answers to these two important questions, not done so?

I phrase the last question in the form of a complaint, a very small complaint, because higher education and the academic profession need several kinds of leadership. Wilson has supplied one kind by his scholarly example, and in his long administrative career he has supplied another. The third kind of leadership, if indeed there is a finite number, is the kind supplied by John Stuart Mill through steady reflection on the nature and purposes of higher education, and through his subsequent advice.

Wilson has devoted years to the study of the academic profession, and it is unfortunate that *American Academics* is almost entirely free of the advice we could expect and, I believe, should want from the Logan Wilsons of our profession. In our zeal for social scientific description, we have been vociferous in our deprecation of opinion and advice, labeling all pronouncements "pious rhetoric." Nevertheless, my hope that we will yet have exhortations from Wilson outweighs my small disappointment in the present work.

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TRANSLATED BY MARGARET CROSLAND

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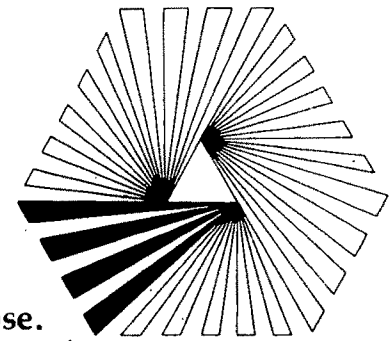
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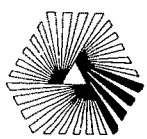


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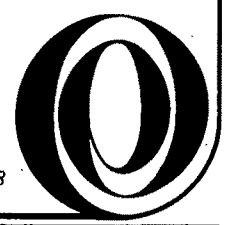
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IN THIS ISSUE

HARRISON C. WHITE is a member of the Department of Sociology at Harvard University. Further developments of the market model in this issue, applied to new varieties of market situation, will appear in a forthcoming volume on structural and network theories, edited by Barry Wellman and Stephen D. Berkowitz, and in a festschrift for Tom Burns being edited by Gianfranco Poggi, Frank Beckhofer, and others. These two new papers supply asymptotic formulae for special cases and dual forms applicable to labor markets, respectively.

OLIVER E. WILLIAMSON is Charles and William L. Day Professor of Economics and Social Science at the University of Pennsylvania. He received an S.B. degree from MIT in 1955, an M.B.A. from Stanford University in 1960, and a Ph.D. from Carnegie-Mellon University in 1963. His recent research deals mainly with the study of economic organization, with emphasis on transaction cost economizing. It joins economics with aspects of law and organization theory.

RONALD L. BREIGER is professor of sociology at Cornell University and a research associate at the university's Institute for Social and Economic Research. His current work includes comparative research on social class, mobility, and community elites, studies of the evolution of role structures, and the further development of network theory and models for the aggregation of social categories.

LEO A. GOODMAN is the Charles L. Hutchinson Distinguished Service Professor of Sociology and Statistics at the University of Chicago and a faculty research associate at the Population Research Center of the University. He is developing mathematical models and statistical methods for the analysis of social processes. The paper in this issue is concerned with occupational mobility and the definition of occupational classes; the methods applied to these matters were developed, in part, in his 1979 article on association models in the *Journal of the American Statistical Association.* His article in the 1981 *Sociological Methodology* provides an overall view of the main ways in which loglinear models and the corresponding statistical methods can be applied.

ARNE L. KALLEBERG is associate professor of sociology at Indiana University, Bloomington. He is extending the research reported in this paper by analyzing the impact of economic segmentation on noneconomic rewards and career processes. He is also engaged in historical and time-series analyses of printing and other selected industries, and is directing the 1982 Indianapolis Area Project, which will examine the determinants of worker attitudes and behaviors in a sample of business firms in Indianapolis.

MICHAEL WALLACE is a doctoral candidate in the Department of Sociology at Indiana University, Bloomington. His current research activities include an ongoing study (with Arne Kalleberg) of industrial processes and work relations in the printing industry. His dissertation is entitled "Working

Class Organization and Labor Militancy: A Comparative Analysis of the U.S. and Canada in the 20th Century."

ROBERT P. ALTHAUSER is associate professor of sociology at Indiana University. His current area of substantive research is stratification and labor markets, in particular the consequences of holding jobs located in different types of markets and the methodological problems arising out of this interest. At present he is working on analyses of employee job histories within several firms, in an attempt to determine where patterned movement across job titles exists and to associate job ladders with types of labor markets.

DAVID JACOBS is associate professor of sociology at the University of Maryland, Baltimore County. His research centers on questions in two areas: organization theory and the political economy of industrial societies. His current projects include a continuation of work reported in this issue and an empirical study of the relationship between aggregate inequality and variables stressed in the literature on segmented economies.

Information for Contributors

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Davis, K. 1963*a*. "Social Demography." Pp. 124–37 in *The Behavioral Sciences Today*, edited by Bernard Berelson. New York: Basic.

———. 1963*b*. "The Theory of Change and Response in Modern Demographic History." *Population Index* 29: 345–66.

Goode, W. J. 1967. "The Protection of the Inept." *American Sociological Review* 32: 5–19.

Moore, Wilbert E., and Arnold S. Feldman. 1960. *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt, ed. 1962. *The American College*. New York: Wiley.

Weber, Max. (1921) 1968. "Society's Problems." Pp. 12–16 in *Economy and Society*, edited by Guenther Roth and Claus Wittich. New York: Bedminster.

Where Do Markets Come From?[1]

Harrison C. White
Harvard University

Production markets have two sides: producers are a fully connected clique transacting with buyers as a separate but aggregated clique. Each producer is a distinctive firm with a distinctive product. Each side continually monitors reactions of the other through the medium of a joint social construction, the schedule of terms of trade. Each producer is guided in choice of volume by the tangible outcomes of other producers—not by speculation on hypothetical reactions of buyers to its actions. Each producer acts purely on self-interest based on observed actions of all others, summarized through a feedback process. The summary is the terms-of-trade schedule, which reduces to constant price only in limiting cases. The market emerges as a structure of roles with a differentiated niche for each firm. Explicit formulae—both for firms and for market aggregates—are obtained by comparative-statics methods for one family of assumptions about cost structures and about buyers' evaluations of differentiated products. Not just any set of firms can sustain terms of trade with any set of buyers. There prove to be three main kinds of markets, and three sorts of market failure, within a parameter space that is specified in detail. One sort of market (PARADOX) has a Madison Avenue flavor, another is more conventional (GRIND), and a third (CROWDED) is a new form not included in any existing theory of markets. Current American industrial markets are drawn on for 20 illustrations, of which three are presented in some detail. Inequality in firms' market shares (measured by Gini coefficients) is discussed.

Why do so many of our industrial markets have but a dozen or so member firms, several of which produce substantial shares of the total output (Scherer 1970; Porter 1980)? It is not enough to cite technological constraints.

Why, when even the largest of firms wants to offer a product new to it to the public, does it usually do so by acquiring the persona of a firm

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belonging to an existing market? This astonishing fact seems to be overlooked by existing theory; practitioners and consultants take it for granted (see Salter and Weinhold 1979, pp. 7–16; Steiner 1975, pp. 192, 200).

Why do economists accept a theory of firms in markets which denies in principle the most commonly observed situation of firms? Most industrial firms most of the time decide production volume within a range where their unit costs are constant or decrease as volume increases (see Ijiri and Simon 1977, p. 7).

I developed some tentative answers to such questions in an earlier technical paper on production markets as induced role structures (White 1981). Here I wish to flesh out the argument and illustrate it by application to a number of current U.S. markets.

What I have proposed is embedding economists' neoclassical theory of the firm within a sociological view of markets. Markets are self-reproducing social structures among specific cliques of firms and other actors who evolve roles from observations of each other's behavior. I argue that the key fact is that producers watch each other within a market. Within weeks after Roger Bannister broke the four-minute mile, others were doing so because they defined realities and rewards by watching what other "producers" did, not by guessing and speculating on what the crowds wanted or the judges said. Markets are not defined by a set of buyers, as some of our habits of speech suggest, nor are the producers obsessed with speculations on an amorphous demand. I insist that what a firm does in a market is to watch the competition in terms of observables.

In my proposal, markets are social structures in which producers reproduce their own set of actions; the set confirms as correct each firm's expectations of what it hoped was an optimal volume. This view is a special case of "rational expectations" (Muth 1961; for a recent survey, see Kantor [1979]). In this feedback model there is also a self-selection aspect derived from the "signaling theory" of Akerlof (1976), Spence (1974), and others intrigued by notions of "imperfect information."

A modest generalization of the notion of price is required: generalized to a market schedule of observed outcomes. These observed outcomes are a set of pairs, one pair for each firm: revenue received for volume shipped. Look at the hypothetical outcome sets in the panels of figure 1. Figure 1*A* outcomes cannot sustain a market, figure 1*B* outcomes can. Figure 1*C*, in which a curve is interpolated through the points of figure 1*B*, is one way a firm may visualize those outcomes, revenue (W) as a function of volume (y). This schedule must not be confounded with the demand function, a hypothetical construct of economists. This schedule may be perceived in terms of price (revenue/unit), but it is a volume-dependent price. As will be seen below, this generalization of the conventional notion of price is crucial for my approach.

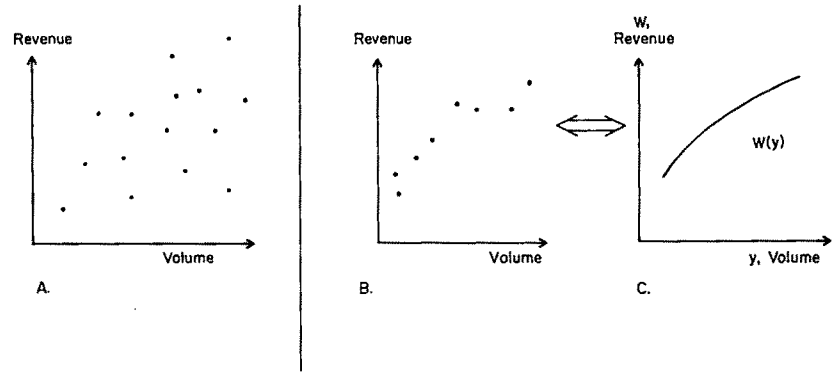


FIG. 1.—Outcomes for each firm in a clique. *A*, Outcomes do *not* sustain a market. *B*, Outcomes *do* sustain a market. *C*, Market schedule inferred from *B;* decisions using it sustain it.

Because it frames the only hard, tangible evidence available, each firm treats $W(y)$ as its own opportunity set. Obtaining even that much information requires alert inquiry—over luncheons with others in the trade, from trade associations, from one's own customers, and so on. Each firm knows that its product is distinctive, but it also knows the difficulty and risk of assessing one's own distinctiveness (see Corey 1978; Porter 1976). In particular, when the total volume one offers in the market changes, its attractiveness to buyers changes, in ways hard to estimate. No firm can reliably assess relative qualities of other firms, and every firm knows that its position could be affected by choices made by any one or more of its competitors. The market schedule $W(y)$ is a shared social construction incorporating all the interaction effects.

Such mutually interlocking confirmations of a unique choice by each producer (fig. 1*B*) are not possible for any arbitrary collection of producers and any population of buyers. A variety of attributes may distinguish individual producer firms (product quality, location, plant investment). On the one hand, some of these attributes affect cost structures (e.g., figs. 4 and 7 below) and thus the production level which looks optimum to that firm. On the other hand, some of these attributes influence attractiveness to buyers. A self-confirming market schedule, $W(y)$, is induced here for a clique of products whose cost structures and taste structures can each be ranked by quality.

A market is an "act" which can be "got together" only by a set of producers compatibly arrayed on the qualities which consumers see in them. Quality arrays have recently figured in microeconomics under the rubrics of "hedonic prices" (Terleckyj 1976) and "consumer as producer" (Lancaster 1966, 1979). Nearly 20 years ago Alonso (1964) proposed an array for locations in a city, where distance from the center provides a natural metric

for equality but also geometric constraints on areas, his analogue of volumes; more recently Rosen (1974) has generalized this schematization to imperfect competition.

Building a market is a conflict-ridden and erratic process with quite a range of outcomes possible in the form of market schedules. The various firms' products will presumably be akin within a market, but we do not as observers impose any a priori cultural or linguistic criterion of similarity. Markets are defined by self-reproducing cliques of firms, and not the other way around.

The body of this paper is divided into a section on the general model and a section on applications and results.

THE GENERAL MODEL

Firms in the market differ from one another, not only in cost structure but also in appreciation of their products by the buyers. These dispersions occupy center stage in my analysis. In this respect I follow the long-standing tradition of economic studies of "imperfect competition" initiated by Chamberlain (see Dixit and Stiglitz 1977; Spence 1976). But that tradition has firms using conjectures on buyer taste to decide their market offers. In contrast, my view, presented above, is that firms decide on the basis of observed positions of all other producers.

In my view, firms seek niches in a market in much the same way as organisms seek niches in an ecology. Because each firm is distinctive, they are engaged not in pure competition but in finding and sustaining roles with respect to one another given an environment of discerning buyers. But there is no auctioneer to shape the market; instead, its structure depends on the interlocking of local orders. This leads to the postulate that firms with neighboring cost schedules (amount of variable cost to produce various volumes) must also have, in the eyes of buyers, neighboring schedules of valuation with respect to volume. If the postulate is not satisfied, the nascent market situation, a set of producers with an attendant population of buyers attracted by them, cannot sustain itself: $W(y)$ will not be reproduced through the self-interested actions of firms, checked by buyers.

In an observed market, the producing firms are dispersed in quality of product perceived by buyers as well as in volume produced. By the postulate of the preceding paragraph, neighboring qualities must lead to neighboring volumes of production. In the model each firm is assigned a value on an index of quality, denoted by n; the value assigned is characteristic: an attribute which cannot be changed quickly as can the volume of production. The volume of production is denoted by y.

The key feature of my approach is this: *Firms can observe only volumes and payments, not qualities or their valuations, and they act on the basis of*

these observations, thereby reproducing the observations. My model, however, can predict all these volume choices by different firms because it assumes knowledge of quality, n, for each firm, as well as valuations. (Higher values on this index are defined as higher quality—always as judged by buyers' evaluations.)

First I develop the cost and valuation schedules, and then I derive the range of market schedules that may result from a given set of facts. A topology of markets—a two-dimensional array with each point a particular shape of payment schedule—follows as a by-product. The varieties and implications of market failures are then discussed before the second part of this paper: applications and predictions for industrial markets.

The central theme proves to be a trade-off between dispersions, an affair of variances rather than the matter of means one might expect from the cliché of supply equaling demand.

The Facts of Cost and Value across Volume and Quality

The primitives of my description are two schedules (of cost and of valuation), each given in terms of two dimensions (volume y and quality n). If the facts about a set of firms and buyers cannot be approximated in terms of nested schedules,[2] those producers and customers will not be able to sustain a market which reproduces itself as in figure 1. In order to achieve a comprehensive yet clear inventory, I shall specialize to particular families of power functions to describe possible schedules.

The schedule of the firm characterized by n for variable costs of production is

$$C(y; n) = qy^c/n^d, \text{ with } q \text{ and } c \text{ positive}. \quad (1)$$

The firm's contribution to buyer valuation is defined as

$$S(y; n) = ry^a n^b, \text{ with } r, a, \text{ and } b \text{ positive}. \quad (2)$$

Intuitively it is clear that a balance must be struck by the market between the trade-off taking place between *contribution* and *cost*, with respect to more volume or less volume, on the one hand, and the trade-off between *desirability* and *expense* with respect to quality, on the other. Equations

[2] *Nested* means simply that the function describing a schedule defined by one value of the parameter (e.g., n) never crosses the function for another value of the parameter. (The concept, though slippery, is familiar and important in other areas, such as Lorenz distributions; see Schwartz and Winship [1980].) This assumption, as well as the more basic assumption that each empirical schedule can be represented by a perfectly sharp function (see n. 10 below), clearly is more stringent than necessary to obtain the main findings. But to simplify explanation of the theory in these first papers, I make not only these assumptions but also the further assumption that particular Cobb-Douglas (power-law) forms are appropriate for the schedule. Elsewhere I have explored alternative functional forms.

(1) and (2) yield the simplest family of schedules that allow these four independent variations of schedule as measured in total dollars and with respect either to physical volume y of production or to its quality n.

Figure 2 schematizes the phenomenology underlying these schedules. It explicates equation (1) as the variable cost of producing the volume y chosen by a firm being the product of the volume-sensitive *cost* and the quality-sensitive *expense*. Cost must increase with volume so that the exponent c is positive; this exponent gives the proportionate logarithmic rate of increase with volume. *Quality*, unlike volume, is in the eye of the beholder, here the buyers, and therefore is an exogenous "social fact" confronting the producers; so the exponent d can be either positive or negative. When positive, d describes what I denote below as a PARADOX market in which a producer whose product is liked better finds it less costly to make! There is in figure 2 a parallel rationale for equation (2). By definition, the exponent b for desirability is positive; one could even insist that it be set equal to unity, but I prefer not to constrain the scaling of quality values n for different firms' products in that way (see discussion of tables 1 and 2 below).

On the buyer side there is an asymmetry which is not captured in figure 2. Firms are the active decision makers; each has an independent cost schedule

i) Phenomenology of Market Context

Schedules	Dispersions: on Volume (y) of Firm's Production	Dispersions: across Firms on Quality Index (n)
Valuation	Contribution of the product increases with volume, as perceived by the buyers	Desirability of the product increases with quality as judged by the buyers
Costs	Cost of production increases as volume increases	Expense of building in the quality changes (+ or −) with increased quality
	Increases With Volume	Changes With Quality

ii) Parameters -- Proportionate (log) Rates

a	b
c	d

iii) The Basic Tradeoffs

-- Over variation, in product volume: $\frac{\text{Contribution}}{\text{Cost}} = a/c$

-- Over variation in producers' quality: $\frac{\text{Desirability}}{\text{Expense}} = b/d$

FIG. 2

known to itself (at least). Buyers, on the other hand, are lumped together as an aggregate, in a passive role. The aggregate buyer may say "no" to the market entry (volume and price pair) offered by a firm, but it has only this binary choice. This binary decision depends on how the buyers in aggregate evaluate more of one firm's product against less of another's. To simplify the model further, I assume that buyers do not see particular pairs of products in interaction, but only the set of products. In formal terms, the buyers in aggregate value their total array of purchases as

$$\mathbf{V}(\#) = [\Sigma\, S(y; n)]^{\gamma}\,. \tag{3}$$

The symbol $\mathbf{V}$ for this valuation is in boldface type to signal that it is an aggregate quantity over the whole market: I adhere to this convention throughout. The symbol # gives the number of firms in the market and hence the number of terms in the summation. Observe that the contribution of one firm's product volume to the total valuation can replace the contribution of any other firm. In the special case in which the exponent a is unity, further volume increments from a given firm are neither more highly valued (as they would be with $a > 1$) nor less highly valued than a beginning volume. More decisive is the overall exponent γ (gamma) which is usually less than unity; this corresponds to a saturation of taste, for the sum of purchases from all firms in the market, by the given aggregation of buyers as the aggregate volume purchased increases.[3]

The scale factors q for cost schedules (eq. [1]) and r^{γ} for valuation schedules (eqq. [2] and [3]) are worth keeping distinct. In the section on applications, changes in these will be interpreted as exogenous shifts in cost and demand imposed on the market. I do not define a distribution of firms over the quality index n, only their number. In this view of markets there are relatively few member firms as producers, as few as a half dozen, and there is no reason to suppose any particular spacing on the quality index among those few: These are best treated as a set of constants to be fitted to the observed producers (as is done in several empirical examples later).

Certain special cases bear considerable weight in the past development of theories of markets, a point I develop at length elsewhere (White 1980). Setting the exponent b to zero reduces the market to pure competition: products of different firms are indistinguishable. Perfect competition models assume in addition that there is no taste for sheer diversity on the part of

[3] In this simplified representation (eq. [3]) of aggregate buyer evaluations across products, I follow recent innovations of microeconomic theorists (e.g., Spence 1976; Rosen 1974; and see Lancaster 1966). Note that demands for different products interact in this representation *only* because γ is not unity for that market. Each S function reports propensities toward evaluation based on that product's attributes, but only in the context of the other products can it be incorporated into an actual valuation ($\mathbf{V}$) with monetary dimension. Observe that the contribution, at the margin, to $\mathbf{V}$ from any particular firm is its S times $\gamma/\mathbf{V}^{(1-\gamma)/\gamma}$.

buyers—in my terms the exponent a is set at unity. It is in this highly specialized context that the notion of supply equaling demand became prominent. Below we see that, although equilibration of aggregate amounts sought and sold is certainly necessary, it is a secondary outcome of the main issue, which is the terms on which aggregation can be carried out meaningfully with diverse products. Supply and demand denote one aspect of the feedback processes which shape markets, but not the behaviorally relevant one or, therefore, the theoretically relevant one.

Motivations and Equilibrium Schedules

Each firm, characterized by its quality n, chooses its volume of production, $y(n)$, to maximize its cash flow, which is the excess of market receipts over out-of-pocket costs. (In this I follow neoclassic orthodoxy and disregard the behaviorist argument for cognitive and motivational limits to optimization.) In equilibrium the frame of choices used must be exactly the observed market schedule defined in figure 1: the $W(y)$ of market receipts for volume shipped.[4] It is the same frame for each firm, even though we as observers know just how firms differ on quality and thus in cost and valuation schedules.

How can this be? It can be only if each firm, because of its own cost structure, is led by the same common schedule $W(y)$ to choose a distinctive niche $y(n)$ of its own, which niche furthermore satisfies the aggregate buyer that it constitutes as good a buy as any other producer is offering. This is my definition of a market, which I shall now formalize and extend.

The buyers in aggregate are able to impose only a necessary condition (eq. [4]) for their net benefit to be maximized. By definition, their net benefit is their total evaluation of purchases, the V of equation (3), less the sum of the payments made to all # firms, the sum of $W[y(n)]$ over each firm. The

[4] A scenario can help as illustration. Suppose some particular firm, say Medusa (actual name of a firm in the market-for-cement illustration later) thinks it can command more than this schedule. Suppose Medusa picks a particular volume, y_0, at which it hopes to get a revenue A, which is larger than the schedule amount $W(y_0)$ and optimizes Medusa's net return, given its hypothesized schedule lying above the $W(y)$ drawn through observed market entries. Suppose y_0 is larger than $y(n)$, where $y(m)$ is the (unique) volume which optimizes Medusa's net return given the existing $W(y)$ and yet keeps Medusa's offerings in line as no more or less attractive to buyers than the other firms' offerings. If volume y_0 is actually sold, less than the volumes indicated from previous market results for the other firms will actually be sold by them; so their optimization computations are falsified, and they must scramble to readjust by lowering prices and/or volumes in search of a self-confirming schedule. But then even the $W(y_0)$ level scorned by Medusa may not be sustained in the lowered schedule eventuating from the scramble, so that Medusa's hope for revenue A need not be validated, and in any case it will perceive a changed situation and a recomputation of optimal volume. More likely, not all the intended y_0 will be sold, since the price was above the price sustainable from the market schedule, at $y(m)$, and indeed at too high a price none at all might be sold, stimulating each of the others to expand a bit to fill a perceived gap. In either event, the $W(y)$ market schedule will be unstable. I therefore suppress such transient readjustments and focus only on what mutual configurations of volumes and prices among firms can sustain themselves in equilibrium.

necessary condition is that each firm's $y(n)$ yield the same ratio of contribution to V over payment $W[y(n)]$. Incorporate this ratio into θ (theta) (see n. 3):

$$S[y(n); n] = \theta W[y(n)] . \tag{4}$$

The firm indexed by n need not and does not know this contribution function, S, of course, or even its own index value n; in equations (2)–(4) we are stipulating possible factual situations around the market in order to see what happens given the way firms behave. There is no market mechanism whereby buyers can coordinate insistence on that particular value of θ which would maximize their aggregate net benefit.

It is straightforward computation (White 1981, eqq. [8]–[15]) to find any schedule $W(y)$ which can sustain itself under the pressures and through the choices of these actors on the two sides of the market. The result is

$$W(y) = (Ay^{(bc+ad)/b} + k)^{b/(b+d)} , \tag{5}$$

subject to two auxiliary sufficiency conditions. (The constant coefficient A is specified below in eq. [11].) Each producer must by its choice of volume satisfy equation (4), and this determines $y(n)$ in terms of the $W(y)$ of equation (5) (as specified below in the dissection of aggregate supply and demand). But every producer's own goal is to choose that volume which maximizes cash flow; this fact yields the two auxiliary conditions.

First, the shape of the schedule in equation (5) guarantees only that each producer firm can find a distinctive niche that yields it an extremum of cash flow; so, to be realistic, the shape must satisfy a second-order condition for maximization, which reduces (White 1981, eqq. [16]–[18]) to

$$d\left(\frac{c-a}{b+d}\right) Ay^{(bc+ad)/b} > \frac{da}{b} k , \tag{6}$$

for a given volume y. Second, a producer firm will not remain in the market unless its optimal niche (at which cash flow, $W - C$, is maximized) in fact yields a positive cash flow to put toward overhead costs and profits. This second inequality can be reduced to a form parallel to the first one in (6):

$$Ay^{(bc+ad)/b} > -k . \tag{7}$$

The presence of an arbitrary constant k, not fixed in terms of other parameters and rates in the system, is the first of two key features of the formula (5) for the market equilibrium schedule. We see that the two auxiliary conditions (6) and (7) can be taken as possibly limiting the volumes (the y values) and thus the underlying fixed attributes (the n's) of firms seeking niches in this market. Or, in some parameter settings, there may be no volumes and thus no quality attributes, n values, and thus no firms that can yield a market equilibrium with some values of k. Qualitatively, k can be seen to set the location of the curve described by $W(y)$, positive k leading

to higher payoff for given volume to the delighted eyes of producers. Historical idiosyncrasy is one way to think of the formation of k—as the almost accidental by-product of a number of producers jockeying for volume and payment sustainable in the presence of the other producers' offerings. It is also clear that a firm can, by cutting its asking price for the same volume, achieve—if it has any motive to—a lowering of the schedule, in effect a change of value of k in the viable equilibrium schedule. (A firm which tries to raise its charge for a given volume above the established terms-of-trade schedule holding at that time may simply find itself with no sales at all: sharp discipline indeed [see n. 4 above].)

The second outstanding aspect of the form for equilibrium market schedule $W(y)$ in equation (5) is the *mixed nature of its exponent* as a (shifted) power function of volume y. The power function form follows, of course, from our use of families of power functions to describe the facts both of cost schedules and of valuation contributions (eqq. [1]–[3]). The interesting question is how this overall exponent, of y neglecting k, namely,

$$\text{exponent} = \frac{b}{b+d}\,c + \frac{d}{b+d}\,a\,, \tag{8}$$

represents a balance between trade-offs on the underlying dimensions of volume and of quality in the costs and valuations. The two trade-offs of interest are (as mentioned above) the ratio a/c for volume dependencies ("contribution"/"cost") and the ratio b/d for quality variations ("desirability"/"expense").

This view of industrial markets insists on and points up the social component which is heavily interlaced with technical and engineering facts and with buyer needs. Instead of a unique equilibration of supply and demand, or devious schemings by speculators in psychology and taste, we see a historically shaped structure of roles among a stable set of producer firms or, if you prefer, an adaptable ecology of niches among a crowd of competing organisms. In either metaphor the actors are making effective decisions on the basis of tangible observations of the actions of their confreres. The market is a public feedback mechanism for trading off divergences among firms and between them and buyers.

A Topology of Markets, and Their Vulnerabilities

We can project on a two-dimensional screen, figure 3, the mutual distancing of sorts of markets from one another, since the preceding section shows that the two ratios a/c and b/d suffice to identify quantitative axes of differentiation. But to identify market results and failures across regions on this screen accurately we must be explicit about how the set of # firms in a market coalesce into stable market aggregations. There is an additional

feedback loop implicit in eqs. (4) and (5), one vulnerable to disruption in the terms of inequalities (6) and (7).

Figure 3 has no left half, since variations with volume (a and c) must both be positive. Figure 3 has both a top half, with $d > 0$, and a bottom half, with d negative. Positive d occurs when correlation of production cost with quality across firms is negative, and conversely for d negative. In the bottom half, new notation must be introduced to avoid confusion:

$$\text{for} \quad d < 0: \quad \delta = -d\,. \tag{9}$$

In markets identified by points in the bottom half of figure 3, the producer whose goods are liked better by buyers is laying out more in costs of production for a given volume of goods. As one might expect intuitively, there proves to be a richer array of market types—and ones sustainable across a wider range of contexts—than is true of markets from the upper half of figure 3.

"Decreasing returns to scale" characterize markets located to the left of the line in figure 3 for $a/c = 1$ (or just $c < 1$ in many texts). "Increasing returns to scale" are supposed, in economic theory (Ijiri and Simon 1977,

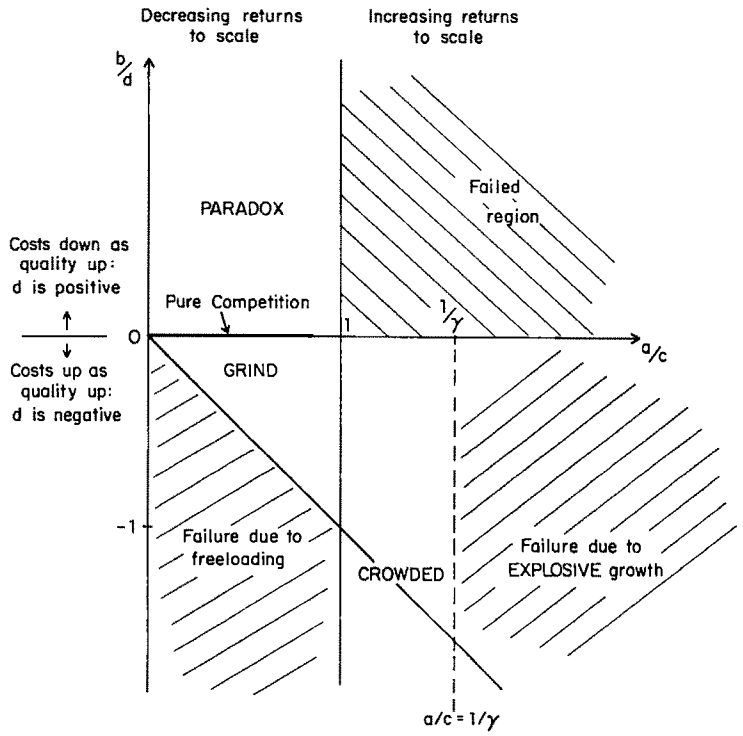


FIG. 3.—Trade-offs in cost versus valuation, across growth in quality (ordinate) and in volume (abscissa).

p. 7), to be inconsistent with sustainable markets. But in my view of market structure, such markets are viable as long as the exponent d is negative, so that cost is higher for higher-quality firms.

Three sorts of market failures can be distinguished in this model.

The only factual contexts absolutely inhospitable to markets, as self-supporting social structures, are those identified with the upper right in figure 3: increasing returns to scale *and* quality correlate *negatively* ($d > 0$ in my convention, eq. [1]) with cost of production. In such contexts, there is nowhere to "shift" the trial market schedule, that is, no value of the constant k in equation (5) for $W(y)$ which will permit satisfying both constraints (6) and (7). Any positive k yields a schedule which permits distinctive niches only for producers who are masochists attempting to minimize their cash flows. Any negative value of k so depresses the terms of trade to producers that no firms are able to achieve positive cash flows.[5]

All other factual contexts, all other parts of figure 3, should at first sight permit at least some equilibrium schedules. There is usually *some* value of k which satisfies both constraints (6) and (7).[6]

However, these otherwise viable ranges of market schedules may under some circumstances encounter one of two forms of instability. First, "freeloading" can unravel a market schedule which at first sight is viable, given firms with a whole range of qualities. In formal terms, "freeloading" results from the wrong kind of overlap between volume regions allowed by one and by the other of the inequality constraints (6) and (7). Return to the point in the PARADOX region; for any positive k value, a range of y exists which does not yield optimal niches for individual firms yet does yield positive cash flows. Then it is easy to show that all firms with quality indices n in the appropriate range would choose some "edge" value of volume as their ("corner") optimum among possible choices; yet they would not yield value for money to buyers: the ratio S/W would be less than θ (see eq. [4]). Thus that part of the schedule over volume, $W(y)$, will not be sustained; but the same problem will reemerge (with a somewhat different set of firms, from a shifted range on the quality index n) for the abbreviated schedule, and in the end it will unravel completely. For any value of k, positive or negative, the lower-left trapezoidal region in figure 3, a region of contexts which yield proper markets by existing market theory (Spence 1976), is

[5] Innumerable specific scenarios could show how a particular set of firms and buyers describable by a point in the upper right would blow up any trial schedule. Parsimony requires stating, as in the text, just the allowed structures and the static criteria which discriminate them; these are given in more detail in White (1976, 1981). But examples of scenarios may help the reader's intuition: see n. 4 above and the case studies below.

[6] In White (1981, table A), I report, for each detailed subregion of the trade-offs plane, the exact range of k values which always yield stable payment schedules. This range can be larger if not all possible levels of quality n are present in the market; so there is interaction between range of k and range of n (worked out in detail in White [1979], pp. 54–57).

vulnerable to freeloading. That is, if firms of all possible quality levels are in the wings seeking possible entrance to that market, for no one of those contexts can any schedule at all (any k value) yield a sustainable market.[7]

Second, there are constraints on viability because of possible explosive feedback effects in the process by which aggregate market size builds. Feedback in aggregation can be schematized as

$$W(y) \rightarrow y(n) \begin{cases} \nearrow W[y(n)] \\ \searrow V(\#,\ y(n)'s) \end{cases} \begin{cases} \searrow \\ \nearrow \end{cases} \theta \rightarrow W(y)\ . \tag{10}$$

The ratio θ ties a market schedule faced by individual firms to the aggregate size of market. This is seen from the specification of the constant A in formula (5) for $W(y)$:

$$A = \frac{(b-\delta)c}{bc-a\delta}\, q\left(\frac{\theta}{r}\right)^{\delta/b}\ . \tag{11}$$

This is a classic feedback indeterminacy in which the production volumes depend on the height of the payment schedule, which to yield equilibrium must depend just so on the actual production volumes chosen by all the # firms. The arbitrary shift constant k, as well as the ratio θ as shown above, enter the determination of equilibrium volumes $y(n)$ and payments $W[y(n)]$, so that in general a numerical solution is required. The special case of $k = 0$ yields guidance in the form of explicit formulae for aggregate sizes, which yield market coefficients θ and A. These formulae (in White 1981) show that the aggregate cash volume of the market, namely, W, is unbounded when

$$c < a\gamma\ . \tag{12}$$

so $a/c = 1/\gamma$ is marked on figure 3 by a dotted line (since it applies only for $k = 0$) and the region to the right is marked EXPLOSIVE.[8] (In the upper half of the figure, for $d > 0$, unbounded growth of aggregate volume begins at $a/c = 1$, the traditional demarcation of "increasing returns to scale" remarked on above.)

In the next part of the paper, numerical examples reporting aggregate

[7] To put it positively, and in more conventional terms, a market can be sustained at a point in this region only if there are high barriers to entry (assuming the initial set of firms set a self-sustainable schedule in motion). Such "metastable" markets, in this lower-left region of the factual trade-offs plotted in fig. 3, should be sought; testing presumes not only measures of a, b, c, and d but also following a putative example over time for evidence of barriers. Barriers can be of many kinds: legal, capital (sunk cost of facilities, establishing distribution, etc.) (Scherer 1970; Porter 1980).

[8] In words: demand is such relative to costs that each producer can keep raising his choice of volume (and perhaps also new firms enter) and yet find that the market sustains the price he envisioned. My comparative statics model cannot, of course, capture this unbounded escalation as a process, or the bounding effects which must set in. New products—whether color TV in 1965 or hula hoops in the late 1950s—are plausible cases.

sizes and market distributions will illustrate how viable markets in one region of figure 3 differ from those of another. One final general distinction should be drawn here. The lower region marked CROWDED, just short of the EXPLOSIVE region, has "increasing returns to scale." Such markets are viable because buyers have no means to realize or organize to exploit the realization that they are better served the *fewer* the firms there are.[9] Under these conditions, the aggregate size of the market, W, actually decreases when a new firm enters and finds a niche on the market schedule.

APPLICATIONS

I report three classes of applications. First, I illustrate in detail each of the three viable market regions with a model of a current U.S. industrial market.[10] Second, I locate a wide variety of empirical markets within my topology. Finally, I illustrate the concentration implications of my model, via prediction of Gini coefficients.

Three specific markets—those for cement, light aircraft, and disposable diapers—provide my illustrations of the three viable market regions.

1. Cement. Empirical sources for this and the other two illustrations are given in table 3 below. Eight firms dominate the current U.S. market for cement considered as a national one: $\# = 8$. Their names are attached in figure 4 to the cost schedules which I assign to each. (The corresponding valuation schedules, the S's of my model, are not shown.) Firms with higher cost schedules also have higher value schedules, so that d is negative. My best estimate is that the cement market is described by $a = .8$, $b = 1$, $c = 1$, $d = -2$ ($\delta \equiv 2$), and $\gamma = .7$ (with r and q each set to unity). This puts the cement market in the middle of the triangle in figure 3 just below the pure-competition line segment. At the top of figure 4 is one viable equilibrium market schedule (heavy line). It has $k = 0$, which in this GRIND region is the lowest value of k not vulnerable to freeloading.[11]

[9] When in fact buyers are highly or totally concentrated—e.g., in purchase of automobile windshield wipers—and the ranges of trade-offs for this region apply, one would expect it to be impossible to form a market (for further discussion see White [1976, 1978]).

[10] Cost and value functions, and index values for n, are chosen to give the best appearing fit for available data, within the constraints of the functional forms assumed earlier. These are illustrations rather than fully developed implementations from and tests of the $W(y)$ model. Full implementation would require tests of goodness of fit of the power functions forms to cost and other schedules. Full implementation is likely also to require introducing stochastic "fuzz" terms into all the schedules. Grossman (1975) has sketched a Bayesian implementation of another kind of rational expectations theory of markets, in which he emphasizes stochastic terms (but leaves producers undifferentiated); in sec. 4 Grossman specifies one simplified estimation framework for Cobb-Douglas forms like eqq. (1) and (2) above.

[11] Given the range of quality values n which I report for them, none of this set of eight firms would be in the temptation region for freeloading. But who knows what obscure local producers might be watching from the wings?

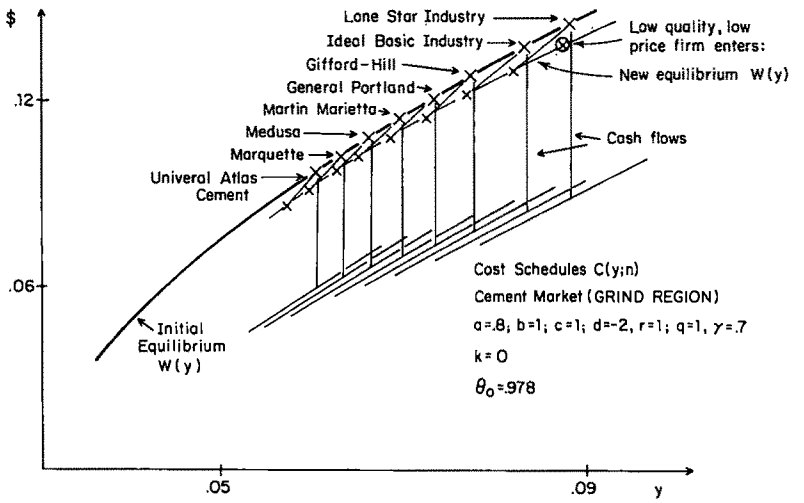


FIG. 4.—Market schedule before and after entry of an additional firm into the cement market.

Arbitrarily, I have used that value of θ which merely breaks even for the buyer side.[12]

Just below the heavy line in figure 4 is a second market schedule, the one which could result if a brand new firm entered at low quality and high volume, as shown (this is a hypothetical, unnamed firm). The changes suit common sense: each other firm loses some volume, as well as by definition some market share, and loses it in sales as well as in physical volume—yet the aggregate size of the market, including the hypothetical ninth firm, is increased. As a test of understanding, the reader should establish what happens to the prices charged by the various existing firms (see last figure in White [1981]).

The companion figure 5, for the same industry and the same initial equilibrium schedule, shows what the new viable market schedule could become if general cost level q for cement producers went up by 20%. In addition, a similar comparative-statics shift of schedule shows what would happen if consumer demand r rose by 10%. (In each case the new schedule remains at the break-even θ value for the new context and k remains zero.)[13]

[12] This is the θ_0 or $t = 1$ case used for calibration in White (1981), eqq. (22)–(27). A rationale is that cement buyers are unlikely to be greatly interested in doing optimally well on their purchases, just on getting enough of the right stuff.

[13] In these illustrations, firms were selected and quality index values (the n's) were assigned so that an orderly terms of trade schedule would result (production volumes or market shares of revenue were the only given data, in addition to discussions of the industries from which a and c were derived). In the absence of complete data, these unfalsifiable selective processes are unavoidable. Once the firms and quality index values have been fixed on the basis of available data, however, predicted effects of "exogenous" changes (embodied in r and q) can provide an opportunity for falsification of the theory.

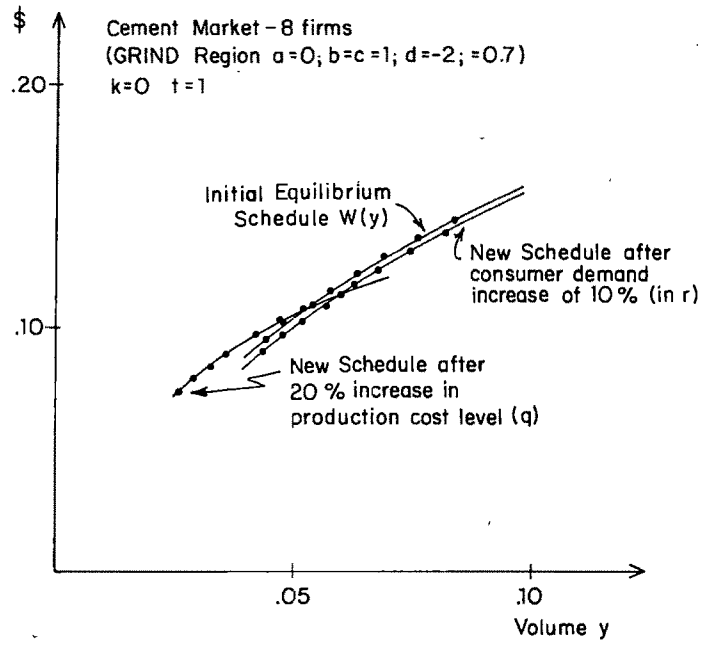


FIG. 5.—Impact on market schedule of exogenous shifts in cost level (q) and demand level (r).

In this market from the GRIND region (in sharp contrast to some shown below) the shift in consumer demand makes very little difference, either in aggregate or in divergences among firms. Each of the eight firms is shown as a dot on each market schedule, so that one sees not only the shift in schedule perceived but also the net shift, after feedback, in volume chosen by a given firm from the shifted schedule of perceived offers. Note that the *increase* in demand, via the feedback, *decreases* each firm's volume and price.

2. Light aircraft. The second detailed illustration is more counterintuitive as a market. I choose a location in figure 3 in the CROWDED region; the actual industry I shall use as an approximation is the manufacturing industry for light aircraft used for recreational flying. There are but three firms currently in this market (Cessna, Piper, and Beech), the minimal number for the theory to make sense but also a very convenient number for exhibiting findings.

Table 1 reports four different changes of equilibrium market schedule, and for each of two values of the shift constant k (in each case at the break-even θ for that context). Because there are but three firms, I can report all these schedules in one table of the numerical values. Cash flow (W less cost) and price are included as well as the schedules $W(y)$.

There is a change in interpretation which I introduce just for this example.

In all equilibrium schedules in table 1, the physical volumes of production for the three firms are kept fixed, and fixed at the levels actually observed in the light-aircraft market in 1969. So a changed schedule is found by computing the changed *quality* index n for each firm needed to yield each the same volume as before. But it is just the quality index n that is to be thought of as an attribute of the producer which is fixed at least in the near term. Hence table 1 should be thought of as the search for the set of con-

TABLE 1

MARKET SCHEDULES AND QUALITY LEVELS WHICH YIELD OBSERVED PRODUCTION VOLUMES: LIGHT-AIRCRAFT INDUSTRY, VARIOUS VALUES OF SHIFT CONSTANT k AND OF COST PLUS DEMAND LEVELS

$k=.5$					$k=1$				
y	n	W	Proportion Cash Flow*	Price	y	n	W	Proportion Cash Flow*	Price
Context: $q=1$, $r=1$, $\gamma=.7$									
1	.613	.4403	.145	.440	1	.487	.3874	.388	.387
1.6	.538	.6633	.364	.414	1.6	.388	.5265	.584	.329
1.9	.503	.7576	.442	.399	1.9	.350	.5777	.645	.304
		$\theta_0=1.63$					$\theta_0=1.54$		
Context: $q=1.2$, $r=1$, $\gamma=.7$									
1	.508	.3771	.315	.377	1	.372	.3069	.549	.307
1.6	.446	.5800	.501	.362	1.6	.296	.4377	.708	.274
1.9	.417	.6684	.566	.352	1.9	.268	.4889	.755	.257
		$\theta_0=1.64$					$\theta_0=1.68$		
Context: $q=1$, $r=1.2$, $\gamma=.7$									
1	.612	.4389	.146	.439	1	.489	.3895	.386	.389
1.6	.537	.6614	.365	.413	1.6	.390	.5286	.582	.330
1.9	.502	.7557	.443	.398	1.9	.352	.5795	.643	.305
		$\theta_0=1.96$					$\theta_0=1.84$		
Context: $q=1$, $r=1$, $\gamma=.9$									
1	.926	.7827	(−.095)	.783	1	.466	.3666	.408	.366
1.6	.812	1.061	.095	.663	1.6	.371	.5043	.602	.315
1.9	.759	1.163	.172	.612	1.9	.335	.5556	.662	.292
		$\theta_0=1.08$					$\theta_0=1.61$		
Context: $q=1$, $r=1$, $\gamma=.7$ (New Firm Added, Third in Size)									
1	.552	.3726	.181	.373	1	.399	.2979	.466	.298
1.3	.518	.4775	.306	.367	1.3	.355	.3677	.578	.282
1.6	.485	.5739	.404	.359	1.6	.318	.4273	.655	.267
1.9	.453	.6618	.482	.348	1.9	.287	.4783	.712	.252
		$\theta_0=1.81$					$\theta_0=1.58$		

NOTE.—Throughout, $a=1=b$, $c=.8$, $d=-2$. θ_0 is the break-even value for θ, with the given k value (see White [1981], eqq. [20]–[25] for details).

* $\{W[y(n)]\}-\{C[y(n);n]\}/W[y(n)]$.

stants and parameter values which yield the best fit to the currently observed market; predictions of changes in volume would be made only later.

In the CROWDED region of figure 3, market schedules can become established only if they are shifted upward and have a positive value of the shift constant k. Regardless of other shifts in constants and parameters, for larger k value, producers of lower quality are sufficient to obtain the observed volume levels y. These volume levels are associated with Beech (smallest, $y = 1$), Cessna ($y = 1.6$) and Piper ($y = 1.9$) in ascending order of size. At the higher k value these producers also generate lower prices (and therefore sales) in all contexts, but cash flows always are a sharply higher proportion of sales.

Table 1 shows also that these market outcomes are quite insensitive, after the feedback effects have played themselves out, to changes in level r of demand. But outcomes are extremely sensitive to changes in the "saturation" rate, the parameter γ. Gamma closer to 1 (less saturation) may lead, for a CROWDED region market like this, to much higher sales volumes; but cash flows to the producers are sharply reduced in spite of higher prices.[14]

3. Diapers. A third market is modeled on the disposable-diaper industry, a very large and quite new one which also now has but three producers, in part because of the huge capital costs of the equipment needed to make diapers on an efficient scale (400 per minute). This industry clearly belongs in the paradoxical category favored by Chamberlain, which in my framework is the upper-left region in figure 3: the better-liked diaper actually has smaller variable costs per unit and thus higher-volume production than the less liked brands. Figure 6 reports one equilibrium market schedule for this case (plus cost schedules), parallel to figure 4 for the cement industry of eight firms. If, analogous to figure 5, we drew a changed equilibrium schedule (still at $k = 0$ and at break-even θ) after an increase of 20% occurred in the level of production costs (given by q), this schedule would be shifted downward uniformly by just under 25%. A parallel demand increase (r up by 20% and a corresponding new break-even value of $\theta_0 = 1.497$ at $k = 0$) leads, after feedback, to a magnified and quite uniform increase of nearly 40% in sales; the latter result is unlike that in figure 5 for the GRIND region market, which was insensitive to r.

As a parallel to table 1 for the aircraft industry in the CROWDED region of figure 3, I report in table 2 also the changes in quality which must be attributed to the three producers in order to maintain their volumes y at the relative levels observed in the market, for various changes in assumptions about cost levels and so on. In contrast to the CROWDED market of

[14] Yet with γ closer to unity, in order to match the observed volumes, one must assume extremely high-quality producers (high values for the three firms on the n index). However, this same saturation value γ can lead, when k is large, to lowered sales and higher profit.

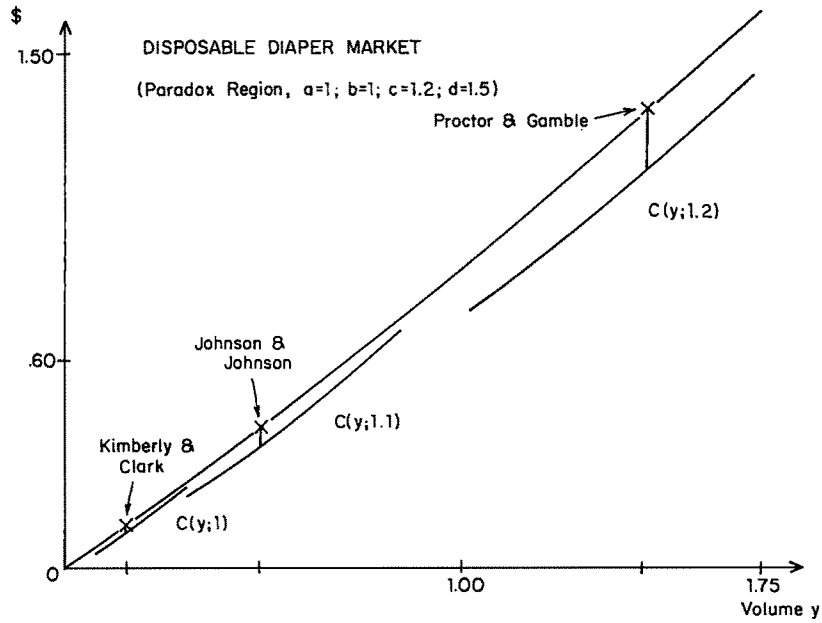


FIG. 6.—Production volume decisions in the disposable-diaper market, at equilibrium. Equilibrium schedule $W(y)$: ($k = 0$; $\gamma = 0.7$; $\theta_0 = 1.313$).

table 1, here the increase in cost level requires that higher quality be attributed to each producer in order to predict the same volumes y, and the prices and sales volumes actually are increased as a result, as is the cash flow. Again in contrast to table 1, but as in figure 5 for a GRIND market, there is very little sensitivity to changes in the demand level r.

Locating U.S. Industrial Markets in the Trade-Offs Plane

In figure 7 (which repeats the topography of fig. 3)[15] an abbreviated name indicates the estimated location in the trade-offs plane of each of a score of current domestic markets. Included are the three (cement, airplanes, diapers) just analyzed. The primary source used is a business school case study series with acronym ICCH.[16] Table 3 gives ICCH identification codes as well as other sources.

[15] No dotted line at $a/c = 1/\gamma$ is shown because γ (crudely: a measure of aggregate elasticity of demand) may be different for each industry. My impressions are that, of the 20 cases in fig. 7, on careful measurement only color TV sets 1965, optical fiber, Minicomputer 1980, and injectors might prove to have a/c beyond the corresponding value of $1/\gamma$ (i.e., in the EXPLOSIVE region of fig. 3); and only the first two might have values of γ greater than unity.

[16] The ICCH series used are but a small fraction of all Intercollegiate Case Clearing House case studies (Graduate School of Business, Harvard University): these are either deliberate studies of a whole industrial market or studies of individual firms which can be combined to yield good coverage of a given market by my definition.

Placement of an industry in figure 7 is an exercise in coding, that is, in qualitative judgment on cost structures and on valuations.[17] Occasionally, as for the farm equipment industry, systematic numerical data is given for the quantities of concern so that numerical estimates of parameters can be made.[18] Other locations are close to guesses, such as those shown for mainframe computers and minicomputers.[19]

TABLE 2

MARKET SCHEDULES AND QUALITY LEVELS WHICH YIELD THE PRODUCTION VOLUMES (y) OBSERVED IN THE DISPOSABLE-DIAPER INDUSTRY UNDER VARIOUS CONTEXTS, WITH SHIFT CONSTANT $k = -40$

y	n	W	Proportion Cash Flow	Price
Context: $q=1$, $r=1$, $\gamma=.7$				
30	3.33	10.38	.058	.35
100	3.67	39.75	.098	.40
296	4.00	128.5	.100	.43
		$\theta_0=9.2$		
Context: $q=1.2$, $r=1$, $\gamma=.7$				
30	3.61	11.07	.219	.37
100	3.98	42.11	.249	.42
296	4.34	136.1	.250	.45
		$\theta_0=9.44$		
Context: $q=1$, $r=1.2$, $\gamma=.7$				
30	3.32	10.38	.058	.35
100	3.66	39.73	.098	.40
296	4.00	128.4	.100	.43
		$\theta_0=11.05$		
Context: $q=1$, $r=1$, $\gamma=.9$				
30	1.80	27.19	.094	.91
100	1.98	100.2	.100	1.00
296	2.16	323.6	.100	1.09
		$\theta_0=1.975$		

NOTE.—Throughout $a = 1 = b$; $c = 1.2$; $d = 1.5$.

[17] Six cases were coded independently by Eric Leifer. In each of these the codings agreed on region. In the absence of systematic numerical data, however, exact agreement on location is virtually impossible.

[18] I.e., the farm equipment case study quotes the proportionate percentage decrease of cost with increase in percentage of (notional) production capacity which is utilized. Depending on the particular component reported, the exponent c lies between 0.7 and 0.9. (See ICCH-9-280-080 [Rev. 2/80], table 5, p. 6.)

[19] The three illustrative cases presented above range from quite exact coding using numerical data (disposable diapers) to more qualitative estimates extrapolated across firms (cement).

TABLE 3

SOURCES AND NOTES FOR TWENTY INDUSTRIES

Name (in Fig. 7) and ICCH Identification*		Notes and Other Sources
Drilling mud:		
9-380-167	...	Market in vibrators to cleanse pollutants from barite
9-380-168	...	
Vacuum tubes:		
1-379-181	Rev. 8/79	Classic declining industry
1-379-184	Rev. 8/79	
Watches:		
6-373-080	Rev. 10/72	...
1-374-050	Rev. 10/75	
1-374-051	Rev. 10/75	
9-373-090	Rev. 9/76	
Baby foods 1965:		
1-379-178	Rev. 8/79	Unusually clearcut case of PARADOX
1-379-185	Rev. 8/79	
Log houses:		
1-378-195	...	Firms which rationalize customized houses
1-379-(196–201)	...	
3-378-193	...	
Circuit breakers:		
9-513-152 (M230)	Rev. 10/75	...
9-513-151 (M229)	Rev. 10/75	
9-565-004	Rev. 1/79	
9-567-005 (AM-P204)	...	
9-578-205	Rev. 10/78	
Disposable diapers:		
9-380-175	...	Smith Barney Harris Upham Research Reports 1979: various dates†
Oil tankers:		
9-379-086	...	See text.
Distribution electrical components:		
5-379-146	...	Service firms that stock tens of thousands of types of parts
1-377-063	...	
5-379-146	...	
3-778-153	...	
9-377-041	Rev. 1/79	
9-380-084	...	
9-377-055	...	
Strode's cables:		
5-377-028	...	Market in metal-ceramic cable connectors, includes Strode division of EG&G, Inc.
9-376-188	Rev. 11/77	
9-377-027	...	
Cement 1970s	...	*Business Week* 1980
Sugar refining 1895	...	Eichner 1969, chaps. 2, 3
Injectors plus electronic fuel injection (EFI):		
9-378-219	...	Injectors are a component of EFI, and a separate market; cases emphasize role structure in these markets
1-378-257	...	
Light aircraft:		
9-369-007 (BP 934R)	...	Low-cost, recreational plane market
9-369-008	Rev. 1970	
9-370-036	...	

* Studies available for a fee from Intercollegiate Case Clearing House, Soldiers Field Road, Boston, Mass. 02163. (Each may run from five to 25 pages of single-spaced pica typescript. Most of these industry studies use real names of firms; some, and most ICCH cases of individual firms, alter data to conceal identity and/or protect confidentiality.)

† Available in business school libraries, unbound.

TABLE 3—*Continued*

Name (in Fig. 7) and ICCH Identification*		Notes and Other Sources
Mainframe computer 1980	...	Corey and Star 1971, pp. 108–56; *Harvard Business Review* 1980; *Fortune* 1980
Minicomputer 1980	...	Hayes 1980
Nickel	...	Salter and Weinhold 1979, chap. 10
Tractors 1970s:		
4-578-083	Rev. 11/77	
9-280-080	Rev. 2/80	
9-171-368 (BC 349)	...	
9-313-123 (BP 866)	Rev. 7/69	...
9-313-154 (PB 867R)	...	
9-377-704	...	
9-574-858	...	
Color TV:		
1-380-180	...	
1-380-181	...	...
1-380-191	...	
Fiber optics:		
1-379-136	Rev. 1/80	Will explode into a cluster of huge new markets: in my terms, γ exceeds unity
1-379-139	...	
1-380-117	...	

Some ICCH case study series contradict the specifications (the "givens") demanded by my model and so do not appear, although those industries might be suitable if studied at another date.[20] Another, on the oil tanker industry, can be rephrased to meet the model specifications.[21]

The firms which participate in many of these markets do so typically as divisions. A separate division, which is an independent profit center and

[20] One such is the series on turbine generators after 1963. This is an industry with but two members, a pure duopoly which is not a market in my sense. Prior to 1963, however, it was a market in my sense, one in which several other U.S. and foreign producers joined the two in eyeing one another for the same business. This earlier form I discuss elsewhere (White 1981) on the basis of Sultan's two-volume account (1974, 1975). The late nineteenth-century market in sugar refining, ably described by Eichner (1969), also ended as a trust, but with many members rather than two. I would code it as having been in the GRIND region in 1885: some product differentiation (according to impurity level and associated taste) but less than the variation in cost among the score of manufacturers around the United States ($b < \delta$). Over the next decade, greatly expanded factories yielded striking economies of scale ($c < 1$). The set of producer firms belongs in the CROWDED region for 1895 as shown in fig. 7. Then the disappearance of product differentiation set the stage for market instability and the Havemeyers' rounding up of all firms into a trust.

[21] If the volume in this market (the decision variable, y) is number of tanker trips supplied by a charterer, the market is *not* a production market but a truck-and-barter market which should be referred (with its erratic prices and speculative features) to the pure theory of exchange (Newman 1965). But one can reorient to see y as the speed at which given tankers are sent on a journey and thus the volume of oil delivered per time period. Figure 7 contains a point for this market, for which the case study material yields quite definite estimates ($c = 2$, $a < 1$, $d < 0$, $\delta \gg b$, $\gamma < 1$). (The existence of such a market as a separate entity is, of course, an idealization, given its heavy dependence on terms of trade and supplies in the spot market for tankers as well as on the charter market.)

relatively autonomous, is the decision maker eyeing the other producers.[22] For example, the ceramic-metal connector market labeled Strode's Cable in figure 7 is that participated in by an independent division of the large firm called EG&G (a division acquired earlier by incorporating an old-line firm named Strode).

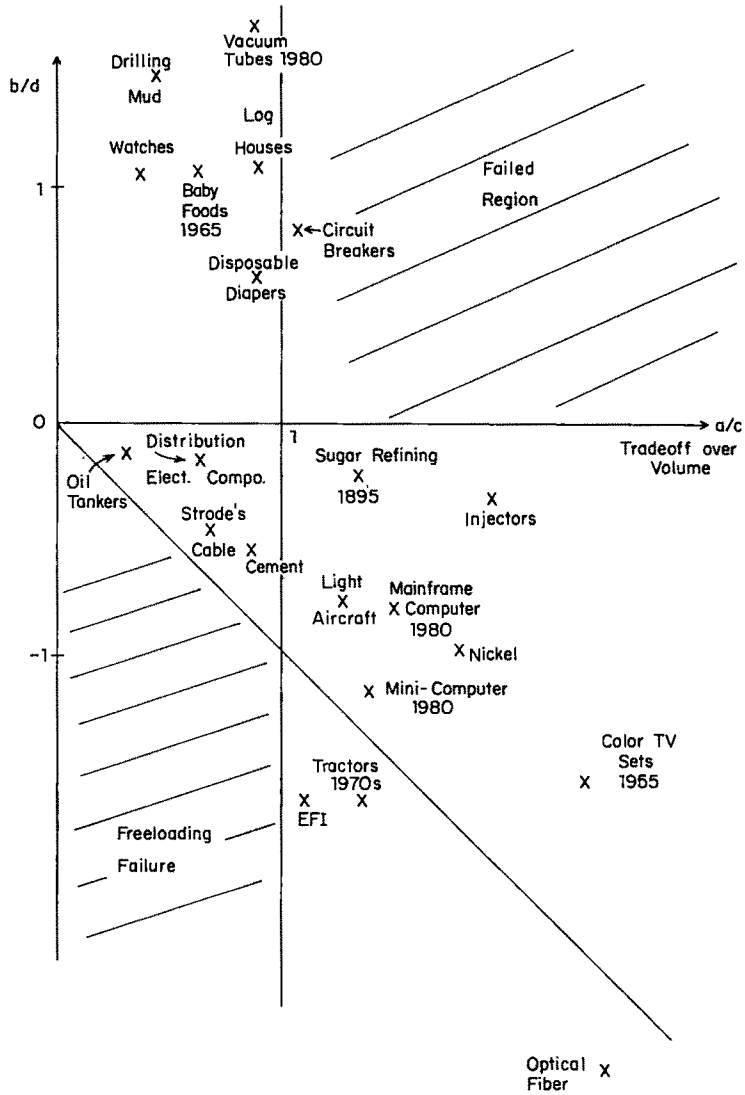


FIG. 7.—Locations of markets on the trade-offs plane (sources in table 3)

[22] This fact could add obvious cross-market complications, but these may be financial (capital, and long-range costs) and not related to production. My model should be seen as a preconglomerate model.

The ICCH reports on the baby-food manufacturing industry yield a clear-cut and well documented example in the PARADOX region, to contrast with the diapers market discussed above. In the late 1920s Gerber created and then led an industry for canned baby foods as decisively as IBM did mainframe computers. Gerber, too, has had precursors and early competitors such as Mead Johnson's pabulum of 1915 and Clapp's canned broth, as well as large competitors which perished without a trace (e.g., Libby, McNeill, Birds Eye).

Mothers were convinced enough of Gerber's quality that it could charge price premiums of over 10% while spending nearly 10% less on total advertising and promotion. In 1965 Gerber had more modern and efficient production facilities than its main competitors—Beech-Nut, Swift, Heinz—which in turn were more efficient than the smaller brand-name competitors, often regional, and the numerous small private-label competitors. Yet economies of scale were not significant, in part because there were so many food varieties (over 100) at a given time as well as a high rate of change of varieties, about one-third per decade.

The large variety of items also tended to limit concentration by buyers on one company's products since other companies offered different varieties that might catch taste. For complex reasons (willingness to give open discounts by volume, or discounts off invoice, or rebates, etc.), supermarkets tended to give Gerber less shelf space than its market share. And contrary to Gerber's and the other companies' predictions, the birthrate had been declining since the late 1950s, and almost all mothers already used the commercial baby foods, so that in 1965 the overall elasticity γ was well under unity.

The constituent estimates underlying the Baby Food 1965 location in figure 7 are $a = 0.6$, $c = 1$, $b = 1$, and $d = 0.8$.[23] These differ substantially from those for the diapers market given above in table 2. The resulting difference in location within the same region of the trade-offs plane leads to substantial differences predicted for other aspects of the two markets (see the next subsection and the Appendix for equations).

Inequality in Market Shares

Since producers watch producers, they are an interrelated set and in the pure case are a clique of mutually aware firms. It follows that there will not be very many firms in a market; probably there will be a dozen or two. Each will tend to be distinct in role and to have a distinct place on the quality index n. It follows that the firm which by the existing schedule $W(y)$ chooses the largest volume will itself have a substantial share of the market,

[23] Furthermore, estimation of the observed terms-of-trade schedule $W(y)$ indicates a large, negative value of the shift constant k.

as will the next few. One hazards a guess that the smallest firm to which the biggest will lend an eye will be within a factor of 10 in size: thus it follows from my theory that a market is arrayed from a leader with a fifth or more of the total sales down to the smallest with 1% or so of the total. But how can we assess these facts in comparison with other sorts of inequality, or understand them in terms of the trade-off ratios which frame figure 7?

I choose this topic of market shares for illustrating my analysis of markets because it seems to me at once one of the most important and one of the most vexed aspects of the present social science understanding of production markets (Scherer 1970; Porter 1980). Observed markets almost entirely escape the computational grasp of existing microeconomic theory (Mansfield 1975; Cohen and Cyert 1975), although by a variety of devices the theory and whatever is observed are declared to be in harmony. Law, in particular antitrust law, seems to shape the concepts more than does microeconomic theory (Williamson [1975]; for a good survey see the last ICCH study cited for circuit breakers in table 3, and for documentation of antitrust policy see Salter and Weinhold [1979], pp. 289–305). The share of total market held by the top four (or 3 or 5 or 8 . . .) producer firms is given prima facie standing as evidence for degree of collusion in price fixing and so on. In contrast, I think assessment of market share distribution should (i) be made on the basis of an explicit theory of market formation and (ii) be assessed in a more general comparative framework for the study of social inequality. This second objective requires looking at firms as social actors; it also calls for a measure of inequality widely used in social science, such as the Gini index.

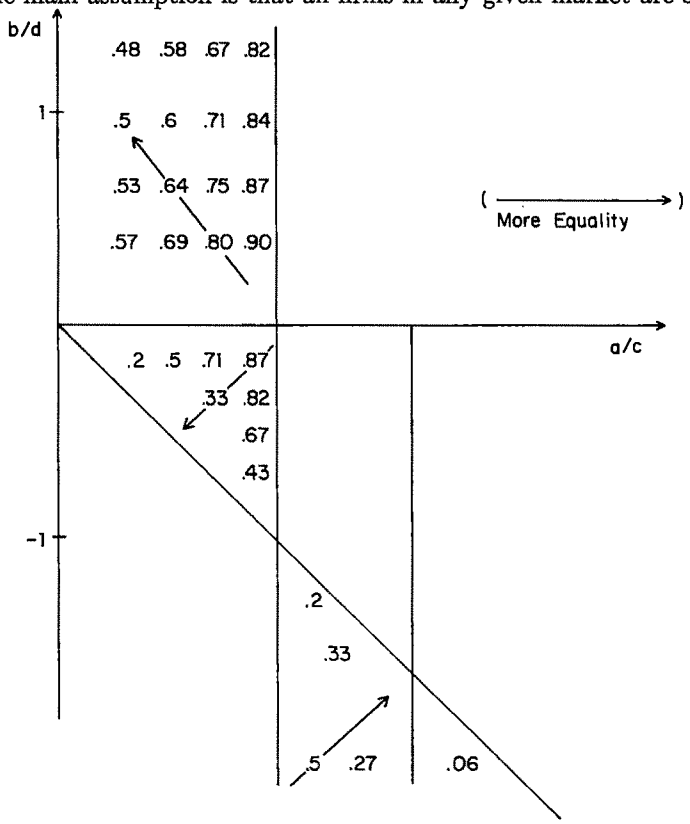
Various degrees of inequality among producers in their market shares yield quite different sorts of roles for firms, as well as different overall atmosphere. For example, does the largest firm overwhelm the others with its presence, or does the traditional English industrial flavor prevail, with firms not varying greatly in size "sharing up" the market in genteel fashion? (Dispersion in profit rates, etc., also are important, but they are influenced in part by facts on fixed investments and the like, which my model ignores.) To achieve a simple formula, I shall further restrict myself to market schedules with $k = 0$.[24] In this special case the inequality in cash flow equals the inequality in market volumes, to which I now turn.

The Gini index is familiar as a measure of inequality or dispersion in personal incomes or wealth: it is derived from Lorenz distributions for cumulative percentage of total income versus cumulative percentage of population (see Schwartz and Winship 1980). It is suitable as well for market volumes (see Granovetter [in press] for general theoretical discussion).

[24] They are known to be stable in all subregions of fig. 3 except the upper portion of CROWDED; i.e., except for $1/\gamma > a/c > 1$, and $a/c > b/\delta$.

This index, call it G, varies between zero (for equality) and one (for total inequality). As calibration: for a uniform distribution over income the Gini is 0.33; typical national income distributions have $G \approx 0.4$, and national wealth distributions may have $G \approx 0.8$.

Figure 8 shows how the value of G predicted for sales volumes of a market tends to change according to location in the trade-offs plane. Its limitation is that the qualities and number of firms must be kept the same, for this purpose; so the various predictions cannot be checked individually against values computed across an assembly of observed markets such as figure 7.[25] The main assumption is that all firms in any given market are spaced



*From eq (14): uniform distribution of firms across all n (0<n<∞), and with k=0.

FIG. 8.—Gini index scores for inequality in sales among producers (from eq. [14]: uniform distribution of firms across all n [$0 < n < \infty$], and with $k = 0$).

[25] In any event there are many practical problems in comparing indices computed from ICCH case studies or from observations in the business press with the projections in fig. 8. Membership in a market as reported may not be that seen by the producers themselves (as my model stipulates): First, various federal definitions (for SIC codes see Shiskin and

uniformly on the quality index n. For mathematical simplicity the lower limit and the upper limit of the range for n are taken to be zero and infinity, respectively, and firms are assumed to be very closely spaced on quality. The general formulae for G under these assumptions (including $k = 0$) are: for physical volume (y),

$$G = \frac{1}{1 + \left|\frac{2(c - a)}{b + d}\right|}; \tag{13}$$

for dollar volume (W),

$$G = \frac{1}{1 + \left|\frac{2(c - a)}{bc + ad}\right|}. \tag{14}$$

Note that markets close to the axis where perfect competition lies ($d \gg b$) tend to have high inequality either on sales or on physical volume. Also, inequality decreases by either equation as one moves away from valuation change with volume being proportional to cost change with volume (i.e., as one moves away from the $a/c = 1$ vertical), whether into GRIND and PARADOX regions on the one side or into the CROWDED region on the other. (The equations do not hold for $|b/d| < a/c$ in the latter, where $k = 0$ is not a stable schedule.)

By inspection of equation (13) one can see that equal physical volumes are approached when the quality trade-off b/d is unity and negative. In the bottom of figure 8 ($d < 0$), when the two trade-off ratios are equal, but opposite in sign, equality in sales volumes among producers is approached. But it is precisely along this line that market structure disintegrates just because it relies on divergences between cost trade-off and value trade-off to cue distinctive niches for firms.

CONCLUSIONS

Markets are tangible cliques of producers observing each other. Pressure from the buyer side creates a mirror in which producers see themselves,

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Peterson [1972]) may yield no statistical "industry" or "product" at all close; yet business journals usually report data in terms of government definitions. Second, the numerical levels of Gini indices for market sales came from a stylized power-law framework for cost and valuation; they do not make accountants' discriminations. Third, most reports of business performance use the legal corporation as unit, but typical *Fortune* 500 firms can have dozens of divisions, which as mentioned above, are commonly the effective actors in the actual markets I model. Reinforcing biases will tend to make predicted values higher than observed values. Eq. (14) assumes an infinite range and uniform distribution for quality n and so should tend to exaggerate inequality; it also assumes a schedule described by a shift index k of zero. Empirical estimates usually come from truncated sets of firms with the smallest omitted and so will tend to underestimate inequality. I see no reason why either of these reinforcing biases should be correlated with the size of G, so the rank order of predicted indices should tend to be the same as that for observed indices.

not consumers. Heterogeneous producers with their differentiated products may find and maintain stable roles or niches. Self-interested optimizing by each of them can sustain a global market schedule $W(y)$, but it is exposed to three sorts of market failure. Basic limits for equilibrium configurations turn out to depend on just two trade-off ratios which summarize the facts of costs and tastes. Thus markets are shaped by trade-offs between dispersions, not by averages as suggested by the cliché that supply equals demand.

APPENDIX

Changes in Cost Level and Product Popularity

Two different kinds of sensitivity of the market as a whole to an exogenous change (see fig. 5), such as an increase in q or r, should be distinguished. The first is the effect on the market schedule $W(y)$ of figure 1. This is a change in height of the price schedule, a scale change in a graph such as figure 4. The second kind is the resultant change, including feedback effects, on the actual volume of sales by each firm and by the market as a whole. The latter, overall resultant is easier to compute: when the shift constant $k = 0$ (White 1981, eqq. [25] and [26]), all regions yield the same proportionality for the aggregate volume of the market W, namely,

$$W \sim (r^c/q^a)^{\gamma/(c-a\gamma)} . \tag{A1}$$

It befits the nature of market formation, as a balancing of trade-offs in dispersions (see fig. 2), that the market volume varies with valuation scale factor r to the exponent c of cost variation with volume; and, similarly, dependence of W on costs scale q is through q raised to the power a of volume's contribution to valuation. But note that market sales total goes down as cost scale goes up. It is also obvious that the sensitivity of market size to either scale factor increases as the demand-saturation exponent rises—as γ becomes closer to c/a. Even more obvious is the fact that this net feedback resultant of change in costs or valuation scales on market sales in aggregate does not depend at all on the trade-off b/d on the quality dimension! These findings generalize the numerical results presented above for three illustrative markets.

But visible changes in the market schedule $W(y)$, the price schedule perceived by producers, take quite a different form, and this form itself differs according to region in the trade-offs plane of figure 3. Rather than quote the formulae (from White 1981, eqq. [28]–[30]), I have illustrated in the three detailed examples how dramatic the change in form is. And there the changed locations of each individual firm are indicated on the changed schedule.

Price Structure

Implicit in the divergence (in text above, eqq. [13] and [14]) between the two Gini indices, for sales volume and for physical volume, is variation of price with volume. The last figure in White (1981) reports the predicted variation in price per unit according to total volumes from various producers in that market, separately for subregions of the trade-offs plane. (Only monotonicity and existence of extrema are specified; so the predicted curve holds throughout the given subregion regardless of the particular values on quality index. The simplicity of these shapes follows from my approximation of cost and valuation schedules by families of power functions.) At various locations within one region of the trade-offs plane, the CROWDED region, one can find price-by-volume curves rising monotonically, or falling monotonically, or concave downward, or concave upward! This plasticity underlines my contention that prices are a secondary and ex post phenomenon.

Consider a numerical price schedule from one of the earlier illustrations, for the light-aircraft industry (with $q = 1 = r$, $\gamma = 0.7$ and take $\theta = 1.63$, see table 1). In the schedule with shift constant $k = 0.5$, the price-volume pairs corresponding to Beech, Cessna, and Piper, respectively, are (where p_n is price per unit)

p_n	.440	.414	.399
y_n	1.0	1.6	1.9
n	.613	.538	.503 .

The n values were chosen to yield the observed ratios of sales volumes among light-aircraft manufacturers: higher values mean (as before) higher quality, so that Beech is the Cadillac and Piper the Chevy.

It is difficult to obtain reliable information on effective prices and physical volumes sold for all of the firms in a given market. It is not sufficient to obtain posted price schedules or price books, since these often represent a basis for negotiation more than a reliable report of practices. In any event my model presupposes aggregation of all sales by the given firm into one aggregate volume, and aggregation of the corresponding revenue across a dispersion of concrete sales prices in different batches to different customers.

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The Economics of Organization: The Transaction Cost Approach[1]

Oliver E. Williamson
University of Pennsylvania

The transaction cost approach to the study of economic organization regards the transaction as the basic unit of analysis and holds that an understanding of transaction cost economizing is central to the study of organizations. Applications of this approach require that transactions be dimensionalized and that alternative governance structures be described. Economizing is accomplished by assigning transactions to governance structures in a discriminating way. The approach applies both to the determination of efficient boundaries, as between firms and markets, and to the organization of internal transactions, including the design of employment relations. The approach is compared and contrasted with selected parts of the organization theory literature.

The proposition that the firm is a production function to which a profit-maximization objective has been assigned has been less illuminating for organization theory purposes than for economics. Even within economics, however, there is a growing realization that the neoclassical theory of the firm is self-limiting. A variety of economic approaches to the study of organization have recently been proposed in which the importance of internal organization is acknowledged.[2] The one described here emphasizes

[1] This paper has benefited from a number of discussions I have had with William G. Ouchi, including those we had at a Mini-Conference on Strategy, Marketing, and Organization (held at the Graduate School of Management, UCLA, during April 1980 under the auspices of Booz, Allen, & Hamilton) and at the recent Conference on the Economics of Organization (held in Berlin in June 1980 under the auspices of the International Institute of Management). It has also benefited from a year-long dialogue on these matters that Ouchi and I have had with Paul Kaestle and William Allen. The paper also benefited greatly from remarks on an earlier version by Banri Asanuma and on a later revision by Herbert Simon. The assistance of *AJS* reviewers in reshaping the manuscript is also appreciated. Requests for reprints should be sent to Oliver E. Williamson, Department of Economics, University of Pennsylvania, Philadelphia, Pennsylvania 19104.

[2] These include the neoclassical theory of the firm—which, however is relatively sparse in its organizational implications—managerial discretion theory (Baumol 1959; Marris 1964; Williamson 1964), team theory (Marschak and Radner 1972), agency theory (Alchian and Demsetz 1972; Jensen and Meckling 1976), and the transaction cost approach (Coase [1937] 1952; Williamson 1975). Although I was aware, when I was

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transaction costs and efforts to economize thereon. More than most economic approaches, it makes allowance for what Frank Knight (1965, p. 270) has felicitously referred to as "human nature as we know it."[3]

Economic approaches to the study of organization, transaction cost analysis included, generally focus on efficiency. To be sure, not every interesting organizational issue can be usefully addressed, except perhaps in a minor way, in efficiency terms. A surprisingly large number can, however, especially if transaction cost aspects are emphasized. This is accomplished by making the transaction—rather than commodities—the basic unit of analysis and by assessing governance structures, of which firms and markets are the leading alternatives, in terms of their capacities to economize on transaction costs.

The transaction cost approach to the study of organizations has been applied at three levels of analysis. The first is the overall structure of the enterprise. This takes the scope of the enterprise as given and asks how the operating parts should be related one to another. Unitary, holding company, and multidivisional forms come under scrutiny when these issues are addressed.[4] The second or middle level focuses on the operating parts and asks which activities should be performed within the firm, which outside it, and why. This can be thought of as developing the criteria for and defining the "efficient boundaries"[5] of an operating unit. The third level of analysis is concerned with the manner in which human assets are organized. The object here is to match internal governance structures with the attributes of work groups in a discriminating way.

Only issues of the two latter kinds are addressed in this paper.[6] The study of both of these issues turns critically on the dimensionalizing of transactions. The antecedent literature from which the transaction cost approach derives is sketched in Section I. The rudiments of the approach, including the dimensionalizing of transactions, are then set out in Section II. Applications to the study of efficient boundaries are developed in Section III. Employment relation issues are addressed in Section IV. Com-

working on *Markets and Hierarchies,* that it had a number of applications outside economics, the book was directed at an economics audience. I was therefore gratified when organization theory specialists recognized merit in the approach. I am especially indebted to William Ouchi for bringing the book to the attention of the organization theory audience (see Ouchi 1977).

[3] Knight's remarks about the human attributes of economic agents have been widely disregarded and attention has been focused narrowly on the risk-bearing aspects of Knight's classic work.

[4] I have discussed these issues at length elsewhere (see Williamson 1970, chaps. 2, 3, and 7; 1975, chaps. 8–9).

[5] The term "efficient boundaries" is borrowed from Ouchi (1980*a*).

[6] For a discussion of the issues that arise at the first level, see the references in n. 4.

parisons with selected aspects of the organization theory literature and contrasts with "power" approaches to the study of organizations are made in Section V. Concluding remarks follow.

I. ANTECEDENTS

The transaction cost approach to the study of organizations relates to three relatively independent literatures. To be sure, there is considerable overlapping among them and they have not proceeded heedless of one another. The extent to which they deal with common issues, however, is rarely recognized.

Considering that economizing is central to the transaction cost approach, it is not surprising that an economics literature is among the antecedents. Also, inasmuch as internal organizational issues are featured, the organization theory literature makes an expected appearance. The third literature is less obvious: this is the contract law literature in which contract is addressed as a governance issue.

Each of these literatures is large, and my summary of the intellectual progression in each is necessarily brief and omits important contributions. The 1930s witnessed significant advances in all three areas. My sketch of the antecedents begins there.

The proposition that the transaction is the basic unit of economic analysis was advanced by John R. Commons in 1934. He recognized that there were a variety of governance structures with which to mediate the exchange of goods or services between technologically separable entities. Assessing the capacities of different structures to harmonize relations between parties and recognizing that new structures arose in the service of these harmonizing purposes were central to the study of institutional economics as he conceived it.

Ronald Coase posed the problem more sharply in his classic 1937 paper, "The Nature of the Firm." He, like others, observed that the production of final goods and services involved a succession of early stage processing and assembly activities. But whereas others took the boundary of the firm as a parameter and examined the efficacy with which markets mediated exchange in intermediate and final goods markets, Coase held that the boundary of the firm was a decision variable for which an economic assessment was needed. What is it that determines when a firm decides to integrate and when instead it relies on the market?

Friedrich Hayek's 1945 article, "The Use of Knowledge in Society," shed further insight. He observed that the economic problem is relatively uninteresting except when economic events are changing and sequential adaptations to these changes are needed. What distinguishes a high per-

formance economy is its capacity to adapt efficiently to uncertainty. Although he did not state the issues in transaction-cost-economizing terms, such terms are implicit in much of the argument.

The postwar market failure literature helped better to define some of the "failures" with markets that common ownership (the firm) served to overcome. It was not until 1969, however, that the underlying difficulties with markets were unambiguously traced to transaction cost origins. As Kenneth Arrow put it: "Market failure is not absolute; it is better to consider a broader category, that of transaction costs, which in general impede and in particular cases completely block the formation of markets" (1969, p. 48).

The appearance of Chester Barnard's book *The Functions of the Executive* in 1938 and of Herbert Simon's explication of the Barnard thesis in *Administrative Behavior* in 1947 are widely recognized as significant events in the organization theory field. Purposive organization was emphasized, but the limits of human actors in bounded rationality respects and the importance of informal organization were prominently featured.

This stream of research was further developed by the "Carnegie School" (March and Simon 1958; Cyert and March 1963). Hierarchical organization and associated controls are traced to the limited capacities of human actors to cope with the complexity and uncertainty with which they are confronted. The organization is essentially viewed as a "problem-facing and problem-solving" entity (Thompson 1967, p. 9). But organizational efforts are often myopic, and demands for control can and often do give rise to dysfunctional outcomes.

Although Alfred Chandler's remarkable book, *Strategy and Structure* (1962), had its origins in business history rather than organization theory, in many respects this historical account of the origins, diffusion, nature, and importance of the multidivisional form of organization ran ahead of contemporary economic and organization theory. The mistaken notion that economic efficiency was substantially independent of internal organizational structure was no longer tenable after this book appeared.

James Thompson built on all of the foregoing in fashioning his classic statement of the organizational problem in 1967. Both uncertainty and bounded rationality were featured. Moreover, implicitly, and sometimes explicitly[7] attention was fixed on efforts to economize on transaction costs. Core technologies, domains (or boundaries) of organized action, and the powers and limits of market and hierarchical modes are all recognized.

The legal literature to which I refer is concerned with contracting—especially the distinction between "hard contracting" (or black-letter law)

[7] For example, Thompson's proposition that "under norms of rationality, organizations group positions to minimize coordination costs" (1967, pp. 64–65) is in this spirit.

and "soft contracting" in which the contract serves mainly as framework. Karl Llewellyn's 1931 essay addressed these issues. He observed that transactions come in a variety of forms and that a highly legalistic approach can sometimes get in the way of the parties instead of contributing to their purposes. This is especially true where continuity of the exchange relation between the parties is highly valued.

Others who adopted and refined this theme include Steward Macaulay (1963), Lon Fuller (1964), Clyde Summers (1969), David Feller (1973), and Ian Macneil (1974). As Macneil puts it, the discrete transaction—"sharp in by clear agreement; sharp out by clear performance" (1974, p. 738)—is very rare in both law and economics, and we deceive ourselves by treating it otherwise. What he refers to as "relational" forms of contracting—which may involve arbitration, collective bargaining, and other types of obligational market exchange—are becoming more important and need to be recognized.

A deepening awareness of transaction cost issues marks the progression of each of the literatures. Among other things, by the early 1970s it was becoming clear that the study of organizations was a comparative institutional undertaking in which alternative governance structures—both within and between firms and markets—required explicit attention. Inasmuch, moreover, as the transactions of interest were not all of a kind, differences among them would evidently have to be recognized. What were the distinguishing attributes? Finally, although transaction cost economizing is an important and greatly neglected topic, such economizing cannot proceed regardless of the production cost ramifications. Put differently, transaction cost economizing needs to be located within a larger economizing framework and the relevant trade-offs need to be recognized.

II. SOME RUDIMENTS

A transaction occurs when a good or service is transferred across a technologically separable interface. One stage of activity terminates and another begins. With a well-working interface, as with a well-working machine, these transfers occur smoothly. In mechanical systems we look for frictions: do the gears mesh, are the parts lubricated, is there needless slippage or other loss of energy? The economic counterpart of friction is transaction cost: do the parties to the exchange operate harmoniously, or are there frequent misunderstandings and conflicts that lead to delays, breakdowns, and other malfunctions? Transaction cost analysis supplants the usual preoccupation with technology and steady-state production (or distribution) expenses with an examination of the comparative costs of

planning, adapting, and monitoring task completion under alternative governance structures.

Some transactions are simple and easy to mediate. Others are difficult and require a good deal more attention. Can we identify the factors that permit transactions to be classified as one kind or another? Can we identify the alternative governance structures within which transactions can be organized? And can we match governance structures with transactions in a discriminating (transaction-cost-economizing) way? These are the neglected issues with which organizational design needs to come to grips. These are the issues for which transaction cost analysis promises to offer new insights.

Behavioral Assumptions

It is widely recognized—by economists, lawyers, and others who have an interest in contracting—that complex contracts are costly to write and enforce. There is a tendency, however, to accept this fact as given rather than inquire into the reasons for it. As a result, some of the consequences of and remedies for costly contracting are less well understood than would otherwise be the case.

What is needed, I submit, is more self-conscious attention to "human nature as we know it." The two behavioral assumptions on which transaction cost analysis relies that both add realism and distinguish this approach from neoclassical economics are (1) the recognition that human agents are subject to bounded rationality and (2) the assumption that at least some agents are given to opportunism.

Bounded rationality needs to be distinguished from both hyperrationality and irrationality (Simon 1978). Unlike "economic man," to whom hyperrationality is often attributed, "organization man" is endowed with less powerful analytical and data-processing apparatus. Such limited competence does not, however, imply irrationality. Instead, although boundedly rational agents experience limits in formulating and solving complex problems and in processing (receiving, storing, retrieving, transmitting) information (Simon 1957), they otherwise remain "intendedly rational."

But for bounded rationality, all economic exchange could be efficiently organized by contract. (The economic theory of comprehensive contracting for unboundedly rational agents has been elegantly worked out.[8]) Given bounded rationality, however, it is impossible to deal with complexity in

[8] The comprehensive contracting model is widely referred to as the Arrow-Debreu model. For a discussion and an interesting contribution to this literature, see Radner (1968).

all contractually relevant respects. As a consequence, incomplete contracting is the best that can be achieved.

Ubiquitous, albeit incomplete, contracting would nevertheless be feasible if human agents were not given to opportunism. Thus, if agents, though boundedly rational, were fully trustworthy, comprehensive contracting would still be feasible (and presumably would be observed). Principals would simply extract promises from agents that they would behave in the manner of steward when unanticipated events occurred, while agents would reciprocally ask principals to behave in good faith. Such devices will not work, however, if some economic actors (either principals or agents) are dishonest (or, more generally, disguise attributes or preferences, distort data, obfuscate issues, and otherwise confuse transactions), and it is very costly to distinguish opportunistic from nonopportunistic types ex ante.

A different way of putting this is to say that while organizational man is computationally less competent than economic man, he is motivationally more complex. Thus, whereas economic man engages in simple self-interest seeking,[9] opportunism makes provision for self-interest seeking with guile. Problems of contracting are greatly complicated by economic agents who make "false or empty, that is, self-disbelieved threats or promises" (Goffman 1969, p. 105), cut corners for undisclosed personal advantage, cover up tracks, and the like.

That economic agents are simultaneously subject to bounded rationality and (at least some) are given to opportunism does not by itself, however, vitiate autonomous trading. On the contrary, when effective ex ante and ex post competition can both be presumed,[10] autonomous contracting will be efficacious. Of these two, effective ex ante competition is a much easier condition to satisfy: it merely requires that there be large numbers of qualified bidders at the outset. The subsequent transformation of an exchange relation involving large numbers to one involving small numbers during contract execution is what causes problems. Whether ex post competition is equally efficacious or breaks down as a result of contract execution depends on the characteristics of the transactions in question, which brings us to the matter of dimensionalizing.

[9] As Peter Diamond has put it, standard "economic models . . . [treat] individuals as playing a game with fixed rules which they obey. They do not buy more than they can pay for, they do not embezzle funds, and they do not rob banks" (1971, p. 31). Only recently has this standard presumption come under scrutiny, often by making allowance for what insurance specialists refer to as "moral hazard," which is a particular form of opportunism.

[10] Although large numbers of qualified bidders are frequently on a parity at the outset, winning a bid and executing a contract often introduces a disparity between the qualifications of winners and those of nonwinners, with the result that bidding competition involving large numbers is not equally effective at the contract renewal interval. For a discussion, see Williamson (1971; 1975, pp. 27–36; 1979*b*); and Klein, Crawford, and Alchian (1978).

Dimensionalizing

As set out elsewhere (Williamson 1979*b*), the critical dimensions for describing transactions are (1) uncertainty, (2) the frequency with which transactions recur, and (3) the degree to which durable, transaction-specific investments are required to realize least cost supply. Only recurrent transactions are of interest for the purposes of this paper;[11] hence attention will hereafter be focused on uncertainty and asset specificity, especially the latter.

Asset specificity is both the most important dimension for describing transactions and the most neglected attribute in prior studies of organization. The issue is less whether there are large fixed investments, though this is important, than whether such investments are specialized to a particular transaction. Items that are unspecialized among users pose few hazards, since buyers in these circumstances can easily turn to alternative sources and suppliers can sell output intended for one buyer to other buyers without difficulty. Nonmarketability problems arise when the specific identity of the parties has important cost-bearing consequences. Transactions of this kind may be referred to as idiosyncratic.[12]

Asset specificity can arise in any of three ways: site specificity, as when successive stations are located in cheek-by-jowl relation to each other so as to economize on inventory and transportation expenses; physical asset specificity, as where specialized dies are required to produce a component; and human asset specificity that arises from learning by doing. The reason asset specificity is critical is that, once an investment has been made, buyer and seller are effectively operating in a bilateral (or at least quasi-bilateral) exchange relation for a considerable period thereafter. Inasmuch as the value of specific capital in other uses is, by definition, much smaller than the specialized use for which it has been intended, the supplier is effectively "locked into" the transaction to a significant degree. This is symmetrical, moreover, in that the buyer cannot turn to alternative sources of supply and obtain the item on favorable terms, since the cost of supply from unspecialized capital is presumably great.[13] The buyer is thus committed to the transaction as well. Accordingly, where asset specificity is great, buyer and seller will make special efforts to design an exchange that has good continuity properties.

The site-specific assets referred to here appear to correspond with those

[11] For a discussion of the organizational consequences of occasional, rather than recurrent, contracting, see Williamson (1979*b*, pp. 246–54). Also see n. 32 below.

[12] For earlier treatments of the economies of idiosyncrasy, see Williamson (1975, pp. 9–10, 27–33, 68–74; 1979*b*, pp. 238–45). Others who are persuaded that idiosyncratic investments are crucial to the understanding of the economics of organization include Klein et al. (1978), Klein (1980), and Teece (1980).

[13] For a somewhat related discussion of symmetry, see Thompson (1967, pp. 32–35).

Thompson describes as the "core technology" (1967, pp. 19–23). Indeed, the common ownership of site-specific stations is thought to be so "natural" that alternative governance structures are rarely considered. In fact, however, the joining of separable stations—for example, blast furnace and rolling mill, thereby to realize thermal economies—under common ownership is not technologically determined but instead reflects transaction-cost-economizing judgments.[14] It will nevertheless be convenient, for the purposes of this paper, to assume that all site-specific stations constitute a technological core the common ownership of which will be taken as given. Attention is thus focused on earlier stage, later stage, and lateral transactions. The efficient governance structure for these turns on physical asset and human asset specificity. Although these are often correlated, it will facilitate the argument to treat them sequentially. Thus, physical asset specificity is emphasized in Section III and human asset specificity is not introduced until Section IV.

III. EFFICIENT BOUNDARIES

The treatment of efficient boundaries in this section deals with only a part, albeit an interesting part, of the full set of organizational issues. Only two organizational alternatives are considered: either a firm makes a component itself or it buys it from an autonomous supplier. Thus mixed modes, such as franchising, joint ventures, etc., are disregarded. I also take the core technology as given and focus on a single line of commerce—say the activities of a particular manufacturing division within a larger industrial enterprise. The object is to describe how the economizing decisions which define the outer boundaries of this division are made.[15]

Schematic Description

Suppose that there are three distinct production stages which, for site-specificity reasons, are all part of the same firm. This is the technological core. Suppose that raw materials are distinct and are naturally procured from the market. Suppose that two things occur at each production stage: there is a physical transformation, and components are joined to the "main frame." And suppose, finally, that the firm has a choice between own distribution and market distribution.

Let the core production stages be represented by $S1$, $S2$, $S3$ and draw these as rectangles. Let raw materials be represented by R and draw this

[14] See Williamson (1971) and McKean (1971) for a discussion of alternative modes and an assessment of transaction cost consequences for site-specific transactions.

[15] The focus is on operating decisions of a firm or market kind. Both strategic decisions and interdivisional asset sharing are ignored.

as a circle. Let component supply be represented by *C1-B, C2-B, C3-B* if the firm buys its components and *C1-O, C2-O, C3-O* if it makes its own components. Draw these as triangles. Let distribution be given by *D-B* if the firm uses market distribution and *D-O* if the firm uses own distribution. Draw these as squares. Finally, let a solid line between units represent an actual transaction and a dashed line a potential transaction, and draw the boundary of the firm as a closed curve that includes those activities that the firm does for itself.

The closed curve that defines the efficient boundary of the firm in figure 1 includes, in addition to the technical core, component *C2* and the distribution stage, *D*. Components *C1* and *C3* and raw materials are procured in the market. Obviously this is arbitrary and merely illustrative. It also oversimplifies greatly. It is relatively easy, however, to elaborate the schema to add to the core, to consider additional components, to include several raw material stages and consider backward integration into these, to break down distribution, etc. But the central points would remain unchanged, namely: (1) the common ownership of some stations—the core—is sufficiently obvious that a careful, comparative assessment is unneeded (site specificity will often characterize these transactions); (2) there is a second set of transactions in which own supply is manifestly uneconomic, hence market supply is indicated (many raw materials are of this kind); but (3) there is a third set of activities for which make-or-buy decisions can only be made after assessing the transformation and transaction cost consequences of alternative modes. The efficient boundary is the inclusive set of core plus additional stages for which own supply can be shown to be the efficient choice.

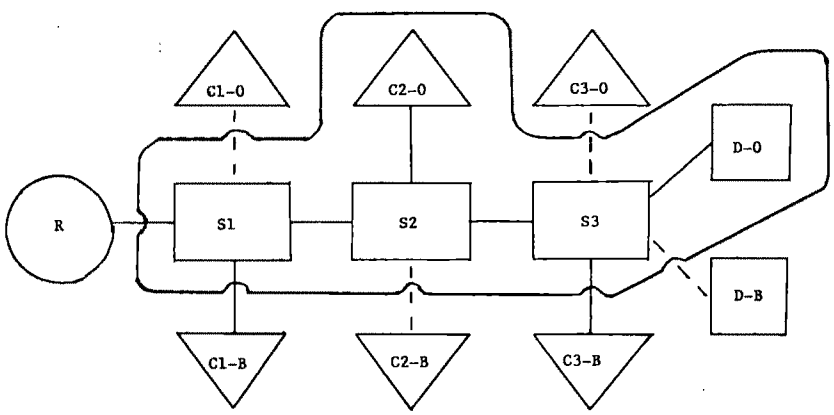


FIG. 1.—Efficient boundary

A Simple Model

The crucial issue is how the choice between firm and market governance structures for decisions related to point 3 above are made. Transaction cost reasoning is central to this analysis, but trade-offs between production cost economies (in which the market may be presumed to enjoy certain advantages) and governance cost economies (in which the advantages may shift to internal organization) need to be recognized.

The issues here are somewhat involved and are set out more fully and formally elsewhere.[16] The central points are these: (1) physical asset specificity is never valued by itself but only because demand is thereby increased in design or performance respects;[17] (2) such valued demand consequences are often realized only at greater production expense (standardized items would be cheaper, often because scale economies could be more fully exhausted); whence (3) the optimal choice of asset specificity requires that demand and production cost consequences be taken into account simultaneously; and (4) governance costs also vary with asset specificity, and these also have to be introduced into the calculus.

The choice between firm and market organization arises in this last connection. If assets are nonspecific, markets enjoy advantages in both production cost and governance cost respects: static scale economies can be more fully exhausted by buying instead of making; markets can also aggregate uncorrelated demands, thereby realizing risk-pooling benefits; and external procurement avoids many of the hazards to which internal procurement is subject.[18] As assets become more specific, however, the aggregation benefits of markets in the first two respects are reduced and exchange takes on a progressively stronger bilateral character. The governance costs of markets escalate as a result and internal procurement supplants external supply for this reason.[19] Thus, the governance of recurrent transactions for which uncertainty is held constant (in intermediate degree) will vary as follows: classical market contracting will be efficacious

[16] The simple model sketched out here is developed more fully in Williamson (1981).

[17] Site specificity, in contrast, involves transportation and inventory cost savings, albeit by complicating the problem of mediating the exchange interface.

[18] For a discussion of bureaucratic hazards, see Thompson (1967, pp. 152–54) and Williamson (1975, pp. 117–31).

[19] Actually, the nature of the asset specificity matters. If the assets in question are mobile and the specificity is due to physical but not human asset features, market procurement may still be feasible. It can be accomplished by having the buyer own the specific assets (e.g., dies). He puts the business up for bid and awards it to the low bidder, to whom he ships the dies. Should contractual difficulties arise, however, he is not locked into a bilateral exchange. He reclaims the dies and reopens the bidding. This option is not available if the specific assets are of a human asset kind or if they are nonmobile. This "refinement" of transaction cost reasoning illustrates how the approach can and should be developed and its predictive power sharpened and tested.

whenever assets are nonspecific to the trading parties; bilateral or obligational market contracting will appear as assets become semispecific; and internal organization will displace markets as assets take on a highly specific character.

The advantages of firms over markets in harmonizing bilateral exchange are three. First, common ownership reduces the incentives to suboptimize. Second, and related, internal organization is able to invoke fiat to resolve differences, whereas costly adjudication is needed when an impasse develops between autonomous traders. Third, internal organization has easier and more complete access to the relevant information when dispute settling is needed. The incentive to shift bilateral transactions from markets to firms increases as uncertainty is greater, since the costs of harmonizing the interface vary directly with the need to adjust to changing circumstances.

At the risk of oversimplification,[20] the essence of the foregoing argument can be shown graphically by expressing both production cost differences and governance cost differences as functions of asset specificity (A). Thus let $\Delta C = f(A)$ be the production cost difference between internal organization and the market, $\Delta G = g(A)$ be the corresponding governance cost difference, and assume that these two functions have the shapes and relative locations shown in figure 2. So long as the vertical sum of $\Delta C + \Delta G$ remains positive, market procurement enjoys the advantage. Indifference between governance structures obtains where $\Delta C + \Delta G = 0$, namely, at $\hat{A}$. Internal procurement enjoys the advantage for values of A that exceed $\hat{A}$ (since $\Delta C + \Delta G < 0$ in this region).

Implicitly, this was the apparatus used in making governance structure assignments for the component and distribution stages shown in figure 1. Inasmuch as component $C2$ was taken out of the market and is supplied internally, while components $C1$ and $C3$ remain in the market, components $C1$ and $C3$ are presumably more standardized[21] ($C1$ represents, say, arma-

[20] The main simplification is that ΔC (and possibly ΔG) is also a function of the amount produced. Figure 2 can be thought of as a cross-section for a fixed level of output. Furthermore, the optimal value of A will depend on both demand effects and absolute cost effects. Only cost differences are shown in the figure.

[21] Transaction-specific investments are related to but need to be distinguished from the more familiar notion of standardization. Although many nonstandard goods and services are produced with the assistance of nonstandard (specialized) assets, this need not be the case. When it is not, the production of nonstandard goods or services with assets that involve little specificity poses few contracting problems. Thus, suppose that a glass manufacturer is producing circular lenses for spotlights and supplies them to a large number of spotlight manufacturers. Suppose that one of the spotlight firms decides to add triangular and square spotlights to its line. Such designs will be recognized as nonstandard, but they will pose special problems in contracting for lenses only if the glass manufacturer has to dedicate special assets to the production of the nonstandard lenses. If he can, with slight modification, produce them with existing plant and labor force, the fact of nonstandard design poses no particular economic

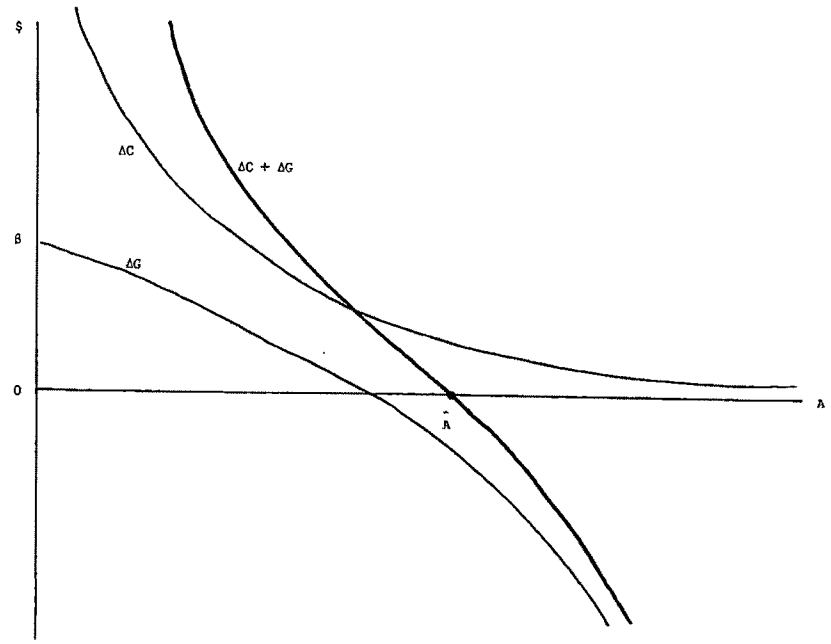


FIG. 2.—Representative net production and governance cost differences

ture wire and $C3$ a transistor) while $C2$ is more specialized (has distinctive chassis styling or performance features). Similarly, the decision to integrate forward into distribution reflects the fact that the product cannot be marketed effectively through standard channels, presumably because specialized human assets are needed to sell and service the product and a bilateral employment relation develops as a consequence. In terms of figure 2, the values of A are low for $C1$ and $C3$ but exceed $\hat{A}$ for both $C2$ and D.

Two Examples

The transaction cost arguments set out above are of a normative kind: what governance structure *should* be chosen. In contrast, the examples developed here describe what has been observed. The critical question is not whether the appropriate governance structure was selected at the

obstacles. If, however, the glass manufacturer must be induced to incur specialized (transaction-specific) investments to produce the triangular or square lenses, a much more complicated contractual situation develops. The parties then have a stake in maintaining a continuing exchange relation (so that the specialized assets can be utilized effectively). Additional governance structure designed to sustain the relation and safeguard it against opportunism is needed.

outset but whether transaction cost factors, possibly manifested as difficulties that resulted from a maladapted structure, are responsible for the eventual configuration.

Automobile body manufacture.—Klein et al. (1978, pp. 308–10) have examined the problems that arose when a bilateral exchange relationship between Fisher Body and General Motors was attempted in the 1920s. The basic facts are these:

1. In 1919 General Motors entered a 10-year contractual agreement with Fisher Body whereby General Motors agreed to purchase substantially all its closed bodies from Fisher.

2. The price for delivery was set on a cost-plus basis and included provisions that General Motors would not be charged more than rival automobile manufacturers. Price disputes were to be settled by compulsory arbitration.

3. The demand for General Motors's production of closed body cars increased substantially above that which had been forecast. As a consequence, General Motors became dissatisfied with the terms under which prices were to be adjusted and urged Fisher to locate its body plants adjacent to GM assembly plants, thereby to realize transportation and inventory economies. Fisher Body resisted.

4. General Motors began acquiring Fisher stock in 1924 and completed a merger agreement in 1926.

Inasmuch as GM cars had distinctive body designs, the production of closed bodies required significant transaction-specific investments to be made. Site-specificity considerations reinforced this need. The transaction, moreover, was evidently beset by substantial demand and cost uncertainties. Since there was little to be gained from market procurement, while the governance costs of market procurement were predictably great, the transaction was one for which internal procurement was indicated. The strains that autonomous contracting experienced could thus have been anticipated, and the eventual reconfiguration from long-term contracting to common ownership is consistent with the basic transaction-cost-economizing argument.

Forward integration.—Chandler (1977) and Porter and Livesay (1971) report that extensive forward integration from manufacturing into distribution occurred in the last 30 years of the 19th century. The reasons for this are several, including the appearance of infrastructure (in the form of the railroad, telephone, and telegraph) and a variety of manufacturing developments. But the response to these developments was anything but uniform. Forward integration included retailing for some commodities (e.g., farm equipment and sewing machines), extended only to wholesaling for others (e.g., tobacco and certain branded items), and was negligible for

still others (e.g., packaged groceries and dry goods). What were the determining factors?

Tracing this differential response is beyond the scope of this paper but is reported elsewhere (Williamson 1980). Very briefly, the pattern appears to be this. Integration into retailing occurred only for commodities that required considerable point-of-sale information, possibly to include demonstration, and follow-on service. Specialized human assets were evidently needed to provide such sales and service. Integration into wholesaling occurred for commodities that were perishable and branded. Forward integration occurred because contracts to turn over inventory and destroy older stocks were neither self-enforcing nor incentive-compatible, hence they placed the manufacturers' reputations at risk. Commodities that had none of these properties were sold through market distribution channels because no special hazards were posed. This progression of forward integration contingent on differential degrees of asset specificity and the differential hazards of opportunism is the principal implication of transaction cost reasoning and appears also to be the main factor explaining the selective degree of forward integration reported by Chandler.[22]

IV. MANAGING HUMAN ASSETS: THE EMPLOYMENT RELATION

It will be convenient, for the purposes of this section, to assume that the transactions in question are site specific, whence internal organization is warranted. Merely to assign a transaction to an internal governance structure does not, however, assure that the efficiency purposes of transaction cost analysis will be realized. It is necessary in addition to examine the human asset characteristics of the internal transactions in question and to fashion the employment relation appropriately.

The same general principles apply to the governance of human assets as apply to the efficient organization of transactions in general. Thus to use a complex structure for governing simple transactions is to incur unneeded costs, while to use a simple structure to govern a complex transaction invites strain. The questions are, How are human asset differences best described, what are the employment relation alternatives, and what is the appropriate correspondence between them?

The discussion is in two parts. The first addresses the organization of human assets at the staff level. The second deals with union organization, which applies primarily at the production level.

[22] Alfred Chandler advises me that he agrees broadly with this interpretation of his results.

Governance, General

Recall that transactions are described in terms of three attributes: frequency, uncertainty, and asset specificity. The assets of interest here involve a continuing supply of services, whence frequency aspects will be suppressed and attention focused on the internal organizational aspects of uncertainty and asset specificity.

It will facilitate the argument to assume that transfers of goods and services across interfaces are not at issue. Internal governance is thus concerned entirely with intrastage activity. Inasmuch as physical assets are nonvolitional, transactions assigned to internal organization pose problems only in conjunction with human asset specificity.[23]

Note in this connection that skill acquisition is a necessary but not a sufficient condition for a human asset governance problem to arise. The nature of the skills also matters; the distinction between transaction-specific and nonspecific human assets is crucial. Thus, physicians, engineers, lawyers, etc., possess valued skills for which they expect to be compensated, but such skills do not by themselves pose a governance issue. Unless these skills are deepened and specialized to a particular employer, neither employer nor employee has a special interest in maintaining a continuing employment relation.[24] The employer can easily hire a substitute and the employee can move to alternative employment without loss of productive value.

Mere deepening of skills through job experience does not by itself pose a problem either. Thus, typing skills may be enhanced by practice, but if they are equally valued by current and potential employers there is no need to devise special protection for an ongoing employment relation. Knowledge of a particular firm's filing system, in contrast, may be highly specific (nontransferable). Continuity of the employment relation in the latter case is a source of added value.

Thus to the neoclassical proposition that the acquisition of valued skills leads to greater compensation, transaction cost reasoning adds the following proposition: skills acquired in a learning-by-doing fashion and imperfectly transferable across employers need to be embedded in a protective governance structure, lest productive values be sacrificed if the employment relation is unwittingly severed. The concern here is with what Knight has referred to as "the internal problems of the corporation, the protection . . . of members and adherents against each other's predatory propensities"

[23] Actually, this assumes away transfer pricing problems, which can be tricky but take us away from our main concerns.

[24] This ignores transitional problems that may be associated with job relocation. All employees experience these, on which account protection against arbitrary dismissal is sought. But the further question is what *additional* safeguards are warranted. This matter turns on human asset specificity.

(1965, p. 254). This poses a problem in the degree to which assets are firm-specific.

The internal organizational counterpart for uncertainty is the ease with which the productivity of human assets can be evaluated. This is essentially the metering problem to which Armen Alchian and Harold Demsetz refer in their treatment of the firm (1972). Their argument is that firms arise when tasks are technologically nonseparable, the standard example being manual freight loading. As they put it (1972, p. 779): "Two men jointly lift cargo into trucks. Solely by observing the total weight loaded per day, it is impossible to determine each person's marginal productivity. . . . The output is yielded by a team, by definition, and it is not a *sum* of separable outputs of each of its members."

When tasks are nonseparable in this sense, individual productivity cannot be assessed by measuring output—an assessment of inputs is needed. Sometimes productivity may be inferred by observing the intensity with which an individual works; this is the aspect emphasized by Alchian and Demsetz. Often, however, the assessment of inputs is much more subtle than effort accounting. Does the employee cooperate in helping to devise and implement complex responses to unanticipated circumstances, or does he attend to his own or local goals at the expense of others? Metering this, except over long observation intervals, can be inordinately difficult.

Human assets can thus be described in terms of (1) the degree to which they are firm-specific and (2) the ease with which productivity can be metered. The fact that Alchian and Demsetz consider only the latter explains the narrow construction of the employment relation in their assessment of economic organization.[25] Both dimensions, however, are critical to an adequate assessment.[26] Letting H_1 and H_2 represent low and high degrees of human asset specificity and M_1 and M_2 represent easy and difficult conditions of meterability, the following four-way classification of internal governance structures is tentatively proposed:

1. H_1, M_1: *internal spot market.*—Human assets that are nonspecific and for which metering is easy are essentially meeting market tests continuously for their jobs. Neither workers nor firms have an efficiency interest in maintaining the association. Workers can move between employers without loss of productivity, and firms can secure replacements without incurring start-up costs. Hence no special governance structure is devised to sustain the relation. Instead, the employment relation is terminated

[25] Alchian and Demsetz treat human assets as fungible. Thus although incumbents may continue to hold jobs for a considerable period of time and may claim to be subject to an "authority relationship," all they are doing is continuously meeting bids for their jobs in the spot market under the Alchian and Demsetz scheme. See Alchian and Demsetz (1972, p. 777) and, for a discussion, Williamson (1975, pp. 66–69).

[26] Alchian evidently agrees. See Klein et al. (1979, p. 322, n. 49).

when either party is sufficiently dissatisfied. An internal spot market labor relation may be said to exist. Examples include migrant farm workers and custodial employees. Professional employees whose skills are nonspecific (certain draftsmen and engineers) also fall into this category.

2. H_1, M_2: *primitive team.*—Although the human assets here are nonspecific, the work cannot be metered easily. This is the team organization to which Alchian and Demsetz refer (1972). Although the membership of such teams can be altered without loss of productivity, compensation cannot easily be determined on an individual basis.[27] The manual freight loading example would appear to qualify. This structure is referred to as a primitive team, to distinguish it from the relational team, described below.

3. H_2, M_1: *obligational market.*—There is a considerable amount of firm-specific learning here, but tasks are easy to meter. Idiosyncratic technological experience (as described, for example, by Doeringer and Piore [1971, pp. 15–16]) and idiosyncratic organizational experience (accounting and data-processing conventions, internalization of other complex rules and procedures, and the like) both qualify. Both firm and workers have an interest in maintaining the continuity of such employment relations. Procedural safeguards will thus be devised to discourage arbitrary dismissal. And nonvested retirement and other benefits will accrue to such workers so as to discourage unwanted quitting (for a discussion, see Mortensen 1978).

4. H_2, M_2: *relational team.*—The human assets here are specific to the firm and very difficult to meter. This appears to correspond with the "clan" form of organization to which William Ouchi (1980*b*) has referred. The firm here will engage in considerable social conditioning, to help assure that employees understand and are dedicated to the purposes of the firm, and employees will be provided with considerable job security, which gives them assurance against exploitation. Neither of these objectives can be realized independently of the other.

Relational teams are very difficult to develop, and it is uncertain how widespread or sustainable they are. It is argued that some of the Japanese corporations are organized in this way (for a discussion, see Lifson 1979), but the interpretation of this is subject to dispute. Certain utopian societies are organized as relational teams, but these have experienced severe continuity problems as the initial membership, which often was highly committed, retired or expired (see Kanter 1972; Manuel and Manuel 1979).

The above described match of internal governance structures with the

[27] This assumes that output is a joint product and that input differences cannot be easily ascertained.

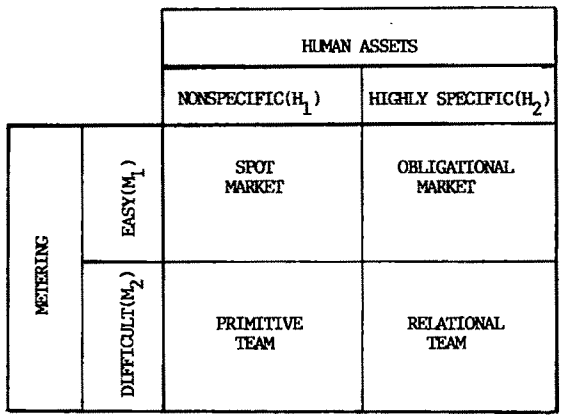


FIG. 3.—The governance of internal organization

internal transactional attributes just described is summarized in figure 3. Admittedly, describing internal transactions in bivariate, binary terms simplifies considerably. The overall framework is nevertheless in place and refinements can be made as needed. (Thus, mixed internal governance structures will presumably arise to service transactions that take on intermediate, rather than extreme, *M* and *H* values.)

Despite its simplicity, the four-way classification is instructive in several respects. For one thing, even this simple four-way classification of the employment relation is useful in breaking down what has previously been subsumed under the broad heading of unified governance. Second, and related, merely to recognize that a recurrent transaction involves high asset specificity and hence is appropriately organized under unified governance is not sufficient to assure that the efficiency purposes of transaction cost analysis will be realized. It is also necessary to recognize that asset specificity breaks down into site, physical, and human asset categories and that these have significantly different internal governance ramifications. Third, differential meterability also matters. The fact that internal transactions dimensionalize along lines similar to those used to describe transactions generally (see Sec. II) reinforces confidence in the underlying transaction cost approach.

Some Remarks on Union Organization

The foregoing discussion of internal governance structures refers mainly to staff rather than production-level employees. Since it is among the latter that union organization appears, the question arises as to whether transaction cost reasoning has useful applications to the study of collective

organization. To the extent that it does, further confidence in the power of the approach is presumably warranted.

The general reasons that collective organization of the work force affords efficiency benefits when the human assets in question are firm-specific in significant degree have been set out elsewhere (Williamson, Wachter, and Harris 1975). Rather than repeat them here, I merely observe that the transaction cost approach to the study of unionization yields testable implications that do not derive from more familiar theories of unionization that rely on power or politics to drive the analysis (Freeman and Medoff 1979). The principal implications are: (1) the incentive to organize production workers within a collective governance structure increases with the degree of human asset specificity; and (2) the degree to which an internal governance structure is elaborated will vary directly with the degree of human asset specificity. Transaction cost analysis thus predicts that unions will arise early in such industries as railroads, where the skills are highly specific, and will arise late in such industries as migrant farm labor, where skills are nonspecific. It further predicts that the governance structure (job ladders, grievance procedures, pay scales) will be more fully elaborated in industries with greater specificity than in those with less (steel vs. autos is an example). The preliminary data appear to support both propositions.[28]

The transaction cost hypothesis does not deny the possibility that unions will appear in settings where human asset specificity is slight. Where this occurs, however, the presumption is that these outcomes are driven more by power than by efficiency considerations. Employers in these circumstances will thus be more inclined to resist unionization; successful efforts to achieve unionization will often require the assistance of the political process; and, since power rather than efficiency is at stake, the resulting governance structure will be relatively primitive.

V. RELATION TO THE ORGANIZATIONAL LITERATURE

As noted at the outset, some of the antecedents and the behavioral assumptions employed in the transaction cost approach have their origins in the organization theory literature. Further connections between transaction cost economics and that literature are sketched. The transaction cost approach is then contrasted with the "power" approach to the study of organizations.

[28] The arguments and the evidence are developed more fully in Scott R. Williamson (1980).

Some Comparisons

The transaction cost approach is usefully compared with the population ecology model, with Thompson's work on organizations, with a recent survey of interorganizational linkages, and with the posterior rationality perspective. Michael Hannan and John Freeman's influential statement of the population ecology model poses the following provocative question: "Why are there so many kinds of organizations?" (1977, p. 936). The transaction cost approach affords a partial answer: there are so many kinds of organizations because transactions differ so greatly and efficiency is realized only if governance structures are tailored to the specific needs of each type of transaction.

Hannan and Freeman also observe that "little attention is paid in the organizations literature to issues concerning the proper units of analysis" (1977, p. 933). They argue, however, that choice of the unit of analysis is important and "involves subtle issues [with] far reaching consequences for research activity" (1977, p. 933). I fully concur and argue that the transaction is usefully made the basic unit of analysis. Among other things, this practice shifts attention away from technology (and technological determinism) and sensitizes analysts to transaction costs and the crucial importance of organizations for economizing on such costs. This brings organization theory to the fore, since choice of an appropriate governance structure is preeminently an organization theory issue.

The population ecology model emphasizes adaptive fitness (Hannan and Freeman 1977; Aldrich 1979). It operates at a relatively high level of abstraction, however, and hence does not offer specific predictions as to which particular organizations will have superior properties in which circumstances. The transaction cost approach has addressed this issue mainly in the context of commercial organizations, in which both product and capital market competition are the sources of natural selection pressures. How broadly it will apply elsewhere remains to be seen. It is nevertheless interesting that public utilities can be studied in this way (Williamson 1976). More generally, any issue that can be posed, directly or indirectly, as a contracting problem can be analyzed to advantage in transaction-cost-economizing terms.[29]

The transaction cost approach has numerous points of contact with Thompson's work. Thus both he and I emphasize that human agents are subject to bounded rationality and that the basic problem with which

[29] An illustration of a problem that I once believed to be outside the scope of transaction cost analysis is the oligopoly issue. Once I had rethought the issue in contracting terms, it became clear that a number of useful statements could be made about the likelihood of successful collusion among oligopolists. See Williamson (1975, chap. 12).

organizations must contend is adapting effectively to uncertainty.[30] Both of us are also interested in the problem of efficient boundaries (what Thompson refers to as the "domain" [1967, p. 26]), and we both contend that economizing on "coordination costs" (Thompson 1967, pp. 57–65) is crucial to the definition of the boundary and to the way in which internal relations are ordered. I also pick up his notion of the "technical core" (Thompson 1967, p. 11) in my discussion of efficient boundaries, and his discussion of power as a reciprocal condition (Thompson 1967, p. 32) is similar to (though in other respects it goes beyond) mine.

Thompson and I differ in that he does not appear to make allowance for trade-offs between production economies and transaction cost economies,[31] while I do. In addition, he does not dimensionalize transactions. Many of his propositions appear to be nontestable for this reason, but at least some of them could be restated to advantage using the dimensionalization of transactions proposed above. His contracting, coopting, and coalescing arguments (Thompson 1967, pp. 35–37), for example, can be expressed in terms of the frequency, uncertainty, and asset specificity properties of the transactions in question. Thus, assume that the transactions in question are recurring and involve an intermediate degree of uncertainty. Then autonomous contracting will be used when assets are nonspecific; obligational contracting (which is akin to co-opting) will be used for assets of an intermediate degree of specificity; and merger (coalescing) occurs if assets, especially human assets, are highly specific. We also differ somewhat in our treatments of collective bargaining. I contend that the governance structure within which collective bargaining operates will be specifically attuned to the nature of the human assets in question. This is not inconsistent with Thompson's discussion (1967, pp. 109–10) but goes beyond it.

It is also of some interest to relate the transaction cost approach to the recent survey of interorganizational linkages by Laumann, Galaskiewicz, and Marsden (1978). Similarities here include their discussion of

[30] See Thompson (1967, pp. 9–13). Thompson's view that "structure is a fundamental vehicle by which organizations achieve bounded rationality" (1967, p. 54) is close in spirit to mine, though I would express it somewhat differently. The manner in which the internal affairs of the firm are decomposed determines whether the organization is able to cope effectively within the bounded rationality limits to which its management is subject.

[31] Thus, Thompson refers repeatedly to minimizing activities without inquiring whether successive minimizing efforts are independent. If they are not, it is not possible simultaneously to "minimize the power of task-environment elements" (Thompson 1967, p. 32) and to "group positions to minimize coordination costs" (Thompson 1967, p. 57). Moreover, the trade-offs between organizing costs and operating costs need to be faced. For any given output, the object is to minimize the *sum* rather than either one.

modes, relationships, and linkages in an open-systems context. They adopt a relatively microanalytic approach to the study of transactions and contend that "interorganizational and intraorganizational transactions [must] be distinguished, which thus implies that the problem of delineating organizational boundaries be faced" (1978, p. 460). This is precisely the issue addressed in Section III, above. They argue further that the "specific form taken by the total network . . . will also be influenced by the context of the relationships . . . as well as by the modality or normative context within which network formation occurs" (1978, p. 461). Expressed in my terms, it does not suffice to assign a transaction to one governance structure (a firm or a market) or another. It is furthermore necessary to attune the exchange relationship to the continuity needs of the parties. When these are minimal, autonomous contracting is both efficient and effective. As the needs for contingent cooperation increase, however, autonomous contracting is supplemented by mandated rules or by mutual efforts (including merger) to discourage aggressive suboptimization (Laumann et al. 1978, p. 468). Within internal organization, moreover, there is a further need to examine the characteristics of the employment relation and to attune it in a discriminating way (see Sec. IV, above).

Whereas Laumann et al. describe network modalities in terms of competitive and cooperative modes, I favor a three-way description in which networks are described as autonomous, cooperative, and strategic. Introduction of this last goes beyond the scope of this paper but makes allowance for "interorganizational relations [that] take on a more perduring nature than that of the narrowly defined instrumentalities of procuring necessary inputs and disposing of products, . . . [but include] seeking unfair advantage and subverting the market mechanism" (Laumann et al. 1978, p. 467). Whether such strategic uses of interorganizational relations are feasible turns on market structure considerations. A transaction cost interpretation of strategic abuses can be developed and has been set out elsewhere (Williamson 1979*a*).

The Laumann et al. discussion of the "resource-dependency" theory is interesting in two respects (1978, p. 470). For one thing, Laumann et al. question whether it is sufficient to focus on dyadic exchange. My answer is that dyadic exchange is very powerful and less delimiting than some suggest (though I concede that triadic or higher-order analysis is sometimes indicated). They also observe that exchange theory has a tendency "to become tautological" and that specific exchanges, once formed, may be resistant to reassignment thereafter. I examine the tautological aspects of exchange theory in the discussion of "power" below. The difficulty of changing trading partners to which they refer is akin to my distinction

between ex ante and ex post competition. The issue is this: do the benefits of large-numbers bidding competition (which condition can normally be presumed at the outset, when all potential bidders are at a parity in experience respects) continue at the contract renewal interval, or are they upset during contract execution? The transaction cost answer is that the initial large numbers bidding competition will be *transformed* into one of bilateral exchange at the contract renewal interval *if* execution entails nontrivial transaction specific investments. Winners will then enjoy an advantage over nonwinners, but not otherwise.[32]

Consider finally the relation between bounded rationality, as it is used by Simon and employed here, and the concepts of hyperrationality and "posterior rationality" (Weick 1969; March 1973, 1978). Bounded rationality has been defined as behavior that is "*intendedly* rational, but only *limitedly* so" (Simon 1961, p. xxiv). Insistence that the limited capacities of human agents have important organizational ramifications distinguishes Simon's work from that of the hyperrationality genre. But the absence of hyperrationality does not imply irrationality. On the contrary, the human agents with whom Simon is concerned are attempting effectively to cope. This is what intended rationality is all about. To regard organizations as devices by which to *economize* on bounded rationality is thus suggested by this perspective and is central to the transaction cost approach.

Weick's emphasis is rather different. He argues that decisions made by boundedly rational actors "will be made in terms of localized disturbances to which abbreviated analyses will be applied, with short-term recommendations as the result. A search for more stable solutions . . . is unlikely; consequences are not given much attention, and apparently logical solutions may prove faulty as their consequences ramify" (1969, p. 10). Accordingly, Weick treats cognitions as retrospective (1969, p. 30) and contends that environments are "enacted" (1969, p. 64). As March puts it, "Posterior rationality models maintain the idea that action should be consistent with preferences, but they conceive action as being antecedent to goals" (1978, p. 593).

Intended rationality and posterior rationality models have different organizational design ramifications. Thus, whereas Simon recognizes hierarchy as a means by which to effect semidecomposability, thereby to economize on bounded rationality and produce order out of organizational chaos (by

[32] Another point of contact between transaction cost economics and the Laumann et al. survey concerns the frequency dimension for describing transactions. As they point out, whether relationships are "episodic or highly recurrent" affects the way they are organized (1978, p. 465). Although recurrent transactions are emphasized throughout this paper, frequency is expressly included in my discussion of governance structures elsewhere (1979*b*, pp. 246–54).

permitting operating and strategic decisions, for example, to be clearly distinguished), Weick gives little attention to rational organizational design. Unable to plan or prepare for contingencies, Weick's organizations are given to myopic groping. Ex ante planning gives way to ex post rationalization.

Since the study of organizations can usefully be informed by both perspectives, a forced choice between them is unnecessary and unwise. The question of concentrating research resources nevertheless needs to be faced. Inasmuch as our understanding of organizational anatomy is still primitive, since the study of anatomy logically precedes pathology, and as transaction cost economizing is central to the design and assessment of governance structures, I urge that greater attention to anatomy—viewed through the lens of transaction cost reasoning—is indicated at this juncture.

Power

The resource-dependency model sometimes makes reference to efficiency but more often relies on power in explaining organizational outcomes. Inasmuch as power is very poorly defined and hence can be used to explain virtually anything, the tautological objection to resource-dependency analysis is easily understood. Ready access to a power explanation has also had the unfortunate effect of removing efficiency analysis from center stage.

Thus consider Jeffrey Pfeffer's assertion that if "the chief executive in a corporation always comes from marketing . . . there is a clue about power in the organization" (1978, p. 23). Viewed from a power perspective, the argument evidently is that the marketing people in this corporation have "possession of control over critical resources" (1978, p. 17), have preferential access to information (1978, p. 18), and are strategically located to cope with "critical organizational uncertainty" (1978, p. 28). I do not disagree with any of this, but would make the more straightforward argument that the marketing function in this organization is especially critical to competitive viability.

As Ouchi and I have argued elsewhere (1981), those parts of the enterprise that are most critical to organizational viability will be *assigned* possession of control over critical resources, will *have* preferential access to information, and will be *dealing* with critical organizational uncertainties. In some organizations this may be marketing, in others it may be R & D, and in still others it may be production. Indeed, we argue that failure to assign control to that part of the enterprise on which viability turns would contradict the efficiency hypothesis but would presumably be explained as a power outcome.

Or consider the transformation of the merchant capitalist described by Glenn Porter and Harold Livesay. They report that during the first two centuries after the initial English settlement on the North American continent, "urban merchant capitalists . . . were the wealthiest, best informed, and most powerful segment of early American society" (1972, p. 6). These all-purpose merchants nevertheless gave way to specialized merchants early in the 19th century; such merchants then became "the most important men in the economy" (1972, p. 8). But specialized merchants in turn found their functions sharply cut back by the rise late in the 1800s of integrated manufacturers: "The long reign of the merchant had finally come to a close. In many industries the manufacturer of goods had also become their distributor. A new economy dominated by the modern, integrated manufacturing enterprise had arisen" (1972, p. 12).

Power theory must confront two troublesome facts in explaining these changes. First, why would the all-purpose and later the specialized merchants ever permit economic activity to be organized in ways that would remove power from their control? Second, why did power leak out *selectively*—with the merchant role being appropriated extensively by some manufacturers but not by others? As discussed above and developed elsewhere (Williamson 1980*a*), the transaction cost approach explains both in terms of efficiency. Perhaps power theory can sometimes add detail. However, until it has been much more carefully delimited—which, I submit, will entail dimensionalizing—power theory, as an overall approach to the study of organizational change, is a pied piper whose enticements are better resisted in favor of more mundane efficiency considerations.

VI. CONCLUDING REMARKS

Transaction cost analysis is an interdisciplinary approach to the study of organizations that joins economics, organization theory, and aspects of contract law. It provides a unified interpretation for a disparate set of organizational phenomena. Although applications additional to those set out here have been made,[33] the limits of transaction cost analysis have yet to be reached. Indeed, there is reason to believe that the surface has merely been scratched.

Transaction cost reasoning probably has greater relevance for studying commercial than noncommercial enterprise, since natural selection forces operate with greater assurance in the former. Transaction cost economizing

[33] For applications to organization form, see n. 4 above; for a discussion of oligopoly, see n. 29 above; natural monopoly is assessed in transaction cost terms in Williamson (1976).

is nevertheless important to all forms of organization. Accordingly, the following proposition applies quite generally: governance structures that have better transaction cost economizing properties will eventually displace those that have worse, ceteris paribus. The *cetera,* however, are not always *paria,* whence the governance implications of transaction cost analysis will be incompletely realized in noncommercial enterprises in which transaction cost economizing entails the sacrifice of other valued objectives (of which power will often be one; the study of these trade-offs is an important topic on the future research agenda).

Certain methodological features of the transaction cost approach should perhaps be made more explicit. Three are especially noteworthy. For one thing, the transaction cost approach employs functional analysis in the following sense: "Institutions are functional if reasonable men might create and maintain them in order to meet social needs or achieve social goals" (Simon 1978, p. 3).[34] Second, the approach straddles the methodological dispute that separates maximizers and satisficers. Thus it relies on economizing arguments (which disciplines the analysis and appeals to maximizers) but substitutes comparative institutional for optimizing procedures (which is more in the spirit of satisficing). Inasmuch as the assessment of discrete structural alternatives can often be performed without "elaborate mathematical apparatus or marginal calculation" (Simon 1978, p. 6) and is furthermore entirely adequate for many purposes, such an unpretentious approach to the study of organizations has much to commend it. Third, as already noted, the transaction cost approach relies—in a somewhat informal, background, and long-run way—on the operation of natural selection forces.

While it is injudicious to claim too much for the transaction cost approach, neither do I want to claim too little. At present, it is probably under- rather than overapplied to organization theory. In contrast with the highly microanalytic approach to the study of organizations, in which personalities and detailed organizational procedures are scrutinized, and the highly aggregative approach to organizations employed in mainline economics, the transaction cost approach employs a semimicroanalytic level of analysis. This appears to be a level of analysis at which sociologists and other students of organization enjoy a comparative advantage. Facility with the apparatus, however, requires that an irreducible minimal investment in transaction cost reasoning be made. This paper attempts both to supply requisite background and to make substantive headway on some of the governance issues of common interest to economics, law, and sociology.

[34] The only change that is necessary for my purposes is that "private or social" should be substituted for "social" in the two places where "social" appears in this quotation.

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The Social Class Structure of Occupational Mobility[1]

Ronald L. Breiger
Cornell University

This paper provides an analytical framework within which hypotheses of class structure are brought to bear directly in the formulation of models for the occupational mobility table. The proper aggregation of rows and columns is portrayed as the fundamental theoretical issue in mobility table analysis, rather than as an exogenous "given" to be decided upon prior to the construction of explicit models. Homogeneity of mobility within and between classes, class hierarchy, and tangible boundedness are the central themes. These themes are implemented in loglinear models and applied in the analysis of large (17-category) intergenerational mobility tables. Four such tables from the studies of Blau and Duncan and of Featherman and Hauser are fitted acceptably. Seven falsifiable hypotheses about the social class structure of occupational mobility are identified and assessed comparatively within the new framework.

> A "social class" makes up the totality of those class situations within which individual and generational mobility is easy and typical. [Weber (1922) 1978, p. 302]

An anomaly characterizes the present state of social mobility research. On the one hand, following Goodman's (1972) articulation of a general multiplicative model for cross-classified data, some of the most innovative models and most sophisticated data analyses of contemporary sociology have been formulated to deal with the specifics of the occupational mobility table (Baron 1980; Clogg 1981; Duncan 1979; Featherman and Hauser 1978; Goldthorpe 1980; Goodman 1979*c*; Hauser 1979; Hope 1980*b*). On the other hand, research in this area remains largely atheoretical. There is a persistent tendency, increasingly acknowledged by mobility researchers themselves, for the empirical analyses to talk past the earlier and classical theoretical

[1] Robert M. Hauser and David L. Featherman generously provided the data from their study (Featherman and Hauser 1975, 1978) on which table 1 of this paper is based. I gratefully acknowledge the detailed and productive comments of Harrison C. White, John F. Padgett, and Michael J. Mandel on four earlier drafts of this paper, and the suggestions of James N. Baron, Clifford C. Clogg, Otis Dudley Duncan, Robert M. Hauser, David Karen, David A. Lax, and Lauri L. Perman. Financial support was provided under grants SER76-17502 and SES80-08658 from the National Science Foundation. Requests for reprints should be sent to Ronald L. Breiger, Department of Sociology, Cornell University, Ithaca, New York 14853.

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statements of Marx, Weber, and Sorokin (Åberg 1979; Charvàt, Linhart, and Vercernik 1978; Garnier and Hazelrigg 1976; Goldthorpe 1980; Horan 1974; see also Giddens 1973; Parkin 1979; Wright 1979). Occasionally critics seem content to castigate "increased methodological sophistication, [employed] to the detriment of sociological thought" (Bertaux 1976, p. 394; Coser 1975), thereby advancing neither activity. This paper attempts a constructive reformulation.

Social mobility analysts do not take social classes seriously: that is the problem. As in the landmark contribution of Featherman and Hauser (1978), mobility analysts have consistently "approached the mobility table *without* strong theoretical presuppositions about affinities among occupational strata. Like Blau and Duncan, [they] have worked inductively" (Featherman and Hauser 1978, p. 140, emphasis supplied; see also Clogg [1981] and the discussion in n. 6 below). My aim in this paper is to develop an operational conceptualization of social class structure which enables this concept to be brought to bear directly in the formulation of models for the occupational mobility table.

The explicit goal here is a theoretically informed *concept* of social class structures, that is, "one [providing] for a *reconstruction of data*" (Merton 1968, p. 144), and not the construction or reconstruction of terminology. My usage of the term "class" is much closer to Weber's concept of "social classes" (more fully elaborated below) than to the various class formulations of Marx. It is not incompatible with the latter and may well provide a means of specifying their empirical consequences. Nonetheless, the core of my approach is social class as "the totality of . . . class situations within which . . . generational mobility is easy and typical," with class situations defined as "typical probabilit[ies] of . . . gaining a position in life" (Weber [1922] 1978, p. 302) and reported in occupational mobility tables.

My starting point is the recognition that "the problem of the existence of distinct class 'boundaries' . . . is not one which can be settled *in abstracto*" (Giddens 1973, p. 110). I argue that identification of the social class boundaries of occupational mobility requires "methods that focus attention in turn on various subsets of the entire [mobility] table" (Goodman 1968, p. 1093). The general loglinear model of Goodman (e.g., 1968, 1969, 1972, 1974, 1979*c*) provides the richest context for developing specific models. The new framework developed here is formally a special case of Goodman's general model. However, it addresses the conceptual problem posed succinctly by Vanneman (1977, p. 786): "The advances introduced by Goodman . . . only increase the importance of selecting the appropriate categorization scheme in the first place. In stratification research, for instance, what categories do constitute the empirical classes of modern society, and how does the composition of these classes shift according to institutional context or national setting?"

The following section of this paper offers a formulation of social class that specifies how to study occupational mobility tables. I point out its affinities with and departures from several accepted viewpoints. The new framework is offered in the form of three interrelated theses. Each thesis may also be read as a criterion for an acceptable model of the mobility table. The section headed "Implementation" takes up each thesis in turn; a model that meets these criteria is proposed for highly disaggregated (i.e., large) tables. My indebtedness to Hauser (1979) is made clear there, as are the tangible incompatibilities in our approaches. In the "Data Analysis" section the new framework is brought to bear on 17-category intergenerational mobility tables; four such tables from the studies of Blau and Duncan (1967) and Featherman and Hauser (1978) are fitted acceptably. Seven falsifiable hypotheses about the social class structure of occupational mobility are identified and comparatively assessed within this framework.

THEORETICAL ISSUES

A Statement of the Problem

The most crucial respect in which theorists and empirical analysts have talked past one another may be stated as follows: *There does not exist a model of the mobility table that takes the proper number and composition of occupational categories as an explicit theoretical decision.* This lack of attention notwithstanding, the proper number and composition of social classes, ranging from two classes to "an almost endless multiplicity of class situations" (see Giddens 1973, p. 48), is perhaps the most central problem to be addressed by any theory of social class.

Empirical researchers have turned away from detailed, highly disaggregated mobility tables. The aggregation of mobility data, allegedly "prior" to the construction of models (and certainly prior to the construction of explicit models), is the norm among social mobility analysts. Hauser (1979), for example, sums across the rows and columns of a 17-category table (containing $289 = 17 \times 17$ cells) to obtain a five-category version (containing 25 cells), at which point he claims he *begins* to apply his new model. But I claim that the reduction of the original table to the smaller version, for which incomplete justification is provided (Featherman and Hauser 1978, p. 29) other than the straightforward observation that "we were unable to specify design matrices for 17×17 mobility tables to our satisfaction" (1978, p. 181), constitutes a crucial theoretical decision, which is no less important for being an implicit decision.[2]

[2] Similarly, Goldthorpe, Llewellyn, and Payne combine categories of a 36-category table so as to produce a seven-category version, and then confine their explicit modeling efforts to the smaller table (Goldthorpe 1980, pp. 38–42, 94–120). They explain their reduction

Combining particular categories of a variable can lead to the deletion of parameters that would otherwise be necessary (Goodman 1973; Duncan 1975), or to the insertion of parameters that would otherwise be unnecessary, or to no change in the parameters (Goodman 1979*a*, p. 1087; see also Allison 1980). Even if a model is correctly specified for a five-category mobility table (for example), this fact alone carries no implication that the model fits (say) the 17-category table from which the smaller one was constructed, or vice versa.[3] The ubiquitous proclivity of empirical researchers for combining categories (i.e., producing smaller versions of a given table) in the absence of any consistent framework of sociological theory avoids confronting this issue and turning it to advantage. Indeed, Duncan's (1979, p. 801) recent results "suggest that an important limitation of previous analyses has been the use of too highly aggregated versions" of the mobility table; in particular, the "substantive plausibility" of certain models was enhanced when Duncan applied them to an eight-category rather than to a five-category version of the same table.

Three Theses on Social Class Structures

Until the 1950s, sociological theories of the aggregation of mobility chances were inseparable from theories of social class. The attempt to pull these concerns apart has faltered (Goldthorpe 1980, p. 38). It is now time to bring social class back in to an empirical conceptualization of occupational mobility. This task requires the focused reorientation of theory and methods, and the disavowal both of methods whose elaborate aesthetics obscure empirical problems and of theories with no referent to existing data. The essentials of the framework I propose are offered below in the form of three interrelated theses.

THE AGGREGATION THESIS: With reference to a mobility table, a *social*

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of the given 36 categories to the derived seven, which they then term "classes," by reference to "the available evidence" of comparability on a variety of criteria (Goldthorpe 1980, p. 39). The problem of translating these assorted criteria into a single partition of occupations may be highlighted by noting that, even if the 36 categories were fully rank ordered (a more restrictive assumption than the one the authors actually use), the number of ways to partition them into a seven-category version that preserves the ordering would be in excess of 1.6 million. Goldthorpe and his co-workers are among the most thoughtful researchers who have considered problems of the aggregation of occupational categories without, however, formulating explicit models for such aggregation.

[3] "The fact that the methods presented in this series of articles [by Goodman] can be applied when each variable can have any given number of categories does *not* at all imply that the results obtained with these methods will be unaffected by the number of categories used with each variable" (Goodman 1979*a*, p. 1089, original emphasis). A similar problem exists with respect to finite state Markov chain models (Kemeny and Snell 1960; Bartholomew 1967, p. 18) and with respect to the latent structure models of the mobility table of Clogg (1981).

class structure is a single partition of occupational categories imposed simultaneously on origins (rows of the table) and destinations (columns).

THE INTERNAL HOMOGENEITY THESIS: Mobility from any occupation to any other is explained by the social class structure. Specifically: the dependence of destination on origin (the association of rows and columns) *is always zero* within the subtable formed by crossing the occupations in any class A and those in any class B (where A and B may also index the *same* class).

THE CLASS HIERARCHY THESIS: Classes are ordered with respect to typical mobility chances. Interclass mobility is governed by a small number of parameters stating the dependence of *class* of destination on *class* of origin.

The aggregation thesis serves well to indicate the present disjunction between theory and methods. Many otherwise incompatible theories of class have as a goal the provision of claims about the aggregation of definite social collectivities, for example, Weber's postulation of four social classes, one of which—the propertyless intelligentsia—is composed of technicians, civil servants, and "various kinds of white-collar employees" (Weber [1922] 1978, p. 305). However, no loglinear models of the mobility table exist at present for the aggregation of the social collectivities indexed by the table's rows and columns. Instead, many of the distinctive contributions of loglinear models to sociological analysis (e.g., Goodman 1965, 1968, 1969, 1972, 1979*b*; Hauser 1979; see also Bishop, Fienberg, and Holland 1975, pp. 206–10, 320–24; Pullum 1975; White 1963) result from the identification of various homogeneous sets of *cells* interior to the cross-classification, and the conditioning of "structural" effects on the assignment of cells to such regions. As Baron (1980, p. 817) points out, these contributions stand in sharp contrast to the usual treatment of the table as a conceptual and statistical whole (by equating the structural *effects* with the row and column *marginals*). Given the extensive search to identify "homogeneous" regions, it is remarkable that no loglinear mobility model has been formulated for the aggregates of cells induced by a simultaneous partition of the rows and columns of a table.[4]

In marked contrast to this lack of attention on the part of empirical analysts, it is precisely this form of aggregation that is most relevant to

[4] These brief remarks will be understood more fully by the reader in conjunction with the discussion of models (1) and (2) presented in the "Implementation" section below. Outside the framework of loglinear models, various analysts have investigated certain consequences of simultaneous partitions of a table's rows and columns; however, see n. 5 below and related discussion in the text. Robert Hauser has pointed out to me that the unpublished dissertation of Béland (1978) anticipates much the same idea as the aggregation thesis of this paper, as implemented in model (1) to be presented below. With reference to Béland, Hauser employs this criterion (a single partition of categories applied simultaneously to a table's rows and columns) to "measure the information lost in aggregating the data" (Hauser 1979, p. 450; Featherman and Hauser 1978, pp. 180–84).

theorists of social mobility. To Sorokin ([1927] 1959, pp. 439–40), for example, it appeared "natural that there are cleavages not so much between occupational groups in the narrow sense of the word, as between bigger social subdivisions going on along the lines of the 'affine' and 'non-affine' occupational subdivisions. . . . These differences, being reinforced by differences in the economic status of such classes, create a basis for what is styled as the present class-differentiation, with its satellites in the form of . . . class antagonism and class friction."

The "affine" occupational subdivisions of Sorokin are the "social classes" of Max Weber ([1922] 1978, pp. 302–7, 926–39). Besides his well-known differentiation of "class" from "status" and "party," Weber also distinguished "social classes" from classes based on purely economic criteria. Social class is a distinctive concept for Weber because such classes can be defined only on the basis of an analytical aggregation of mobility chances.

> A social class, in Weber's sense, is formed of a cluster of class situations which are linked together by virtue of the fact that they involve common mobility chances, either within the career of individuals or across the generations. . . . As Weber himself points out, the notion of 'social class' comes much closer to that of 'status group' than does the conception of purely economic class (although as with economic class situations, individuals who are in the same social class are not necessarily conscious of the fact). The notion of social class is important because . . .[without it] it is possible to distinguish an almost endless multiplicity of class situations. But a "social class" exists only when these class situations cluster together in such a way as to create a common nexus of social interchange . . . [Giddens 1973, p. 48]

The thesis of internal homogeneity.—The amgibuity of the last sentence quoted above reveals the insufficiency of the aggregation thesis, if it is uncoupled from the other two theses, to account for social mobility. Certain analysts have partitioned rows and columns of mobility tables, either by ad hoc procedures or by the use of clustering algorithms (Blau and Duncan 1967, pp. 58–67; Hope 1972; Vanneman 1977). Laumann (1966, pp. 89–104; Laumann and Guttman 1966) has defined a social class as "a subset of a population determined by partitioning . . . according to such associational relationships as . . . kinship, friendship, and common residence," and has identified occupational clusters on the basis of coordinates resulting from smallest-space analyses. However, all these procedures "avoid the question of how many classes exist in a society" (Vanneman 1977, p. 788).[5] The

[5] In the same vein, none of these analysts addresses the question of the statistical fit of his partition model to the data, which is a persistent concern of the present paper. Johnson (1980, pp. 67–70) provides a useful discussion of Hope's "contingency hierarchies" model in this regard. The goodness-of-fit criterion applied in Laumann's (1966, 1973) smallest-space analyses does not pertain to the aggregation of occupational categories into larger units.

problem is that, "while there may be an indefinite multiplicity of cross-cutting interests created by differential market capacities, there are only, in any given society, a limited number of classes" (Giddens 1973, pp. 105–6). What is the theoretical property of a social class structure that allows the relatively unambiguous identification of "a limited number of classes"?

It is in his answer to this crucial question that the recent theory of Giddens (1973) arrives, circuitously, back at the position of Blau and Duncan. After discussing this position, I indicate why I abandon it in favor of the thesis of internal homogeneity.

"A consistent pattern of disproportionately low movements between occupational groups is all that is meant by a class boundary here" (Blau and Duncan 1967, p. 60). Likewise, for Giddens (1973, p. 107):

> By the . . . term ["mediate structuration"] I refer to the factors which intervene between the existence of certain given market capacities and the formation of classes as identifiable social groupings, that is to say which operate as "overall" connecting links between the market on the one hand and structured systems of class relations on the other. . . . The mediate structuration of class relationships is governed above all by the distribution of mobility chances which pertain within a given society . . . In general, the greater degree of "closure" of mobility chances—both intergenerationally and within the career of the individual—the more this facilitates the formation of identifiable classes.

In brief, the approach of Giddens, like that of Blau and Duncan, proposes only an internal definition of social classes, not a concept of class structure or of a global mobility *regime*. For these writers, classes are essentially sociometric "cliques" defined so that mobility chances (either on average, or in comparison with expected values under a hypothesis of "perfect mobility") are higher *within* the cliques than *between* them.

The approaches of Blau and Duncan (1967, pp. 58–67) and Giddens (1973, pp. 99–117) cannot be called "explanations" of mobility among occupational categories; Blau and Duncan are clear about it. An explanation would lead to the reproduction of the observed data on detailed mobility between each pair of occupations. Such an explanation "remains to be given by the analyst who can provide the information, and work out the techniques, whereby variables may be partialled out of a typical mobility table to the point where intergenerational constraints on [occupational] destination have been eliminated" (Hope 1980*a*, p. 10). My contention is that an explanation is provided by the conditional independence of occupational origins and destinations within each class and within each pair of classes. The "variables [which should] be partialled out" are the marginal distributions of an explicitly hypothesized social *class* structure, as defined in the thesis on aggregation.[6]

[6] Compare this criterion of internal homogeneity (which assumes the aggregation thesis of this paper) with the criterion of conditional independence applied in models of *latent*

Stated positively, the self-consistent relation of social classes and occupational mobility takes the following form, according to the internal homogeneity thesis. Occupational destination depends on origin *only* when the partition of occupational categories into classes is *ignored*. This is the sense in which occupational mobility may be said to be "explained by" the social class structure.

Or, stated negatively, the units of analysis for occupational mobility are social classes. The researcher who ignores these units confronts a rich and variegated tableau in which occupational mobility (and inheritance) is stronger with respect to certain pairs of occupations than others. In general, the larger the table the more difficult it is to visualize this mosaic as a whole and to interpret its structured pattern of mobility (and inheritance). Analysis of the mosaic may then give way to conventions of routine art appreciation; thus: "It must be emphasized that there is certainly never anything even approaching complete closure" of mobility chances in advanced capitalist societies (Giddens 1973, p. 107).

The proper partition of occupational categories into social classes, then, is one which satisfies the global criterion of internal homogeneity. If there is no such partition for a given table of mobility chances, the approach of this paper clearly fails. If there is more than one such partition, this approach degenerates to the extent that the various partitions are mutually inconsistent (see n. 9 below).

The class hierarchy thesis.—The discussion so far has emphasized an extremely strong definition of social class which goes beyond a partition of occupational categories to require that the partition induce a global mobility regime. Still missing is any stipulation of an ordering among the classes in their provision of mobility chances, so that "the picture . . . which emerges . . . is one marked out by a hierarchy of broad occupational categories each

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structure. Boudon (1973, 1975) and others have proposed, and Clogg (1981) has formulated and implemented, latent structure models of the occupational mobility table. In Clogg's models, "class" is the term given to each of the t latent component tables (each of size $R \times R$, given R occupational categories) into which the original $R \times R$ table is decomposed. Then the hypothesis is that "within the tth latent class the manifest variables . . . are mutually independent" (Goodman 1974, p. 215). In contrast, in this paper "class" refers to a tangible grouping of occupational categories—not to an array of latent $R \times R$ tables of imputed mobility counts. The approach of Clogg (1979, 1981) and that of the present paper each yield substantive insights not obtainable by use of the other approach. The class concept of this paper, in contrast to the class concept of latent structure models, has the relative advantages of (*a*) capturing the usage of Sorokin and Weber, (*b*) allowing the proper number and composition of occupational categories to be taken as an explicit and falsifiable theoretical decision, (*c*) leading to models that fit large mobility tables, (*d*) avoiding the identification problems discussed in Clogg (1981), and (*e*) avoiding recourse to a substantive sociological conceptualization in which classes are viewed as relevant for sociological analysis only as "inherently unobservable" entities (Clogg 1981). The benefits of Clogg's (1981) approach follow from his provision of explicit models which generalize and resolve the classic mover-stayer distinction of mobility table analysis.

representing a different position in the scale of material and nonmaterial benefits" (Parkin 1971, p. 24).

Such a stipulation is provided by the thesis on class hierarchy. This thesis relates expressly to a reduced-form table having one row and column for each class. Thus, a necessary precondition is that internal homogeneity actually prevails. Satisfaction of the criterion of internal homogeneity insures that mobility among *classes* is not confounded with mobility among *occupations*. This precondition (further elaborated below in the subsection headed "Combining Categories") distinguishes the reduced-form tables I study from the reduced-form tables of others (see n. 2 above and related discussion in the text).

Models have been proposed and implemented to exploit the assumption of ordinality for the rows and/or columns of a table (Clogg 1980; Duncan 1979; Goodman 1972, 1979*b*; Levine 1972; Simon 1974); these models are fundamentally interrelated (Goodman 1979*c*). If a mobility table contains a large number of occupational categories, however, the assumption of an ordering of rows and/or columns becomes problematic, both conceptually (because of the increasing prevalence of "horizontal mobility" [Blau and Duncan 1967, p. 117]) and practically (since there are $R!$ possible orderings among R occupational categories). For these reasons, I argue that the appropriate domain of applicability of models for ordered categories is not the *occupational* categories but the *classes* of such categories identified within the framework presented above.

Duality.—Taken together, the three theses postulate a dual structure for the occupational mobility table. What is sought is a single partition of occupational categories, applied simultaneously to the rows and columns of a mobility table. Internally, the rows and columns of each subtable resulting from this partition are unordered, with no dependence of destination on origin. Externally, the classes are ordered with respect to typical mobility chances, class of destination depending differentially on class of origin.

IMPLEMENTATION

Criterion

A model should fit its data. In the preceding section I have consistently identified as one component of "explanation" the ability of a model to reproduce the observed counts of detailed mobility between each pair of occupations. This criterion is not widely accepted by empirical analysts of mobility tables.[7] With rare exceptions (Goldthorpe 1980, pp. 104–8; Hauser

[7] For a brief but cogent discussion of "why a high level of fit should be demanded of mathematical models of mobility processes," see Hope (1976, p. 728).

1979, pp. 445–47) their models produce significant lack of fit to the highly aggregated tables on which they set to work. I view this problem as arising from the lack of a framework for aggregation such as the one proposed in the previous section, and the corresponding lack of applicability of the existing models to large tables. Seeking a model that maintains fidelity to the detailed cell frequencies, I now describe procedures to implement each of the three theses in turn.

Notation

Consider an $R \times R$ table of observed counts $f_{ij} (i = 1, \ldots, R; j = 1, \ldots, R)$ and fitted counts F_{ij} under a specified model; R is the number of occupational categories. As in the previous section, we suppose for substantive reasons that the table is highly disaggregated (R large). This supposition is not, however, required for the purely formal development. As usual, the rows represent occupational origins (e.g., "father's occupation") and the columns represent destinations (e.g., "respondent's first job").

Models

The aggregation thesis.—This calls for a partition of the R occupational categories into c classes. Simultaneous application of this partition to rows and columns of the table results in a partition of the cells (the f_{ij}) into rectangular subtables S_k ($k = 1, \ldots, c^2$).

The thesis of internal homogeneity.—This posits that the observed association of origins and destinations is spurious; absence of association (quasi independence) is specified between origins and destinations *within* each of the rectangular subtables S_k (compare Goodman 1972, p. 689; Hauser 1979, p. 419). Consider the simplest loglinear model satisfying this thesis:

$$F_{ij} = \alpha\beta_i\gamma_j\delta_k \quad \text{for} \quad (i, j) \in S_k , \tag{1}$$

subject to the usual normalization of parameters (Hauser 1979, p. 417).

Formally, model (1) is a special case both of Hauser's (1979) model (see also Goodman 1972, 1979*b*, p. 810) *and* of the new model (2) presented below. Neither of the latter models (Hauser's model and model [2] of this paper) however, implies the other. Comparison of model (1) with the latter two models will therefore serve to highlight the distinctive features implemented in the new approach.

Hauser (1979) partitions the cells of a table into "levels," but he imposes no further restriction (either in theory or in practice; see Hauser 1978, 1979; Featherman and Hauser 1978; Baron 1980; Goldthorpe 1980) that the levels must constitute rectangular subtables formed by application of a single partition to the rows and columns of the table. In Hauser's model,

the cells grouped together into any subset ("level") may in principle be drawn from anywhere in the table. Hauser (1979, pp. 416–17) emphasizes that, "for [his] model to be informative, the allocation of interaction levels across cells of the table must form a meaningful pattern. . . . I have found it difficult to interpret models where the number of interaction parameters is large relative to the number of categories in the occupational classification." In practice, specifying an informative definition of "meaningful pattern" may lead to "a boot strap operation" (Featherman and Hauser 1978, p. 145).

The aim of Hauser (Featherman and Hauser 1973, p. 140) is not a model incorporating social classes; that is the goal of the present paper. Since theories of social class typically concern the aggregation of *categories* and their relational structure (Wright 1979, pp. 3–18) rather than the aggregation of *cells* interior to the mobility table, a model for the aggregation of categories is desirable.

Model (1) is formally a restricted special case of Hauser's model, the restriction being that the partition of cells satisfies the aggregation thesis. This criterion requires the analyst to postulate an assignment of each category in the table to a social class (R assignments are required in all), instead of requiring postulation of an assignment of each cell in the table to a higher level of aggregation (R^2 assignments are required in Hauser's model). As in Hauser's more general model, each expected frequency is the product of an overall effect (α), a row effect (β_i), a column effect (γ_j), and an interaction effect (δ_k). The overall effect is the average of the fitted counts (their geometric mean). The column and row effects reflect features of the overall distributions of respondents and their fathers (respectively) into occupational categories, conditioned upon the model's interaction specification. The interaction effects (there being one such effect for each aggregate of cells identified by the researcher, where these aggregates partition the table's cells) correspond to the "relative densities" of mobility within each respective aggregate (Hauser 1979; Baron 1980, p. 818).

Clearly, the fit of model (1) to large mobility tables is (essentially) always much worse than the fit of Hauser's more general model. This is because the general flexibility afforded the analyst of drawing cells from anywhere in the table into "levels" is sharply curtailed by the aggregation thesis. The corollary of the general flexibility, however, is the danger of a haphazard and uninterpretable aggregation of cells that Hauser warns against.

It follows that some crucial generalization or loosening of model (1) is desirable—a loosening that avoids potentially arbitrary aggregations of cells.

I will now argue for a different generalization of model (1) from the one provided by Hauser. The new model (2) exploits the simple virtue of the

aggregation thesis: "levels" continue to be restricted to the rectangular subtables (S_k) formed from a partition of occupations. Each such subtable has its own row and column effects. A systematic incorporation of these subtable effects, while retaining the partition of the R occupational categories into c classes, is consistent with the theme of this paper that *social classes* are the source of "meaningful patterns" of mobility chances. Thus, consider the model

$$F_{ij} = \alpha\beta_i\gamma_j\delta_k B_{ik}\Gamma_{jk} \quad \text{for} \quad (i,j) \in S_k\,, \tag{2}$$

subject to the normalizations of model (1) and, in addition,

$$\Pi_i\beta_i B_{ik} = \Pi_j\gamma_j\Gamma_{jk} = 1$$

over all i (all j) in the kth subtable. These new parameters insure the fit of the row and column marginals of each respective subtable, thus reflecting features of the origin and destination distributions that are specific to each social class.

Model (2) is thus the hypothesis of simple row-column independence within each subtable. For any particular subtable, the expected values (F_{ij}) produced by model (2) are identical to the expected values (F^*_{ij}) resulting from fitting simple row-column independence to that subtable:

$$F^*_{ij} = \alpha^*\beta^*_i\gamma^*_j \quad \text{for} \quad (i,j) \in S_k\,. \tag{2'}$$

The equivalence of (2) and (2′) is given by:

$$\alpha^* = \alpha\delta_k \tag{3a}$$

$$\beta^*_i = \beta_i B_{ik} \quad \text{for} \quad i \in S_k \tag{3b}$$

$$\gamma^*_j = \gamma_j\Gamma_{jk} \quad \text{for} \quad j \in S_k\,. \tag{3c}$$

Thus, α^* is the average (geometric mean) of the fitted values in the kth subtable, α is the average (geometric mean) of the fitted values in the full ($R \times R$) table, and δ_k is their ratio. Similarly, β^*_i is the effect of row i within the kth subtable, β_i is the effect of row i in the full ($R \times R$) table, and B_{ik} is their ratio. Further discussion is provided in Appendix A.

Formally, model (2) is clearly less parsimonious than model (1): considerably more degrees of freedom are required to fit the additional parameters B_{ik} and Γ_{jk} (see App. A). This relative lack of parsimony in the formal model is countered by the adherence of model (2) to the central themes of this paper. The aggregation criterion is satisfied by model (2), as is the criterion of internal homogeneity (i.e., model [2] mandates that all odds ratios within any subtable S_k have the value of unity). It is unlikely that model (1) can be applied successfully to large tables; model (2) is its natural generalization within the conceptual framework portrayed here. Moreover, the degrees of freedom expended in fitting model (2) are associ-

ated with observed counts of mobility to and/or from social classes; these counts may themselves be modeled explicitly in reduced-form mobility tables (see table 4 and related discussion below).

Analysts of mobility tables often distinguish mobility (the set of cells f_{ij}, $i \neq j$) from "immobility" (the set of cells f_{ii} on the table's principal diagonal), confining their explicit models to the former set of cells. Within the general loglinear model, it is straightforward (Goodman 1965; Bishop et al. 1975) to build this additional restriction into model (2), and I assume it in the applications reported below (see App. A for further details on obtaining fitted values).

The thesis on class hierarchy.—Given a partition of the R categories into c classes that satisfies the internal homogeneity criterion, one may then examine the pattern of mobility among the classes. (Further discussion is provided below in the subsection headed "Combining Categories.") Such examination calls for representation of the $R \times R$ table by a reduced-form $c \times c$ table containing one row and column for each class.

The hierarchy criterion mandates that the classes composing this reduced-form table are ordered and that the dependence of destination class on class of origin is governed by a small number of parameters reflecting this ordering. I will implement this criterion by means of the uniform association model of Duncan (1979; Goodman 1979*b*, pp. 811–12) and "Formulation A" of Simon (1974; Duncan 1979; Goodman 1979*b*, pp. 810–11).

TESTING HYPOTHESES OF CLASS STRUCTURE: DATA ANALYSIS

Mobility table analysis requires an explicit and falsifiable commitment as to the partition of occupational categories into tangible social classes. This is the paradigmatic implication of model framework (2). Heretofore untested commitments abound as to the social class structure of occupational mobility, since non-Marxist theories (Horan 1978) as well as "Marxist theory . . . [have] direct implications for the relationship between occupations and classes as positions within the social structure" (Wright 1979, p. 117; see also Vanneman [1977] on the recasting of social class hypotheses as partitions of occupational categories).

The data analysis presented in this section is nonstandard in several respects which are anticipated here. First, highly disaggregated (17-category) mobility tables are employed, and four such tables from the studies of Blau and Duncan (1967) and Featherman and Hauser (1978) are fitted acceptably by a particular hypothesis, applied under model (2) of the previous section. Second, and most important, seven falsifiable hypotheses about the social class structure of occupational mobility are identified and comparatively assessed within this framework. Third, an aggregated eight-

category mobility table is fitted acceptably by a model postulating an ordering of classes in their provision of interclass mobility chances. These three topics are treated in the subsections below, which take up (in turn) the three theses propounded in this paper.

The primary focus of the data analysis reported here is a 17-category intergenerational mobility table based on the Occupational Changes in a Generation (OCG-II) supplement to the March 1973 Current Population Survey, under the direction of Featherman and Hauser (1975, 1978). Table 1 (a more detailed version of table E.1 in Featherman and Hauser [1978, p. 531], which was kindly provided by the authors) reports mobility from father's occupation to son's first full-time occupation.[8] The fit of each hypothesis will also be assessed with respect to three additional 17-category tables of mobility data arising from Featherman and Hauser's (1978) study and from "OCG-I" (Blau and Duncan 1967).

The Aggregation Thesis

Here I briefly review six hypotheses and indicate sources for each in the literature on occupational mobility. Then I develop my own hypothesis. (The details of each hypothesis—that is, of each hypothesized partition of occupational categories—are provided in App. B).

The conventional triad (H1).—Blau and Duncan (1967, p. 59) suggest the existence of three classes "partitioned by two semipermeable class boundaries that limit downward mobility between generations as well as within lifetime careers, though they permit upward mobility." These classes are the conventional triad: white collar, blue collar, and farm. This partition of the 17 OCG categories (Blau and Duncan 1967, p. 58) is repeated in Appendix B. The Blau-Duncan hypothesis arose from exploratory analysis of three mobility tables. Within the framework of model (2), I will ascertain its goodness of fit to the detailed mobility data.

The proletarianization hypothesis (H2).—Arguing that lower white-collar workers are coming increasingly to resemble manual workers with respect to their mobility chances, Vanneman (1977, pp. 793–98) suggests a three-class partition differing from Blau and Duncan's in that clerical workers are grouped inside the working class, not with white-collar occupations. According to Vanneman (1977, p. 804): "The pattern of results is convincing.

[8] More precisely: table 1 is a classification of son's first full-time civilian occupation by father's (or other family head's) occupation at son's sixteenth birthday, the sons being American men aged 20–64 in March 1973. The reported frequencies are based on a complex sampling design and have been weighted to estimate population counts while compensating for certain types of nonresponse, as detailed in Featherman and Hauser (1978, p. 150, and App. B). The weighted data of table 1 have been rounded to integer values from the unrounded data (kindly provided by Featherman and Hauser) that I analyzed.

TABLE 1

COUNTS IN A 17-CATEGORY CLASSIFICATION OF MOBILITY FROM FATHER'S OCCUPATION TO SON'S FIRST FULL-TIME CIVILIAN OCCUPATION, 1973

FATHER'S OCCUPATION	RESPONDENT'S FIRST OCCUPATION																
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
1. Professionals, self-employed	25	107	20	11	3	27	8	8	8	8	5	12	11	4	7	2	1
2. Professionals, salaried	8	395	64	42	9	116	40	34	59	15	40	61	75	20	30	4	16
3. Managers	14	317	116	89	6	144	68	40	56	31	33	82	89	16	60	5	7
4. Sales, other	7	120	34	45	2	73	27	13	25	7	11	31	33	5	21	1	4
5. Proprietors	19	187	69	52	33	112	82	34	50	36	35	69	103	24	63	11	14
6. Clerks	4	203	41	26	4	145	42	23	49	19	34	74	95	28	42	3	13
7. Sales, retail	5	77	20	20	2	57	47	13	23	8	12	48	46	11	19	3	4
8. Crafts, manufacturing	7	208	49	33	2	174	52	151	67	29	59	104	262	81	75	8	16
9. Crafts, other	6	215	54	38	5	172	54	65	195	53	57	195	175	47	110	6	34
10. Crafts, construction	6	132	29	20	6	131	51	54	71	170	43	142	158	39	130	4	40
11. Service	8	122	47	24	3	170	49	56	65	36	126	130	174	51	110	6	37
12. Operatives, other	5	142	33	18	6	184	71	55	102	49	81	391	239	67	149	11	58
13. Operatives, manufacturing	9	160	37	28	1	188	75	108	93	36	89	171	529	118	123	10	36
14. Laborers, manufacturing	2	33	5	5	0	40	13	26	22	13	25	55	97	92	38	1	26
15. Laborers, other	4	54	11	8	6	86	24	37	42	30	53	101	126	47	142	6	45
16. Farmers	13	252	58	34	10	188	94	86	145	121	102	323	399	150	259	457	981
17. Farm laborers	2	39	8	3	3	39	23	27	42	21	40	96	114	46	83	18	376

SOURCE.—Data provided by Featherman and Hauser (see text, n. 8).

Utilizing two different operational definitions of similarity, two different statistical measures of association, two different categorization schemes, and two different geographic bases, the analysis has supported a single conclusion. The lower-white collar [workers] are clustered with the working class as predicted by the proletarianization hypothesis." Like most of the other writers considered here, Vanneman did not explore the goodness of fit of his hypothesis to data.

The occupational elite hypothesis (H3).—Hope (1972, pp. 173–79) proposes a different modification of the Blau-Duncan partition. Hope's "overall picture is one of a three-class society topped by an elite" (1972, p. 178). Specifically, Hope separates self-employed professionals from the other occupations in Blau and Duncan's white-collar class. Blau and Duncan (1967, p. 72) had previously acknowledged that "the self-employed professionals as a whole constitute a distinct economic elite in our society. . . ." The difference between Blau and Duncan's partition (hypothesis 1) and Hope's (hypothesis 3) arose from differences in their exploratory data-analytic procedures; confirmatory analysis of the statistical fit of either partition was not undertaken.

Hypotheses of occupational strata (H4, H5, H6).—Several mobility table analysts combine categories of 17-category tables to produce smaller tables as a *prelude* to their explicit modeling efforts, and not due to an overt concern with social class structure. Nonetheless, the question naturally arises as to whether the subtables (S_k) identified by these partitions exhibit homogeneity internally. This question will be addressed here within the framework of model (2). I consider the five-category partition of Featherman and Hauser (1978, p. 28; designated as H4 here and in App. B); the nine-category partition of Pullum (1975, p. 116; designated H5); and the 12-category partition of Featherman and Hauser (1978, p. 181; designated H6).

The class-autonomy hypothesis (H7).—I now propose a modification of Hope's partition (H3). The additional distinctions I impose within two of the classes he identified reflect hypothesized variations in the occupational autonomy accorded to workers on the basis of their employment.

There is a broad consensus on the insufficiency of the conventional white-collar and blue-collar categories to account for mobility patterns. The necessary criterion for distinguishing patterns of mobility *internal* to these segments is referred to variously as the entrepreneurial/nonentrepreneurial distinction (Carlsson 1958, pp. 144–46), the bureaucratic/nonbureaucratic setting of jobs and the attendant "possibilities for self-direction and self-employment" (Featherman and Hauser 1978, p. 32; Laumann 1973, p. 81), the degree of role circumscription due to "rational principles explicitly formulated" (Blau and Duncan 1967, p. 73), and the relative control over the labor process exercised by workers in various class locations (Wright

1978, pp. 64–67). What has been missing is an explicit model of class boundaries by which occupational autonomy may be brought to bear directly in an explanation of detailed mobility flows.

With this criterion as the goal, I propose a subdivision of Hope's white-collar segment into three classes (II, III, IV), and I distinguish three classes (V, VI, VII) within his blue-collar segment. The resulting hypothesized partition is as follows (numbers in parentheses refer to the categories of table 1):

I. Proprietary professionals (1)
II. Salaried middle class (2, 3, 4)
III. Small employers (5)
IV. Routinized white-collar workers (6, 7)
V. Semi-autonomous craftsmen (9, 10)
VI. Manufacturing workers (8, 13, 14)
VII. Archaic working class (11, 12, 15)
VIII. Agricultural class (16, 17)

As in Hope's hypothesis (H3), self-employed professionals are deemed to constitute a distinct class (I), as are all agricultural occupations (class VIII).

Within the white-collar segment postulated by Hope (H3), I insist on distinguishing proprietorship of a business from salaried employment. In national samples such as those considered in this paper, proprietors (class III) for the most part own the types of modest establishment (Blau and Duncan 1967, p. 72) often associated with the "traditional" petty bourgeoisie (Wright 1978, p. 39). I subdivide salaried white-collar occupations according to the degree of autonomy typically exercised by their incumbents. The salaried middle class (II) includes occupations (engineer, health administrator, and insurance agent provide examples) whose incumbents, although employees, exercise considerable control over the conditions of their work and the manner in which it is accomplished (compare Wright 1978, p. 81). In contrast, routinized white-collar employees (class IV) such as bank tellers, mail carriers, and sales clerks have such a small degree of control over their work situations as to prohibit (by hypothesis) their inclusion with other white-collar workers (compare Vänneman 1977).

Within the conventional blue-collar segment, the progressive bureaucratization of manual work in the manufacturing industry (Featherman and Hauser 1978, p. 32) and the high rates of unionization and other forms of class organization there (Wright 1978, p. 172) relative to other industries lead me to group together (in class VI) *all* jobs in manufacturing (craft, operative, and laborer) and to distinguish them from cognate occupations in other industries (compare Tolbert, Horan, and Beck 1980). The craft workers in these other industries (e.g., carpenters, typesetters, television mechanics) share relatively nonbureaucratic settings and "have at least a

limited form of autonomy in their immediate labour process" (Wright 1978, p. 81). I distinguish them (class V) from the class (VII) of low-level manual occupations existing in large part prior to the rise of industrial capitalism and still largely outside its immediate scope (e.g., meat cutters, except manufacturing; grounds keepers; practical nurses).

The Thesis on Internal Homogeneity

Each of the seven hypotheses was applied to the data of table 1 by fitting model (2) of the section headed "Implementation," employing each hypothesis in turn to induce the requisite partition. The question posed here, for each hypothesis, is the degree to which it identifies an exhaustive set of regions within the table in such a manner that each region forms a unified field internally. (In the next subsection I will assess the claim that the classes are *ordered* in their provision of interclass mobility chances.)

Results appear in the first column of table 2, which reports the likelihood ratio goodness-of-fit statistic (G^2) for each of the seven applications of model (2) to the data of table 1.

The reader should bear in mind that each of the seven hypotheses was formulated by a researcher (or a research team) with considerable prior knowledge of the detailed interaction structure of the mobility table under study. In three cases (H1, H2, H3) the hypotheses resulted from fairly mechanical exploratory search procedures. In the case of H7, I had extensive prior knowledge of the detailed interactions of table 1. The reader is therefore encouraged to view the findings of the first column of table 2 as *indices* of fit, rather than as formal tests of goodness of fit (see Bishop

TABLE 2

TEST STATISTICS FOR SEVEN HYPOTHESES ON FOUR 17-CATEGORY INTERGENERATIONAL MOBILITY TABLES

Hypothesis	df	First Job 1973 (1)	First Job 1962 (2)	Current Job 1973 (3)	Current Job 1962 (4)
H1	180	407.8	336.7	371.1	323.0
H2	180	405.8	358.6	329.7	321.1
H3	154	361.5	300.9	327.3	282.3
H4	128	279.7	244.9	238.6	207.3
H5	55	158.4	146.4	116.8	86.2
H6	19	127.2	97.9	58.8	47.7
H7	69	76.9	81.7	87.6	80.5

SOURCES.—Col. 1 is table 1 of this paper; col. 3 adapted from Featherman and Hauser (1978, p. 535), cols. 2 and 4 adapted from Blau and Duncan (1967, pp. 496 and 497).

NOTE.—Each hypothesis was fitted under model (2). Reported here are the degrees of freedom (df) and likelihood ratio goodness-of-fit (G^2) statistics resulting from the test of each hypothesis on each 17-category table.

et al. [1975, p. 311] for an elaboration of this distinction; on the "extreme rarity" of formal hypothesis tests in the social sciences, see Leamer [1978]).

The fit of hypothesis H7 to the data of table 1 is acceptable by conventional standards. Under the convenient myth that the hypothesis was formulated with no prior knowledge of the data, the probability associated with a goodness-of-fit statistic (G^2) of 76.9 on 69 df is .24. By way of comparison, none of the other hypotheses fits the data even at the .001 level. The two hypotheses (H5 and H6) with *fewer* than 69 df do not fit appreciably better than the first four (H1 through H4). Neither Vanneman's nor Hope's proposed modification (H2 and H3, respectively) of the Blau-Duncan hypothesis (H1) results in a substantially improved fit to the data within the framework of model (2).[9]

Fitted values to table 1, under hypothesis H7 applied via model (2), are reported in table 3.[10]

Because of the danger that hypothesis H7 fits idiosyncratic features of table 1, I tested its fit (as well as the fit of the six other hypotheses) on three additional 17-category tables of intergenerational mobility: data obtained in 1962 on mobility to first job (Blau and Duncan 1967, table J2.2) and data obtained in 1973 and 1962 on mobility to respondent's "current occupation" (Featherman and Hauser 1978, table E.5; Blau and Duncan 1967, table J2.1). Both the Blau-Duncan and the Featherman-Hauser studies employed identical 17-category occupational divisions.[11] Results of these tests appear in the second, third, and fourth columns of table 2.

[9] Any partition (H_a) *contains* another partition (H_b) if (H_a) $\cap$ (H_b) = (H_b); that is, if all the class boundaries of (H_a) are preserved in (H_b). For example, Hope's partition (H3) is contained in Blau and Duncan's (H1), differing only in its refinement of Blau and Duncan's "white collar" into self-employed professionals and others (see App. B). Hope's refinement of the Blau-Duncan hypothesis leads to a reduction in G^2 of 46.3 (=407.8 − 361.5) at the expense of 26 (=180 − 154) df (see the first column of table 2), which is a marginally significant improvement in fit. Similar assessments may be constructed for all other pairs of hypotheses related in such a way that one hypothesis contains the other. This statement arises from the observation that model (2) under partition H_b includes the parameters of model (2) under partition H_a if H_a contains H_b; therefore, these loglinear models are hierarchical in the sense of Goodman (1970).

[10] Cells in rows 1 and 5 and cols. 1 and 5; cells [6,7], [9,10], [16,17] and their transposes; and diagonal cells 1, 5, 6, 7, 9, 10, 16, and 17 are fitted exactly under hypothesis H7; see App. A. The other diagonal cells report "implied or imputed diagonal counts" (see Duncan 1979, p. 800; Goodman 1972). The 204 remaining cells are here fitted with 69 df (G^2 = 76.9; see table 2). These 204 cells contain 13,892 of the 19,913 cases.

[11] Prior to analysis, reported cell counts in table J2.1 (Blau and Duncan 1967, p. 496) were weighted by a constant so as to yield an adjusted table sum of 10,533 (which is the adjusted sum reported for this table in Featherman and Hauser [1978, p. 534]). A similar adjustment was applied to table J2.2 (Blau and Duncan 1967, p. 497). See Featherman and Hauser (1978, pp. 511–14) for a discussion of weighting and sampling variability as applied to the OCG-I and OCG-II studies. On the comparability of the definitions of "first job" employed in the two studies, see Featherman and Hauser (1978, pp. 23–24).

TABLE 3

FITTED COUNTS FOR TABLE 1 FROM MODEL (2) AND HYPOTHESIS H7

FATHER'S OCCUPATION	RESPONDENT'S FIRST OCCUPATION																
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
1. Professionals, self-employed	25	107	20	11	3	27	8	8	8	8	5	12	11	4	7	2	1
2. Professionals, salaried	8	177.8	59.7	46.7	9	110.7	44.8	34.6	53.6	20.0	30.0	61.7	77.9	15.8	39.4	5.8	14.3
3. Managers	14	321.4	107.9	84.3	6	150.6	60.9	39.1	63.0	23.5	39.8	81.9	88.0	17.8	52.4	3.5	8.7
4. Sales, other	7	115.5	38.8	30.3	2	70.8	28.7	13.6	22.6	8.5	14.4	29.7	30.6	6.2	19.0	1.5	3.8
5. Proprietors	19	187	69	52	33	112	82	34	50	36	35	69	103	24	63	11	14
6. Clerks	4	195.7	42.7	32.2	4	145	42	23.8	49.0	18.5	29.9	80.1	95.3	26.2	39.6	4.2	11.8
7. Sales, retail	5	85.0	18.5	14.0	2	57	47	11.5	23.0	8.7	15.8	42.3	46.2	12.7	20.9	1.8	5.0
8. Crafts, manufacturing	7	208.1	47.4	34.3	2	168.2	58.5	73.2	67.2	28.8	55.9	106.3	262.7	80.6	76.0	4.7	19.2
9. Crafts, other	6	218.1	52.0	36.3	5	167.9	58.0	63.3	195	53	53.6	179.8	177.5	45.9	128.1	4.8	35.1
10. Crafts, construction	6	129.4	30.9	21.5	6	135.3	46.7	55.5	71	170	46.6	156.4	155.5	40.2	111.4	5.3	38.6
11. Service	8	134.0	38.4	20.8	3	164.5	53.8	48.9	65.4	36.0	63.4	127.9	177.9	54.7	111.6	6.2	37.0
12. Operatives, other	5	134.4	38.5	20.9	6	192.1	62.8	62.9	97.7	53.8	83.4	168.4	228.8	70.3	146.8	9.9	59.0
13. Operatives, manufacturing	9	161.7	36.8	26.6	1	194.8	67.8	107.7	89.6	38.4	89.7	170.8	386.6	118.7	122.0	9.2	37.4
14. Laborers, manufacturing	2	31.0	7.1	5.1	0	39.2	13.6	26.8	24.5	10.5	27.8	53.0	96.4	29.6	37.9	5.5	22.2
15. Laborers, other	4	50.7	14.5	7.9	6	83.1	27.2	36.4	46.8	25.7	51.1	103.2	132.5	40.7	90.0	7.3	43.5
16. Farmers	13	253.7	58.2	32.8	10	186.2	96.0	87.1	151.0	114.9	107.2	317.3	396.7	151.1	259.3	457	981
17. Farm laborers	2	36.8	8.4	4.8	3	40.9	21.1	25.7	35.4	26.9	34.2	101.1	117.0	44.6	82.7	18	376

NOTE.—Counts fitted exactly under the model are reported as integers (see text, n. 10).

Hypothesis H7 is seen to fit each of the four 17-category tables. On 69 df, the probabilities associated with likelihood ratio values of 76.9, 81.7, 87.6, and 80.5 are .24, .14, .06, and .16, respectively. These findings suggest that (H7) *does* fit idiosyncratic features of table 1, but they demonstrate its acceptable fit to each of the other tables as well. In contrast, none of the other six hypotheses provides a satisfactory fit to these 17-category tables.[12]

Interpretation.—A model of the mobility table must eliminate the influence of origin and destination distributions on the observed pattern of mobility. Models differ in their conceptualization of this task.

The standard baseline model of statistical independence ("perfect mobility"), for example, identifies the origin and destination distributions with the row and column marginals of the entire $R \times R$ table. Suppose this model had fit the data of table 1. Among the implications of such a finding would be that the odds of becoming (say) a proprietor instead of a laborer were identical for respondents originating in *each* occupational category, net of the abovementioned marginal distributions.

The perfect mobility model may be interpreted as the special case of model (2) in which the analyst supposes that there are no social classes ($c = 1$; see App. A). Classes are conceptualized as those features of the social structure which allow the perfect mobility model to obtain simultaneously within each subtable resulting from a partition of occupational categories. Rather than employ increasingly elaborate models of association for the table as a whole, the goal here is to elaborate social structure so that "perfect mobility" actually holds.

Given the acceptable fit of the modeled counts of table 3 to the observed counts of table 1, and the unacceptable fit of the perfect mobility model,[13] we learn that the latter model confounds patterns of occupational mobility with the effects of mobility to and from social classes. We learn that, net of the class-specific marginals, the association within table 1 is not statistically significant ($P = .24$). This is the sense in which the social class structure postulated by hypothesis H7 explains the observed counts of table 1.

Combining categories.—Until this point, I have refused to reduce the initial 17×17 table to a table of smaller size. Suppose that we now reduce it to size 8×8, by combining categories into the eight classes posited by hypothesis H7. The result is a table reporting observed counts of mobility within and between the social classes. This reduced-form table, constructed from the data of table 1, is provided in table 4 (upper panel).[14]

[12] Of the six other hypotheses, the best fit is provided by H5 applied to the 1962 data on mobility to current job: df = 55, $G^2 = 86.2$, $P = .005$.

[13] On $256 = (17 - 1)^2$ df, the model of independence applied to table 1 yields $G^2 = 7{,}808.2$. Related comments appear in n. 15 below.

[14] The unrounded data on which table 1 is based (see n. 8 above) were employed for this purpose.

Given that we have established the conditional independence of origins and destinations for the full (17 × 17) table, we are in a position to interpret the reduced-form table (table 4) in a new way. Although it is a table of observed counts of mobility within and between classes, it is also something else. It is a matrix of class-specific marginals whose influence was removed in order to fit the model of table 3.[15] (Each count in the upper panel of table

TABLE 4

INTERCLASS MOBILITY TO FIRST JOB, 1973, WITH FITTED COUNTS FOR MODEL M6 AND ROW EFFECTS PARAMETERS ($\hat{b}_i$) FOR THREE MODELS

	Son's Class (First Job)							
	I	II	III	IV	V	VI	VII	VIII
	Observed Counts							
Father's class:								
I	25	137	3	35	16	22	24	3
II	29	1,222	17	467	191	324	368	38
III	19	308	33	194	86	160	167	26
IV	9	388	6	291	99	216	229	23
V	13	488	11	408	490	538	676	84
VI	19	558	4	542	259	1,464	739	98
VII	16	460	14	584	325	853	1,283	163
VIII	14	395	13	344	328	822	902	1,832
	Fitted Counts, Model M6							
Father's class:								
I	6.7	129.4	1.8	48.9	15.8	23.4	19.0	1.5
II	31.4	762.3	13.6	458.1	186.8	348.6	357.1	36.4
III	12.4	311.2	5.7	199.8	84.2	162.3	171.9	18.1
IV	14.8	378.1	7.1	250.9	107.5	210.8	226.9	24.3
V	15.4	468.7	10.5	440.7	244.7	524.5	672.0	85.7
VI	18.9	566.0	12.5	516.3	259.3	596.2	752.6	94.6
VII	14.1	480.1	12.0	565.9	323.1	844.5	1,211.7	173.1
VIII	12.1	400.7	9.8	451.4	252.0	643.8	902.9	126.1
	Row Effects Parameters ($\hat{b}_i$)							
Model:								
M2	1.52	1.27	1.13	1.10	.93	.89	.83	.60
M5	1.43	1.11	1.09	1.07	.90	.91	.82	.80
M6	1.42	1.12	1.09	1.07	.90	.91	.81	.82

SOURCE.—Table 1 of this paper.

[15] A formal decomposition of association in the 17 × 17 table underlies these observations; it is sketched here for the interested reader. One array of class-specific marginals is given by the upper panel of table 4; these are the sums of the subtables S_k into which the 17 × 17 table has previously been partitioned under hypothesis H7. Under the model of independence, this marginal array exhibits $49 = (8 - 1)^2$ df. A second array of class-specific marginals is the 17 × 8 table reporting the *occupational* origins of respondents in each *class;* these are the row marginals of the subtables S_k. Under independence, this marginal array exhibits $112 = (17 - 1)(8 - 1)$ df. The final array of class-specific marginals is the 8 × 17 table reporting the *class* origins of respondents in each *occupation* (also 112 df); these are

4 is, by definition, the sum of one of the 64 subtables S_k resulting from our eight-class hypothesis.) Therefore, a model that fits the reduced-form (class $\times$ class) table of counts will simultaneously and by definition fit certain marginals that were netted out to produce the model that adequately characterizes the initially given (17 $\times$ 17) table. Moreover (to place a remark of Keith Hope's within the framework of the present paper), "we should always begin by fitting the interactions (product terms) and only later fit the main terms" (Hope 1980*a*, p. 9).

We now turn, therefore, to the reduced form (8 $\times$ 8) table of interclass mobility.

The Class Hierarchy Thesis

For expository purposes I discuss the fit of five models to the data of table 4; these five models are the ones treated in Duncan (1979). Results are reported in table 5.

The model of independence or "perfect mobility" (model M1 of table 5) is the standard baseline. Comparing its G^2 value (6,672.4) to the G^2 obtained in testing independence on the *full* (17 $\times$ 17) table (see n. 13), we observe that 85% (=6,672.4/7,808.2) of the association among the *occupational* categories of table 1 is due to the association of *class* of origin with *class* of destination (see related comments in n. 15 above).

Comparing M1 to the model of "quasi-perfect mobility" (independence for off-diagonal cells; model M3 of table 5), we observe the substantial tendency for *class* of destination to be the same as *class* of origin.

Each of the remaining models imposes an assumption as to the *ordering*

TABLE 5

TEST STATISTICS FOR SIX MODELS FITTED TO TABLE 4

Model	df	G^2
Independence (M1)	49	6,672.4
Row effects (M2)	42	2,821.3
Quasi independence, diagonal omitted (M3)	41	886.2
Uniform association, diagonal omitted (M4)	40	168.4
Row effects, diagonal omitted (M5)	34	100.1
Row effects, diagonal and three other cells omitted* (M6)	31	39.5

* The other cells are (VIII, IV), (VIII, V), and (VIII, VI) (see text).

the column marginals of the subtables S_k. Observe that $49 + (112 - 49) + (112 - 49) = 175$, the degrees of freedom used up in testing model (2) when the diagonal cells (f_{ii}) as well as the off-diagonal cells (f_{ij}) are estimated directly. There are $256 = (17 - 1)^2$ df associated with the model of independence for the full ($R \times R$) table, and $256 - 175 = 81 = (R - c)^2$, the number of df left by model (2) (see App. A; here, $R = 17$ and $c = 8$).

of the social classes. Duncan's "uniform association" model (M4) introduces a single additional parameter, b, which "has an obvious conceptual analogy to the regression coefficient, since it governs the shift of the destination distribution that occurs for each 'step' of the ordered origin classification" (Duncan 1979, p. 797; see also Goodman 1979*b*). Model M4 therefore requires that the rows and columns of the table be ordered identically and in the "correct" way.[16] This model leads to a substantial improvement over M3 at the additional expense of just 1 df. Still, the model does not characterize the data.

Models M2 and M5 assume only that *destinations* (cols. of the table) are ordered; the rows need not be. These models require a set of parameters, b_i, one for each row (hence Duncan's term, "row effects" models). The gaps between adjacent columns are assumed equal (Simon 1974, p. 972), but these gaps (governed by the b_i) are allowed to differ between rows. Each b_i is defined *net* of the table's row and column marginal effects, and is interpretable as the conditional odds ratio of mobility to any class j, relative to class $j + 1$, given origins in class i (Simon 1974). Models M2 and M5 differ only in that row effects for the latter model are specified exclusively for the off-diagonal cells.

Model M5 (row effects, diagonal omitted) is the best-fitting model of those considered so far. Moreover, examination of the standardized cell residuals (Bishop et al. 1975, pp. 136–41) resulting from the fit of this model indicated that its lack of fit is due almost entirely to its lack of fit in three cells, each of which involves mobility out of the agricultural class (i.e., row VIII).[17]

In order to confirm that the lack of fit of model M5 results almost exclusively from its lack of fit to these three cells, a row effects model was fitted once again to the table 4 data. This time the three cells identified in note 17 (as well as the cells on the main diagonal, as in model M5) were

[16] Fitted counts for model M4 were obtained by the trial-and-error search method of Duncan (1979, pp. 797–98). The estimated value of b ($\hat{b} = 1.070$) was computed to three decimal places. A more general discussion of the estimation of expected frequencies under this model and the others discussed below is provided by Goodman (1979*b*, pp. 817–18, 1979*c*). In this regard the reader's attention is also called to the comprehensive computer program of Haberman (1979, pp. 571–85) that "can be used to compute maximum likelihood estimates for any log-linear model."

[17] Specifically, the observed mobility from agricultural occupations to routinized white-collar occupations (cell [VIII, IV]) was substantially *less* than that predicted by model M5, while the observed mobility of agricultural workers into semi-autonomous craft jobs (cell [VIII, V]) and into manufacturing jobs (cell [VIII, VI]) was substantially *greater* than the model's predictions. Blau and Duncan (1967, pp. 51, 79) suggest that "farm laborer" serves as an "entry" occupation for young workers "most of [whom] cannot compete effectively for white-collar positions, and [who] thus move into manual work."

treated as structural zeros (Bishop et al. 1975, pp. 140–41).[18] This model is designated M6 here and in table 5. With a goodness-of-fit test statistic of 39.5 on 31 df, the row effects model M6 fits the data acceptably by conventional standards ($P = .14$). Fitted counts obtained from this model are reported in the second panel of table 4. The $\hat{b}_i$ parameters estimated from this model (normed multiplicatively to unity), as well as those resulting from M2 and M5, appear at the bottom of table 4.

Interpretation.—The fit of model M6 provides support for the model's assumption that social class *destinations* are ordered uniformly (from I to VIII as listed in table 4 and previously in the text).[19] The model imposes no assumption about the ordering of class *origins*, however, and in this light it is interesting to examine the row effect parameters (the $\hat{b}_i$). The model specifies (for example) that, net of the row and column marginal effects of table 4, the son of a self-employed professional is 1.42 times more likely ($\hat{b}_i = 1.42$) to have a first job in class II than in class III (and, more generally, that he is 1.42^n times more likely to have a first job in class j than in class $j + n$).

We can define the mobility *differential* bestowed upon sons originating in class i, relative to those who begin life in the adjacent class $i + 1$, as the ratio $\hat{b}_i/\hat{b}_{i+1}$. For the model that fits (M6), the largest mobility differential is for sons of self-employed professionals (class I) relative to those of the salaried middle class (class II). This differential is 1.27 ($=\hat{b}_1/\hat{b}_2 = 1.42/1.12$; see the last line of table 4). The second largest differential is the one spanning the collar line ($\hat{b}_4/\hat{b}_5 = 1.07/0.90 = 1.19$). The third largest is the one between manufacturing and "preindustrial" work ($\hat{b}_6/\hat{b}_7 = .91/.81 = 1.12$). Each of the remaining differentials between adjacent classes is less than 1.03.

[18] Fitted counts for models M5 and M6 were obtained by use of the iterative procedure devised by Duncan (1979, p. 798). This procedure yields "implied or imputed" fitted counts (Duncan 1979, p. 800) even for cells treated as structural zeros.

[19] Correspondingly, the assumption that occupational destinations are ordered uniformly for the *ungrouped* categories (17-category level) is *not* sustained by these data, subject to (*a*) the infinitesimal proportion of possible orderings (of which there are $17! = 3.56 \times 10^{14}$ in all) investigated by this writer, and (*b*) the modeling context of table 5. (More general frameworks, not considered here, are presented in Goodman [1979*c*] and in Clogg [1980]). For example, model M5 applied to the table 1 data yields a G^2 of 1,588.4 on 223 df. With the rows and columns of table 1 permuted to accord with Duncan's socioeconomic index of occupations (see Featherman and Hauser 1978, p. 28), model M5 yields a G^2 of 2,018.8 or 2,033.5 (depending on how the tie between categories 11 and 13 on Duncan's index is resolved), also on 223 df. For this and a related reason (the increasing prevalence of "horizontal mobility" as larger tables are considered [Blau and Duncan 1967, p. 117]), I have argued that the appropriate domain of applicability of models for ordered categories is not the *occupational* categories but the *classes* of such categories identified by application of the aggregation and internal homogeneity criteria.

The finding of a high mobility differential for sons of self-employed professionals as the strongest class difference appearing in these data conflicts with Baron's conclusion (1980, p. 835) that "the major contrast . . . appears to involve a tightly woven 'mental' or white-collar sector, on the one hand, and all other occupational situses—highly differentiated among themselves —on the other." Strict comparisons are unwarranted, however. Baron's examination of the correspondence between Rogoff's Indianapolis study and contemporary mobility trends at the national level employed a different aggregation of occupations and a different modeling framework from those employed here, and his aggregation was not intended to satisfy the criterion of internal homogeneity developed in this paper.

These data bear out the "elite" standing of the sons of self-employed professionals (see discussion above of Hope's hypothesis H3), if the reader is willing to apply the term "elite" to a class with an even higher propensity "not to fall" than would have been the case if the $\hat{b}_i$ parameters had increased *uniformly* with class of origin (i.e., if model M4 had provided a satisfactory fit; see related comments on M4 and M5 in Duncan [1979, p. 799]).[20] The partition applied here (H7) shares with Hope's the postulation of self-employed professionals as a distinct class, but it specifies a more highly differentiated social class structure. Moreover, hypothesis H7 satisfies the criterion of internal homogeneity (see table 2), which is central to the modeling framework developed in this paper.

This discussion of row effects ($\hat{b}_i$) has been confined to parameters estimated from the model (M6) that fits the data. These $\hat{b}_i$ parameters are virtually identical to those estimated from model M5 (compare the last two lines of table 4). Therefore, our interpretation of the $\hat{b}_i$ is *not* altered by the additional exclusion of three cells in row VIII that defines the difference between the model that fits the data (M6) and Duncan's model of "row effects, diagonal omitted" (M5).

Summary.—The major result of the data analysis of this paper is the demonstration of a duality with respect to interclass mobility and occupational mobility. Within the subtables (S_k) defined by social class partition (H7), occupational origins and destinations are independent (table 2). Mobility between the classes (table 4) is characterized by the differential dependence of destination class on origin class under the assumption that destination classes are ordered uniformly. Models that incorporate these claims maintain fidelity to the detailed pattern of cell frequencies.

[20] The mobility differential between class I and class II estimated from model M4, which provides an unsatisfactory fit to the data, is 1.07 (see n. 16). Model M6 estimates this mobility differential as 1.27 (see text).

CONCLUSION

The occupational mobility table has served as an anvil for sociological craftwork since the days when blacksmiths outnumbered social scientists in the American labor force. The history of sociological technique consists in no small degree of implements honed and fashioned at this anvil (see, e.g., Bibby 1975; Blau and Duncan 1967; Boudon 1973; Featherman and Hauser 1978; Goodman 1969; Pullum 1975). The present paper arose from the recognition that sociologists have remained reluctant, throughout this historical period, to forge the *texture* of the material on which they set to work. They have not addressed explicitly the problem that "changes from a finer to a coarser classification (or vice versa) can affect all the measures of association of which we know" (Goodman and Kruskal 1954, p. 737).

The clear fact that "there is no final solution to the problem of the breadth and heterogeneity of categories in a classification of occupations" (Duncan 1968, p. 682; compare Duncan 1975) does not license sociologists to ignore the equally clear fact that "in a class composed totally of the affine occupational groups, e.g., of different groups of unskilled and semiskilled labor, there appears and exists a community of interests . . . considerably different from [that] of another class composed totally of other affine occupational groups. . . . Thus far the partisans of the class struggle may have a basis for their activity and propaganda" (Sorokin 1927, pp. 439–40).

This paper has offered a framework for aggregating occupational categories into affine (social) classes. The aggregation of rows and columns has been portrayed here as the fundamental theoretical issue in mobility table analysis, rather than as an exogenous "given" to be decided upon prior to the construction of explicit models. Internal intraclass and interclass homogeneity, class hierarchy, and tangible boundedness are the central principles elaborated, implemented, and applied to data within this framework.

I have argued that this framework leads to the honing of existing techniques to produce models that fit large mobility tables. Falsifiable commitments which address the problem of the structuring of occupations within tangible social classes are essential for the analysis of mobility within this framework. The proper partition of occupations into social classes is one which satisfies the criterion of internal homogeneity within each class and between each pair of classes (see tables 1, 2, and 3). The appropriate way to combine occupational categories, according to this framework, is to aggregate occupations *within* the same social class (see the reduced-form table 4). In this way, the definition of classes is taken to be theoretically and methodologically prior to the study of mobility patterns *between* them.

The detailed features of large tables can and should be exploited to assess

the goodness of fit of explicit hypotheses about aggregation. The formulation of various hypotheses of interest would call for the use of different sets of initial categories than the 17 OCG categories employed here. The OCG categories are indicative, however, of the possible directions for future work. Consider, for example, the partition of 55 industries into "core" and "periphery" sectors recently proposed by Tolbert, Horan, and Beck (1980) and hypothesized to provide appropriate units of analysis for the study of labor market structure (among other phenomena). Within the framework developed in this paper, the disaggregation of mobility tables by detailed industry code would allow the formulation and testing of hypotheses of a "dual" or a more elaborate "segmented" labor market according to which labor pools tend to be distinct and insulated from one another through the presence of separate seniority ladders. Application of the framework developed here to "internal" labor markets (intragenerational job mobility within a single institution or a small set of "related" institutions) also seems promising, owing to the necessity for aggregating detailed job titles into larger, internally homogeneous categories reflecting typical mobility chances (Rosenbaum 1979; Spilerman 1977).[21]

There is no shortage of hypotheses. What has been lacking has been an operational concept of social class structures. With such a concept, hypotheses can be brought to bear directly in the formulation of models for the occupational mobility table.

APPENDIX A: SOME PRACTICALITIES OF MODEL FITTING

This Appendix treats models (1) and (2) of the "Implementation" section, and presupposes the notation used there. The other models discussed in this paper (see table 5) are treated extensively elsewhere (Duncan 1979; Goodman 1972, 1979*c*; Simon 1974).

Obtaining Fitted Values

Model (1) is a special case of Hauser's (1979) model. Fitted values under model (1) may therefore be obtained by the procedure he outlines (1979, pp. 418–19). Without gain or loss of information, represent the given $R \times R$ table of observed counts as a three-dimensional array $X = \{x_{ijk}\}$ of size $R \times R \times c^2$ defined as:

$$x_{ijk} = \begin{cases} f_{ij} & \text{for} \quad (i, j) \in S_k \\ \phi & \text{otherwise}, \end{cases}$$

[21] The framework of this paper also has relevance for models of social networks; see Breiger (1981) and Holland and Leinhardt (1981).

where ϕ signifies the treatment of a cell as a structural zero (Bishop et al. 1975, p. 59). Let P (father's occupational category), S (respondent's occupational category), and C (the subtables S_k) represent the variables indexed, respectively, by i, j, and k. Following conventional notation, in which the highest-order marginal configurations fitted under a given hierarchical log-linear model are listed in a series of parentheses (Bishop et al. 1975, p. 66), expected values for model (1) are produced by fitting marginal configurations $(P)(S)(C)$ of the three-dimensional X array (compare Hauser 1979, p. 419).

Fitted values for the new model (2), then, result from fitting marginal configurations $(PC)(SC)$ of the same three-dimensional array (see eq. [2] in the text).

Fitted values for model (2) can also be obtained from an identical procedure (see eq. [2']). Consider in turn each of the subtables S_k ($k = 1, \ldots, c^2$). For each subtable, fit the model of row-column independence. The equivalence of this procedure for obtaining fitted values and the procedure of the preceding paragraph is given by eq. [3] in the text.

As in the applications reported in the text, one may choose to confine the explicit model to off-diagonal cells (cells f_{ij} such that $i \neq j$). In that case, constrain the definition of the three-dimensional array to include $X_{ijk} = \phi$ for $i = j$, and proceed as above. In the case of model (2), this procedure is identical to fitting the row-column independence model to each subtable *not* containing cells on the diagonal of the $R \times R$ table, and fitting the model of quasi independence for off-diagonal cells ("quasi-perfect mobility") to each subtable containing *some* cells on the diagonal of the $R \times R$ table.

Degrees of Freedom

Calculation of degrees of freedom for the two models is straightforward (see, e.g., Bishop et al. 1975, pp. 114–22). Model (1) requires fitting a main effect, $(R - 1)$ nonredundant row parameters, $(R - 1)$ nonredundant column parameters, and $(c - 1)^2$ nonredundant subtable parameters. The number of degrees of freedom associated with model (1) will therefore in general be $R^2 - 1 - 2(R - 1) - (c - 1)^2 = (R - 1)^2 - (c - 1)^2$; compare Hauser (1979, p. 418). If model (1) is confined to off-diagonal cells, there will in general be $(R^2 - R) - 1 - 2(R - 1) - (c - 1)^2 = R(R - 3) - c(c - 2)$ degrees of freedom.

Model (2) posits independence within each of the subtables S_k (though clearly not necessarily within the full $R \times R$ table). This model requires the fit of a main effect for the $R \times R$ table, $c^2 - 1$ nonredundant subtable parameters, $c(R - c)$ nonredundant row parameters, and $c(R - c)$ nonredundant column parameters. Model (2) is therefore in general associated with $R^2 - 1 -$

$(c^2 - 1) - 2c(R - c) = (R - c)^2$ degrees of freedom. If model (2) is confined to off-diagonal cells, as it is in the text, its associated number of degrees of freedom is in general $(R - c)^2 - V$, where V equals R less the number of classes containing fewer than three occupational categories. Consider, for example, the number of degrees of freedom associated with the test of hypothesis (H7) on a 17-category table. The hypothesis posits eight classes, of which five contain fewer than three occupational categories. Here $R = 17$, $c = 8$, $V = 17 - 5$, and the number of degrees of freedom is $(17 - 8)^2 - (17 - 5) = 69$ (see the last row of table 2).

Both models (1) and (2) are generalizations of row-column independence (or of quasi independence for off-diagonal cells), and model (2) is a generalization of model (1). In the special case in which the analyst supposes there are no social classes ($c = 1$), one obtains from both models the model of simple row-column independence with $(R - 1)^2$ degrees of freedom, or the model of quasi independence for off-diagonal cells with $(R - 1)^2 - R$ degrees of freedom.

APPENDIX B: SEVEN HYPOTHESES

Name	Source	Partition
The conventional triad (H1)....	Blau and Duncan (1967, p. 58)	(1 2 3 4 5 6 7)(8 9 10 11 12 13 14 15)(16 17)
Proletarianization (H2).....	Vanneman (1977, pp. 793–98)	(1 2 3 4 5 7)(6 8 9 10 11 12 13 14 15)(16 17)
Occupational elite (H3).....	Hope (1972, p. 179)	(1)(2 3 4 5 6 7)(8 9 10 11 12 13 14 15)(16 17)
Five strata (H4).....	Featherman and Hauser (1978, p. 28)	(1 2 3 4)(5 6 7)(8 9 10)(11 12 13 14 15)(16 17)
Nine strata (H5).....	Pullum (1975, p. 116)	(1 2)(3 4)(5)(6 7)(8 9 10)(11)(12 13)(14 15)(16 17)
Twelve strata (H6).....	Featherman and Hauser (1978, p. 181)	(1)(2)(3)(5)(4 7)(6)(8 9 10)(11)(12 13)(14 15)(16)(17)
Class autonomy (H7).....	See text	(1)(2 3 4)(5)(6 7)(9 10)(8 13 14)(11 12 15)(16 17)

NOTE.—See text table 2. Each hypothesis is a partition of the 17 categories listed and named in table 1 (the same listing as in Featherman and Hauser [1978], app. E). The interested reader should note that Featherman and Hauser's categories 11 and 13 (service and manufacturing operatives, respectively) are denoted 13 and 11 (respectively) in Blau and Duncan 1967 (app. J). The Featherman-Hauser numbering is followed consistently in this paper.

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Criteria for Determining Whether Certain Categories in a Cross-Classification Table Should Be Combined, with Special Reference to Occupational Categories in an Occupational Mobility Table[1]

Leo A. Goodman
University of Chicago

This article considers two kinds of criteria for determining whether certain rows in a two-way cross-classification table should be combined and/or whether certain columns in the table should be combined. These criteria pertain to homogeneity and structure. When these criteria are applied to the classic cross-classification on British occupational mobility recently studied by Duncan, Goodman, Hauser, and Clogg, new insights into these data are obtained. The application of these criteria in the present article also helps to clarify the meaning of the criteria considered here and related terms recently discussed by Breiger.

The utility of the criteria discussed herein will be illustrated by applying them to the classic occupational mobility data of Glass (1954) and Miller (1960) recently studied by Duncan (1979), Goodman (1979*b*), Hauser (1979), and Clogg (1981). Table 1A gives these data in the form presented by Miller (1960), table 1B gives them in the form presented by Glass (1954),[2] and table 1C gives them in still another form. Duncan (1979), Hauser (1979), and Clogg (1981) discussed the analysis of table 1A, and Goodman (1979*b*) considered tables 1A and B. In the present article we shall discuss the analysis of table 1C and use our analysis of this table, together with some additional results on the analysis of tables 1A and B, to further illuminate these data.

The occupational mobility table presented by Glass (1954) (table 1B) can be obtained from table 1A by combining occupational categories 5 and 6

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[2] The entry in cell (V,V) of table 1B was given as 714 in the earlier literature that used the occupational mobility data in Glass (1954); but for the sake of consistency with the results given by Miller (1960), we have changed this number to 715. (This change will not affect the analysis presented here.)

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in the latter table, while table 1C is obtained from table 1A by combining occupational categories 5 and 4. Glass's table (table 1B) combined in a single category routine grades of nonmanual occupations and skilled manual occupations, whereas our table 1C combines in a single category routine grades of nonmanual occupations and those nonmanual occupations classified immediately above the routine grades. With our analysis of tables 1A,

TABLE 1

OCCUPATIONAL MOBILITY TABLES

A. CROSS-CLASSIFICATION OF BRITISH MALE SAMPLE ACCORDING TO EACH SUBJECT'S OCCUPATIONAL STATUS CATEGORY AND HIS FATHER'S OCCUPATIONAL STATUS CATEGORY, USING EIGHT STATUS CATEGORIES

Father's Status	Subject's Status							
	1	2	3	4	5	6	7	8
1.....	50	19	26	8	7	11	6	2
2.....	16	40	34	18	11	20	8	3
3.....	12	35	65	66	35	88	23	21
4.....	11	20	58	110	40	183	64	32
5.....	2	8	12	23	25	46	28	12
6.....	12	28	102	162	90	554	230	177
7.....	0	6	19	40	21	158	143	71
8.....	0	3	14	32	15	126	91	106

B. CROSS-CLASSIFICATION OBTAINED FROM TABLE 1A BY COMBINING STATUS CATEGORIES 5 AND 6

Father's Status	Subject's Status						
	I	II	III	IV	V	VI	VII
I.....	50	19	26	8	18	6	2
II.....	16	40	34	18	31	8	3
III.....	12	35	65	66	123	23	21
IV.....	11	20	58	110	223	64	32
V.....	14	36	114	185	715	258	189
VI.....	0	6	19	40	179	143	71
VII.....	0	3	14	32	141	91	106

C. CROSS-CLASSIFICATION OBTAINED FROM TABLE 1A BY COMBINING STATUS CATEGORIES 4 AND 5

Father's Status	Subject's Status						
	I	II	III	IV	V	VI	VII
I.....	50	19	26	15	11	6	2
II.....	16	40	34	29	20	8	3
III.....	12	35	65	101	88	23	21
IV.....	13	28	70	198	229	92	44
V.....	12	28	102	252	554	230	177
VI.....	0	6	19	61	158	143	71
VII.....	0	3	14	47	126	91	106

B, and C, we shall see that, by applying the two kinds of criteria considered here, we obtain good reasons for replacing the eight occupational categories considered by Miller (table 1A) by a system of seven categories. Our replacement by the system of seven categories introduced in table 1C meets the standards set by both kinds of criteria considered here, whereas the replacement by the system of seven categories in Glass's table fails to meet the standards set by one of the kinds of criteria.

The classic British occupational mobility data will be used here for expository and illustrative purposes, but we could have used instead other examples of occupational mobility data that might be more interesting to some of our readers. We decided to use these classic data (despite their limitations) because they have been analyzed by various earlier writers using a wide variety of methods (see, e.g., literature cited in Goodman [1972] and Haberman [1974]), and we thereby facilitate comparison of the results that will be presented in the present article with the results presented by the various earlier writers.

The criteria and methods discussed in the present article can be applied to occupational mobility tables and also to other kinds of cross-classification tables. For an $I \times J$ cross-classification table, these criteria and methods can be used to determine whether certain rows in the table should be combined and/or whether certain columns in the table should be combined. We can consider cross-classification tables in which (1) the number of rows equals the number of columns ($I = J$) and there is a one-to-one correspondence between the row categories and the column categories (as in the occupational mobility table), or (2) the number of rows equals the number of columns but there is not a one-to-one correspondence between them, or (3) the number of rows does not equal the number of columns ($I \neq J$). The criteria and methods presented here can be applied to each of these types of cross-classification tables.

The two kinds of criteria that will be considered here are homogeneity criteria and structural criteria. The homogeneity criteria will be discussed next.

HOMOGENEITY CRITERIA

In his discussion of a somewhat related topic, Breiger (1981) defined and applied the term "internal homogeneity," but we shall see that in the present context his particular definition and application of the term are not helpful. Some other homogeneity criteria will be presented here, and these criteria will be applied in the present context to obtain additional insight into the occupational mobility tables (tables 1A, B, and C).

Let us consider whether the eight occupational categories in table 1A should be replaced by the system of seven "classes" obtained from this

table when, say, categories 4 and 5 are combined (see table 1C). If Breiger's definition of the term "internal homogeneity" were applied in the present context, the boundaries between the seven "classes" would be used to form $7 \times 7 = 49$ rectangular subtables from the entries in the 8×8 table (table 1A), and each of these rectangular subtables would then be used to test the usual null hypothesis of statistical independence between the corresponding rows and columns of the subtable. However, each of the 49 subtables is either a 1×1 table (consisting of only one entry),[3] or a 1×2 table (consisting of two entries in only one row),[4] or a 2×1 table (consisting of two entries in only one column),[5] or a single 2×2 table.[6] Since the usual null hypothesis of statistical independence is meaningless in a 1×1 table, or a 1×2 table, or a 2×1 table, the only subtable among the 49 subtables that could possibly be used to test this hypothesis is the single 2×2 table noted above. But this particular 2×2 table includes two entries that were on the main diagonal of table 1A (the entries [4,4] and [5,5]),[7] and all the entries on the main diagonal were deleted in Breiger's application of the term "internal homogeneity." Thus, this particular definition and application of the term are not helpful in the present context. Some other homogeneity criteria will be presented next.

To determine whether rows 4 and 5 in the 8×8 table (table 1A) are "homogeneous," let us consider first the 2×8 subtable formed from the two rows (rows 4 and 5) and the eight columns. This particular subtable is presented here as table 2A. with the corresponding "occupational inheritance" entries deleted.[8] To examine whether the two rows of table 2A are homogeneous, we can perform the usual test of the hypothesis of quasi-

[3] Each 1×1 table can be described by a single entry (i,j) obtained from the ith row and jth column of the 8×8 table (table 1A), with $i \neq 4$ or 5 and $j \neq 4$ or 5. (Thus, the value of i here can be 1, 2, 3, 6, 7, or 8; and similarly for j.)

[4] Each 1×2 table can be described by a set of two entries $(i,4)$ and $(i,5)$, with $i \neq 4$ or 5.

[5] Each 2×1 table can be described by a set of two entries $(4,j)$ and $(5,j)$, with $j \neq 4$ or 5.

[6] The single 2×2 table can be described by the set of four entries (4,4), (4,5), (5,4), and (5,5).

[7] See n. 6 above.

[8] For occupational mobility tables of the kind described by table 1A, we have found that the entries on the main diagonal often need to be treated separately from the other cells in the table (owing to "occupational inheritance" and related factors), and that analyzing the table with the entries on the main diagonal deleted is often worthwhile (see, e.g., Goodman 1969, 1972; Haberman 1974). Similarly, we delete these "occupational inheritance" entries in the analysis of a given subtable of the occupational mobility table, e.g., the entries in table 2A corresponding to (4,4) and (5,5) in table 1A. For other kinds of cross-classification tables, there may be good reasons for deleting a set of entries different from the set usually deleted in the occupational mobility context, or there may be no need to delete any entries at all.

TABLE 2

SUBTABLES OF TABLE 1A USED IN APPLYING HOMOGENEITY CRITERIA TO STATUS CATEGORIES 4 AND 5

A. Applied to Rows 4 and 5							
11	20	58	...	40	183	64	32
2	8	12	23	...	46	28	12
B. Applied to Columns 4 and 5							
8	18	66	...	23	162	40	32
7	11	35	40	...	90	21	15
C. Applied to Rows 4 and 5 and to Columns 4 and 5							
...	...	...	8	7	...	...	...
...	...	...	18	11	...	...	...
...	...	...	66	35	...	...	...
11	20	58	...	40	183	64	32
2	8	12	23	...	46	28	12
...	...	...	162	90	...	...	...
...	...	...	40	21	...	...	...
...	...	...	32	15	...	...	...

independence in table 2A.[9] This test yields a χ^2 value of 6.61, with 5 df.[10] Thus, the two rows of table 2A are judged to be homogeneous, and the corresponding rows 4 and 5 of table 1A are also judged to be homogeneous.

To determine whether columns 4 and 5 in table 1A are homogeneous, we can apply to these columns a procedure similar to the one described in the preceding paragraph for rows 4 and 5. This procedure now yields table 2B (rather than table 2A), and the corresponding test applied to table 2B now yields a χ^2 value of 1.20, with 5 df. Thus, the two rows of table 2B are judged to be homogeneous, and the corresponding columns 4 and 5 of table 1A are also judged to be homogeneous.

Tables 2A and B have in common only the entries (4,5) and (5,4) from table 1A.[11] Since the χ^2 tests described above for tables 2A and B did not actually take into account those two entries,[12] the χ^2 test for table 2A and the corresponding test for table 2B are statistically independent (asymptotically). The sum of the two χ^2 values can be used to test the hypothesis that rows 4 and 5 are homogeneous and columns 4 and 5 are homogeneous,

[9] The test of quasi-independence in table 2A is equivalent to the usual test of independence in the 2×6 subtable formed from table 2A by deleting cols. 4 and 5 (see, e.g., Goodman 1968). For the 2×6 table, there will be $1 \times 5 = 5$ df in testing the usual hypothesis of independence.

[10] For the sake of brevity, we shall report in the text of this article only the values of the χ^2 statistic pertaining to the likelihood-ratio.

[11] The occupational inheritance entries (4,4) and (5,5) were deleted in tables 2A and B.

[12] The χ^2 test for table 2A was equivalent to a χ^2 test obtained when the entries (4,5) and (5,4) from table 1A are deleted in table 2A (see n. 9), and a similar remark applies to the corresponding test for table 2B.

ignoring entries (4,5) and (5,4) from table 1A. We thus obtain the sum 6.61+ 1.20 = 7.81, with 5 + 5 = 10 df. This hypothesis of homogeneity is thereby accepted.

We next show how to test this hypothesis without ignoring entries (4,5) and (5,4) from table 1A. To examine the hypothesis that rows 4 and 5 are homogeneous and columns 4 and 5 are homogeneous, we now consider the 8 × 8 table formed from table 1A by deleting all entries that are not in one of the two given rows or in one of the two given columns. This particular subtable is presented here as table 2C, with the corresponding occupational inheritance entries deleted.[13] The usual test of quasi-independence in table 2C does not ignore entries (4,5) and (5,4). When this test is applied to table 2C, we obtain a χ^2 value of 7.87, with 11 df. Thus, this hypothesis of homogeneity is also accepted.

To examine in more detail this hypothesis of homogeneity, we can partition the χ^2 statistic from table 2C into its relevant components.[14] These components can be described in terms of the subtables included in tables 3 and 4. The subtables in table 3 were formed from the entries in table 2C as follows: table 3A was formed from rows 4 and 5 and columns 1, 2, and 3 of table 2C; table 3B was formed from rows 4 and 5 and columns 6, 7, and 8. table 3C was formed from columns 4 and 5 and rows 1, 2, and 3; and table 3D was formed from columns 4 and 5 and rows 6, 7, and 8. (The subtables in table 4 will be described later.)

Since tables 3A–D are 2 × 3 tables, there will be 1 × 2 = 2 df in testing the usual hypothesis of independence in each of these tables. The corresponding χ^2 values (each with 2 df) are 1.73, 4.19, 0.82, and 0.26, for tables 3A, B, C, and D, respectively. The sum of these four χ^2 values can be used to test the hypothesis of independence in the four tables (tables 3A–D). We thus obtain the sum 1.73 + 4.19 + 0.82 + 0.26 = 7.00, with 4 × 2 = 8 df. Thus, this hypothesis of homogeneity is also accepted.

To show how the χ^2 values obtained from the subtables in table 3 are related to the χ^2 values obtained for tables 2A, B, and C, we next consider tables 4A, B, and C. Table 4A is a 2 × 2 table formed from the entries in rows 4 and 5 of table 2C by combining columns 1, 2, and 3 into a single column and combining columns 6, 7, and 8 into a single column (with the

[13] Note that table 2C is a combination of tables 2A and B.

[14] The general method for partitioning the χ^2 statistic described in, e.g., Goodman (1968, 1970, 1978) can be applied here. The χ^2 statistic for testing the hypothesis of "homogeneity" in, for example, a 3 × 2 table (with 2 × 1 = 2 df) can be partitioned into two components: (*a*) a component (with 1 df) to test the hypothesis of homogeneity in the 2 × 2 subtable formed from, say, rows 1 and 2 of the 3 × 2 table (deleting row 3 of the table); and (*b*) a component (with 1 df) to test the hypothesis of homogeneity in the 2 × 2 table formed from the 3 × 2 table by combining the corresponding entries in rows 1 and 2. The general method applied in this simple example will be applied in a similar way now to table 2C and later to other tables considered in this article.

TABLE 3

SUBTABLES OF TABLE 2C USED IN APPLYING HOMOGENEITY CRITERIA

A. Applied to Rows 4 and 5 in Columns 1, 2, and 3		
11	20	58
2	8	12
B. Applied to Rows 4 and 5 in Columns 6, 7, and 8		
183	64	32
46	28	12
C. Applied to Columns 4 and 5 in Rows 1, 2, and 3		
8	18	66
7	11	35
D. Applied to Columns 4 and 5 in Rows 6, 7, and 8		
162	40	32
90	21	15

TABLE 4

TABLES FORMED FROM TABLE 2C AND TABLE 3 USED IN APPLYING HOMOGENEITY CRITERIA

A. Applied to Rows 4 and 5 in Columns 1+2+3 and 6+7+8	
89	279
22	86
B. Applied to Columns 4 and 5 in Rows 1+2+3 and 6+7+8	
92	234
53	126

C. Applied to Rows 4 and 5 in Columns 4, 5, and 1+2+3+6+7+8 and to Columns 4 and 5 in Rows 4, 5, and 1+2+3+6+7+8		
...	40	368
23	...	108
326	179	...

entries [4,5] and [5,4] deleted);[15] table 4B is a 2×2 table formed from the entries in columns 4 and 5 of table 2C by combining rows 1, 2, and 3 into a single row and combining rows 6, 7, and 8 into a single row (with entries [4,5] and [5,4]) deleted);[16] and table 4C is a 3×3 table (with the main diagonal deleted) formed from table 2C by combining columns 1, 2, 3, 6, 7, and 8 into a single column and combining rows 1, 2, 3, 6, 7, and 8 into a single row.[17] Since tables 4A and B are 2×2 tables, there will be a $1 \times 1 = 1$ df in testing the usual hypothesis of independence in each of these tables. Also, since table 4C is a 3×3 table with three cells deleted, there will be $2 \times 2 - 3 = 1$ df in testing the hypothesis of quasi-independence in this table. The corresponding χ^2 values (each with 1 df) are 0.69, 0.11, and 0.06, for tables 4A, B, and C, respectively. The sum of these three χ^2 values is $0.69 + 0.11 + 0.06 = 0.86$, with $3 \times 1 = 3$ df.

The sum of the χ^2 values pertaining to the subtables in table 3 was 7.00 with 8 df, and the sum of the χ^2 values in the preceding paragraph was 0.86 with 3 df. The sum of these two sums is $7.00 + 0.86 = 7.87$[18] (with $8 + 3 = 11$ df), which is equal to the corresponding χ^2 value obtained in the analysis of table 2C presented above.

We have partitioned above the χ^2 value (with 11 df) obtained in the homogeneity test pertaining to table 2C into a component pertaining to homogeneity tests in table 3 (with 8 df) and a component pertaining to homogeneity tests in table 4 (with 3 df). The component pertaining to table 3 was also partitioned into four subcomponents (each with 2 df),[19] and the component pertaining to table 4 was partitioned into three subcomponents (each with 1 df).

Table 5A summarizes the partitioning of the homogeneity test pertaining to table 2C into components pertaining to homogeneity tests in tables 3

[15] Note that table 4A can also be formed from the row totals of table 3A and of table 3B.

[16] Note that table 4B can also be formed from the row totals of table 3C and of table 3D.

[17] The general method for partitioning the χ^2 statistic illustrated in the simple example in n. 14 was applied successively to table 2C to obtain the components pertaining to tables 2A and B and 4C (see also nn. 9 and 12), with the components pertaining to tables 2A and B partitioned further to obtain subcomponents pertaining to tables 3A and B and 4A and tables 3C and D and 4B, respectively. Table 4C is obtained when the homogeneity criteria are applied to entries (4,5) and (5,4) in table 2C. Note that table 4C can also be formed from the entries (4,5) and (5,4) in table 2C, the row totals of table 4A, and the row totals of table 4B. The row totals of table 4A are equal to the corresponding totals for rows 4 and 5 in table 2C, with entries (4,5) and (5,4) deleted; and the row totals of table 4B are equal to the corresponding totals for columns 4 and 5 in table 2C with the same two entries deleted. (See also the corresponding row totals from table 2A, and the corresponding row totals from table 2B, with the same two entries deleted in each case.)

[18] The sum is 7.87 (rather than 7.86), with summands having more significant digits than are reported here.

[19] Each subcomponent (with 2 df) can be partitioned into two sub-subcomponents, each with 1 df. The general method for partitioning the χ^2 statistic illustrated in the simple example in n. 14 can be applied here to each subcomponent.

TABLE 5

COMPONENTS OF HOMOGENEITY CRITERIA APPLIED TO STATUS CATEGORIES 4 AND 5

Component	df	Likelihood-Ratio χ^2
A. For Homogeneity Criteria Applied in Tables 3 and 4		
1. Table 3A	2	1.73
2. Table 3B	2	4.19
3. Table 4A	1	.69
4. Table 3C	2	.82
5. Table 3D	2	.26
6. Table 4B	1	.11
7. Table 4C	1	.06
Total	11	7.87*
B. For Homogeneity Criteria Applied in Tables 2A and B and 4C		
1. Table 2A	5	6.61
2. Table 2B	5	1.20
3. Table 4C	1	.06
Total	11	7.87

* See n. 18.

and 4; and table 5B provides a somewhat different partitioning of that homogeneity test into components pertaining to homogeneity tests in tables 2A and B and 4C.[20] Note that the χ^2 value obtained for table 2A (with 5 df) is equal to the sum of the χ^2 values obtained for tables 3A and B and 4A; and the χ^2 value obtained for table 2B (with 5 df) is equal to the sum of the χ^2 values obtained for tables 3C and D and 4B.[21] Thus, the homogeneity tests in tables 3 and 4A and B can be used to partition into components the homogeneity tests in tables 2A and B.

The results presented above demonstrate that occupational categories 4 and 5 in table 1A pass the various homogeneity tests introduced in this section. These results provide justification for the system of seven categories in table 1C.

A somewhat similar set of homogeneity tests can also be applied to examine whether occupational categories 5 and 6 in table 1A should be combined. Table 6 summarizes the results thus obtained. Compare table 6 with table 5. The total χ^2 value on the bottom line in tables 6A and B was obtained by applying a homogeneity test directly to a table somewhat

[20] With respect to the homogeneity tests in tables 2A and B, see nn. 9 and 12. The general method for partitioning χ^2 illustrated in the simple example in n. 14 can be applied successively to obtain the partitionings in tables 5B and 5A and also the other partitionings noted next in this paragraph.

[21] This equality holds with summands having more significant digits than are reported here.

similar in form to table 2C. (The corresponding total χ^2 value on the bottom line in tables 5A and B was obtained by applying a homogeneity test directly to table 2C.) The total χ^2 value on the bottom line in tables 6A and B was obtained from the 8×8 table formed from table 1A by deleting all entries that are not in one of the two relevant rows (rows 5 and 6 in this case) or in one of the two relevant columns (cols. 5 and 6 in this case), with the corresponding occupational inheritance entries deleted. The general method for partitioning the χ^2 statistic illustrated in the simple example in footnote 14 was then applied to the 8×8 table described above to obtain a component pertaining to a 2×6 table (with 5 df, on line 1 of table 6B) formed from the entries in the two relevant rows, and a component pertaining to a 6×2 table (with 5 df, on line 2 of table 6B) formed from the entries in the two relevant columns, and a component pertaining to a 3×3 table with the main diagonal deleted (with 1 df, on line 3 of table 6B) formed from the 8×8 table by combining all the columns in the 2×6 table above into a single column and combining all the rows in the 6×2 table above into a single row.[22]

TABLE 6

COMPONENTS OF HOMOGENEITY CRITERIA APPLIED TO STATUS CATEGORIES 5 AND 6

Component	df	Likelihood-Ratio χ^2
A. For Homogeneity Criteria Applied in Tables Similar to Tables 3 and 4		
1	3	3.03
2	1	2.81
3	1	3.17
4	3	10.05
5	1	.09
6	1	18.75
7	1	.07
Total	11	37.97
B. For Homogeneity Criteria Applied in Tables Similar to Tables 2A and B and 4C		
1	5	9.01
2	5	28.89
3	1	.07
Total	11	37.97

[22] See nn. 9, 12, 14, 17, and 20. By applying the same general method for partitioning the χ^2 statistic, the components pertaining to the foregoing 2×6 table and 6×2 table (on lines 1 and 2 of table 6B) were partitioned further to obtain the subcomponents in table 6A on lines 1, 2, and 3 and on lines 4, 5, and 6, respectively. With respect to, say, the 2×6 table, the corresponding subcomponents (on lines 1, 2, and 3 of table 6A) were obtained by applying the homogeneity criteria to (1) the 2×4 table formed from the relevant rows

From table 6 we see that occupational categories 5 and 6 in table 1A fail the homogeneity tests.[23] These results indicate that the system of seven categories in table 1B (Glass's table) cannot be justified when the homogeneity criteria are applied.[24]

The total χ^2 values on the bottom lines in tables 5 and 6 were partitioned in these tables into sets of χ^2 components and subcomponents that would be relevant in the present context. These total χ^2 values could also have been partitioned into sets of χ^2 components and subcomponents different from the particular sets presented here. (See the general methods of partitioning χ^2 in, e.g., Goodman [1968, 1970, 1971].) In other contexts, different sets of components and/or subcomponents may be more relevant.

The results presented in this section show that Glass (1954) combined in table 1B two occupational categories that had heterogeneous distributions, whereas we combined in our table 1C two categories that had homogeneous distributions. When the definition of these occupational categories is taken into account, the results presented here are not altogether unexpected. (See the definition of the occupational categories included in, e.g., the introductory section of the present article.) The definition of the various occupational categories can sometimes serve as a partial guide, directing the research worker toward the pairs of categories that might possibly pass the homogeneity tests. Later in this article we shall provide a different technique that can also serve this purpose. The research worker can use this technique along with information culled from the definition of the categories to obtain additional partial guidance on this matter.

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in the columns below the two relevant columns in the 8×8 table, (2) the 2×2 table formed from the relevant rows in the columns above the two relevant columns in the 8×8 table, and (3) the 2×2 table formed from the relevant rows by combining into a single column the entries in the columns below the two relevant columns and combining into a single column the entries in the columns above the two relevant columns. Each subcomponent that has more than 1 df can also be partitioned further using the same general method (see, e.g., n. 19).

[23] From the bottom line in tables 6A and B, we see that the total χ^2 is 37.97, with 11 df. Thus, this homogeneity test in table 6 was failed. From table 6B, we see that this failure was due to the fact that the distributions in cols. 5 and 6 of table 1A were heterogeneous (see, e.g., line 2 in table 6B). (The distributions in rows 5 and 6 of table 1A were not heterogeneous; see, e.g., line 1 in table 6B.) The heterogeneity with respect to cols. 5 and 6 can be described in more detail using the results given on lines 4, 5, and 6 in table 6A. (The components tested on lines 4 and 6 of table 6A were heterogeneous, and the component tested on line 5 was not.)

[24] In some contexts, the homogeneity test based upon the total χ^2 in tables 6A and B might be replaced by a weaker test obtained by deleting some specified components in the total. For example, we could consider here the deletion of the component on line 7 of table 6A and on line 3 of table 6B. If this deletion is made, the corresponding total χ^2 becomes 37.90, with 10 df (rather than 37.97, with 11 df.) This homogeneity test was also failed. (The test of "internal homogeneity" in Breiger [1981] can be viewed as a test constructed from certain homogeneity components; but in the present context his particular definition and application of this term would correspond to the deletion of all the relevant homogeneity components.)

For the sake of simplicity, we have focused our attention in this section on methods for determining whether a given set of two rows in the cross-classification table are homogeneous and/or whether a given set of two columns in the table are homogeneous.[25] Similar methods can be applied directly to determine whether a given set of r rows in the $I \times J$ table are homogeneous and/or whether a given set of c columns in the table are homogeneous, with $2 \leqq r \leqq I$ and $2 \leqq c \leqq J$. The general approach to homogeneity criteria presented here can also be extended to the situation in which the I rows are partitioned into subsets of rows and/or the J columns are partitioned into subsets of columns, and the homogeneity criteria are applied to each subset of rows and/or to each subset of columns. (The extension of this general approach will be presented in the final section of this article.)

Having provided in the present section some discussion of the homogeneity criteria, we next consider structural criteria.

STRUCTURAL CRITERIA

To illustrate how structural criteria can be applied in the present context, and to demonstrate how these criteria differ from the kind discussed in the preceding section, we shall now apply the structural criteria to examine the same general question considered in the preceding section; viz., whether the eight occupational categories in table 1A should be replaced by the system of seven classes in table 1B or by the system of seven classes in table 1C.

The structure of the association between father's occupational category and son's occupational category was described for tables 1A and B in Goodman (1979*b*) using association models, and we now include here a similar description for table 1C.[26] Table 7 gives the values of the χ^2 statistics obtained under the association models applied to tables 1A, B, and C.[27]

[25] In the occupational mobility context, where the cross-classification table is square (i.e., where $I = J$ in the $I \times J$ table), and there is a one-to-one relationship between the rows and columns, the given set of two columns would usually correspond to the given set of two rows. But for cross-classification tables where there is not a one-to-one relationship between the rows and columns, there would usually be no need to introduce a correspondence between the given set of two columns and the given set of two rows.

[26] In the analysis of tables 1A and B, the entries on the main diagonal were deleted in Goodman (1979*b*); and a similar deletion is made in the analysis of table 1C here. The association models applied here will be discussed briefly in the present section; they will be discussed further in the next section and the penultimate section of this article.

[27] The χ^2 values in tables 7A and B were included in the analysis of tables 1A and B in Goodman (1979*b*) (except for the values on line 5 of table 7B). Table 7C gives similar results for the analysis of table 1C. The association models on lines 5 and 6 of table 7 are Models II* and II, respectively, defined in Goodman (1979*b*). (To save space, we shall not consider here the related Models I* and I defined in the 1979*b* article. With respect

From the bottom line (line 6) in tables 7A, B, and C,[28] we see that, in describing the association in tables 1A, B, and C, it is sufficient to consider the effects of the rows and the columns on the association in each of the occupational mobility tables. Thus, we find that it is not necessary to introduce any kind of interaction between these row and column effects in

TABLE 7

ASSOCIATION MODELS APPLIED TO TABLES 1A, B, AND C

Association Models	df	Goodness-of-Fit χ^2	Likelihood-Ratio χ^2
A. For Table 1A			
1. Null association	41	555.12	446.84
2. Uniform association	40	55.77	58.44
3. Row-effect association	34	43.13	45.91
4. Column-effect association	34	40.63	42.02
5. Homogeneous row-column effect	34	31.21	32.56
6. Row and column effects	28	27.97	29.15
B. For Table 1B			
1. Null association	29	522.98	408.37
2. Uniform association	28	29.54	30.39
3. Row-effect association	23	24.90	26.29
4. Column-effect association	23	24.81	25.43
5. Homogeneous row-column effect	23	21.47	22.98
6. Row and column effects	18	20.13	21.49
C. For Table 1C			
1. Null association	29	544.82	437.43
2. Uniform association	28	38.32	38.48
3. Row-effect association	23	31.43	31.24
4. Column-effect association	23	28.30	28.80
5. Homogeneous row-column effect	23	23.41	24.72
6. Row and column effects	18	21.42	22.83

to the kinds of questions considered in the present article [e.g., questions about homogeneity], various complications arise with Model I [and Model I*] that do not arise with Model II [and Model II*]. See, e.g., discussion of Model II in the 1979*b* article [p. 547] and in Goodman [1981*b*].)

[28] The χ^2 values on the bottom line in these tables were obtained using the iterative procedure for Model II in appendix B of Goodman (1979*b*); and straightforward modifications of this iterative procedure were used to obtain the results presented for the other association models in table 7. (The other association models can be viewed as special cases of Model II; see, e.g., Goodman [1979*b*, 1981*b*].) For each of the other association models, the appropriate modified form of the iterative procedure for Model II turns out to be preferable, in the analysis of some kinds of data, to the corresponding modified form of the iterative procedure for Model I described by (5.2)–(5.3) and appendix A in Goodman (1979*b*). (Indeed, the iterative procedure for Model I described by [5.2]–[5.3] can be improved upon, in the analysis of some kinds of data, by replacing [5.2] by

$$\hat{F}_{ij} = \hat{\alpha}_i \hat{\beta}_j \exp\left[\sigma_j \hat{\mu}_i + \rho_i \hat{\nu}_j\right],$$

with σ_j and ρ_i defined as in [5.3c] and [5.3d], and by replacing [5.3c] and [5.3d] for γ^{**}_i and δ^{**}_j by the corresponding two formulae for μ^{**}_i and ν^{**}_j in appendix B, with σ^*_j and ρ^*_i in the latter two formulae replaced by the σ_j and ρ_i as used in [5.3c] and [5.3d].)

describing the association in each of these tables. The structure of the association in each occupational mobility table can be described by the association models.

In addition to the association model on the bottom line in tables 7A, B, and C, there are also other association models in table 7 that serve to describe the association in the occupational mobility tables. From table 7 we see that the null association model (on line 1 of tables 7A, B, and C) does not fit the data,[29] but the other association models in table 7 fit the data much better. We would reject (at the .05 level) the uniform association model applied to table 1A (see line 2 of table 7A), but accept the other association models following it in table 7A.[30] With respect to tables 1B and C, we would accept (at the .05 level) the uniform association model and the other association models following it in tables 7B and C.[31]

The fact that the uniform association model was rejected for table 1A but accepted for tables 1B and C might appear at first sight to provide some justification for the replacement of table 1A by table 1B or C. Association in tables 1B and C could be described more simply than association in table 1A. On the other hand, table 1A includes more information than tables 1B and C, and so it should not be surprising that the table which includes more information might need a more complicated model to describe this information.[32] The next section will discuss in somewhat more detail the relationship between results obtained when association models are applied to tables in which some rows are combined and/or some columns are combined (e.g., tables 1B and C) and corresponding results obtained when the association models are applied to the table in which no rows are combined and no columns are combined (e.g., table 1A).

Some of the information about the association between father's occupational category and son's occupational category in table 1A is lost when this table is replaced by table 1B or C; but, in a certain sense, less information about this association is lost with table 1C than with table 1B. Note

[29] Since the entries on the main diagonal are deleted, the null association model is equivalent to the quasi-independence model with the main diagonal deleted.

[30] For further details, see Duncan (1979) and Goodman (1979*b*). The models on lines 2 and 3 of table 7A were considered in both of those articles. For related material, see also Goodman (1979*a*).

[31] For further details with respect to table 1B, see Goodman (1979*b*). (In that article and also in the present article, the level of statistical significance reported for a given test is applicable in the classic context of hypothesis testing, where the given test is considered on its own. Problems associated with sequential and/or simultaneous statistical inference and with the effects of scanning and/or ransacking a body of data are important, but they would take us too far afield; for discussion of somewhat related matters, see, for example, Goodman [1969].)

[32] In table 1B, the separate data in rows 5 and 6 of table 1A are combined, and the separate data in cols. 5 and 6 of this table are combined. Also, with the deletion of entries on the main diagonal in table 1B, the data in cells (5,6) and (6,5) of table 1A are omitted (as well as the data in cells [5,5] and [6,6] of table 1A).

that the χ^2 values for the null association model applied to table 1A are closer to the corresponding values for table 1C than for table 1B (cf. line 1 in tables 7A, B, and C).[33] Since table 1C contains more information about the association in table 1A than does table 1B, caution must be exercised when comparing tables 1B and C with respect to the χ^2 values obtained for a given association model (e.g., the uniform association model).[34]

As noted earlier in this section, with respect to tables 1B and C, we would accept the uniform association model and the other association models following it in tables 7B and C. However, the results in tables 7B and C indicate that these association models fit the data in table 1B somewhat better than they fit the data in table 1C.[35] This difference between tables 1B and C is due, to some extent, to the difference in the total association in the two tables.

We have discussed in this section the application of structural criteria to tables 1B and C. With respect to these two tables, we had noted in the preceding section that the homogeneity criteria were not satisfied by the data relevant to the formation of table 1B (i.e., the data in the rows and the columns of table 1A that were combined in table 1B), but they were satisfied by the data relevant to the formation of table 1C (see tables 5 and 6). In the next section, we shall discuss the relationship between the homogeneity criteria considered in the preceding section and the structural criteria considered above.

Before closing the present section, we take note of the fact that the structural criteria were applied here using the class of association models de-

[33] This relationship between the χ^2 values on line 1 in tables 7A, B, and C is essentially a consequence of results obtained in the preceding section with some of the homogeneity tests in tables 5 and 6. The sum of the corresponding χ^2 values on lines 1 and 2 of table 5B and line 1 of table 7C is equal to the corresponding value that would be obtained on line 1 of table 7A if entries (4,5) and (5,4) were deleted together with the entries on the main diagonal of table 1A; and the sum of the corresponding χ^2 values on lines 1 and 2 of table 6B and line 1 of table 7B is equal to the corresponding value that would be obtained on line 1 of table 7A if entries (5,6) and (6,5) were deleted together with the entries on the main diagonal. The relationship between results in tables 5, 6, and 7 will be discussed further in the following sections.

[34] When comparing tables 1B and C with respect to the χ^2 values obtained for, say, the uniform association model, we should take into account the fact that the total association is different in the two tables (see, e.g., line 1 in tables 7B and C).

[35] With respect to table 1C, we do not reject the uniform association model at the .05 level, but we would at the .10 level (see line 2 of table 7C). Also, comparison of lines 2 and 4 in table 7C indicates that the column effect on the association in table 1C is not statistically significant at the .05 level but would be statistically significant at the .10 level. And comparison of lines 2 and 5 in table 7C indicates that a homogeneous row-column effect on the association in table 1C is significant at the .05 level. (This summary of results in table 7C was obtained using the likelihood-ratio χ^2s in this table. If the goodness-of-fit χ^2s in table 7C are used instead, a slightly different summary would be obtained.) With respect to table 1B, the results in table 7B indicate that the uniform association model fits the data well, and the row and column effects on the association are negligible (see Goodman 1979*b*).

scribed in Goodman (1979*b*).[36] Structural criteria can also be applied in the situation in which other kinds of models may be of interest. The models of interest can be used instead of the association models in the application of the structural criteria. The association models were used in the present section to illustrate the application of these criteria. These criteria could be described in more general terms, but we shall not go into these matters here.

HOMOGENEITY AND STRUCTURAL CRITERIA

In the preceding sections, our attention was focused primarily on the analysis of some occupational mobility tables, but the methods applied there in the analysis of these tables could be extended and applied more generally to other kinds of cross-classification tables as well. In the present section, the exposition will be facilitated by first considering the analysis of a general two-way $I \times J$ cross-classification table; then we shall show how to apply the results developed here for the general two-way table to the occupational mobility tables.

For the $I \times J$ cross-classification table, let f_{ij} denote the observed frequency in the ith row and jth column of the cross-classification ($i = 1, 2, \ldots, I; j = 1, 2, \ldots, J$), and let F_{ij} denote the corresponding expected frequency under some model. For 2×2 subtables formed from adjacent rows (rows i and $i + 1$) and adjacent columns (cols. j and $j + 1$), let Θ_{ij} denote the corresponding odds-ratio (for $i = 1, 2, \ldots, I - 1; j = 1, 2, \ldots, J - 1$) based on the expected frequencies:

$$\Theta_{ij} = (F_{ij}F_{i+1,j+1})/(F_{i,j+1}F_{i+1,j}) . \tag{1}$$

Under the usual model of statistical independence between the row categories and the column categories in the $I \times J$ cross-classification,

$$\Theta_{ij} = 1 , \quad \text{for } i = 1, 2, \ldots, I - 1 ; j = 1, 2, \ldots, J - 1 . \tag{2}$$

The odds-ratios Θ_{ij} can be displayed in an $(I - 1) \times (J - 1)$ table. Model (2) states that all the Θ_{ij} in this $(I - 1) \times (J - 1)$ table are equal to 1. This model is equivalent to the usual model of statistical independence between the row categories and the column categories in the $I \times J$ cross-classification; and the usual χ^2 statistic used to test the latter model (with $[I - 1][J - 1]$ df) serves also as a test of the equivalent model (2).

Consider next the model which states that the distributions in two specified adjacent rows (say, rows i' and $i' + 1$) are homogeneous. This model is equivalent to the usual model of statistical independence between the row categories and the column categories in the $2 \times J$ cross-classification formed as a subtable of the $I \times J$ cross-classification by considering only the two specified rows (rows i' and $i' + 1$) in the cross-classification. For

[36] For related material, see, e.g., Duncan (1979) and Goodman (1979*a*).

the two specified rows, this model is equivalent to the model which states that

$$\Theta_{i'j} = 1 , \quad \text{for } j = 1, 2, \ldots, J - 1 . \tag{3}$$

Model (3) states that the $\Theta_{i'1}, \Theta_{i'2}, \ldots, \Theta_{i',J-1}$ are equal to 1. The usual χ^2 statistic used to test the model of statistical independence between the row categories and the column categories in the $2 \times J$ cross-classification described above (with $[2 - 1][J - 1] = J - 1$ df) serves also as a test of the equivalent model (3).

Consider now the cross-classification obtained from the $I \times J$ cross-classification by combining the corresponding entries in the two specified adjacent rows. We thus obtain a $(I - 1) \times J$ cross-classification. If the distributions in the two specified adjacent rows (rows i' and $i' + 1$) of the $I \times J$ cross-classification are homogeneous, we find that (*a*) condition (3) will be satisfied for the specified $\Theta_{i'j}$ (for $j = 1, 2, \ldots, J - 1$) obtained from the $I \times J$ cross-classification, and (*b*) the other odds-ratios Θ_{ij} (with $i \neq i'$) obtained from the $I \times J$ cross-classification will be equal to the corresponding odds-ratios obtained from the $(I - 1) \times J$ cross-classification. Thus we can partition into two components any given hypothesis H about the odds-ratios Θ_{ij} in the $I \times J$ cross-classification which states, in part, that condition (3) is true for the specified $\Theta_{i'j}$ (for $j = 1, 2, \ldots, J - 1$): the first component of H states that the distributions in the two specified adjacent rows (rows i' and $i' + 1$) are homogeneous; and the second component of H states that the hypothesis about the other odds-ratios Θ_{ij} (with $i \neq i'$) holds true for the corresponding odds-ratios obtained from the $(I - 1) \times J$ cross-classification.[37]

Consider now, say, the row-effect association model applied to the $I \times J$ cross-classification. This model states that

$$\Theta_{ij} = \theta_{i\cdot} ; \quad \text{for } j = 1, 2, \ldots, J - 1 , \tag{4}$$

where $\theta_{i\cdot}$ is unspecified (for $i = 1, 2, \ldots, I - 1$). We can estimate the parameters $\theta_{i\cdot}$ in this model, and test various hypotheses about them, using the general methods described in Goodman (1979*b*). Consider, for example, the hypothesis H′ that model (4) holds true and that

$$\theta_{i'\cdot} = 1 , \tag{5}$$

for a specified value i'.[38] Since one of the parameters $\theta_{i\cdot}$ in model (4) is not estimated under hypothesis H′ (namely, the specified parameter $\theta_{i'\cdot}$ in [5]),

[37] The general results described above can be illustrated by considering, say, the special case in which $I = J = 3$, $i' = 1$, and the particular hypothesis H about the odds-ratios Θ_{ij} states that condition (2) is satisfied. The general result can be applied also when the given hypothesis H about the Θ_{ij} is different from the hypothesis that condition (2) is satisfied (as long as the given hypothesis H states, in part, that condition (3) is true for the specified $\Theta_{i'j}$). This general result is applied later in this article and in, e.g. Goodman (1981*b*).

[38] Note that if condition (5) is satisfied, then condition (3) will be satisfied under model (4).

there will be one less parameter to estimate under H′ than under model (4); and thus there will be one more degree of freedom in testing H′ than in testing model (4). Since there are $(I-1)(J-2)$ df used in testing model (4),[39] the number of degrees of freedom used in testing hypothesis H′ will be

$$(I-1)(J-2)+1\,. \tag{6}$$

From the comments at the end of the preceding paragraph, we see that hypothesis H′ can be partitioned into two components: the first component states that the distributions in the two specified rows i' and $i'+1$ are homogeneous; the second component states that the row-effect association model holds true for the $(I-1)\times J$ cross-classification obtained by combining the corresponding entries in rows i' and $i'+1$.

To test the first component in the $2\times J$ subtable formed from rows i' and $i'+1$, there will be $(J-1)$ df; and to test the second component in the $(I-1)\times J$ cross-classification, there will be $(I-2)(J-2)$ df.[40] The χ^2 statistics can be calculated for each component, and the sum of the corresponding χ^2 components can serve as a test of hypothesis H′. The number of degrees of freedom pertaining to the sum of the two components will be

$$(J-1)+(I-2)(J-2)=(J-2)(I-1)+1\,. \tag{7}$$

The number of degrees of freedom in (7) turns out to be equal to the number in (6), since hypothesis H′ could be partitioned into the two components described above.

Consider next, say, the column-effect association model applied to the $I\times J$ cross-classification. This model states that

$$\theta_{ij}=\theta_{\cdot j}\,,\quad \text{for } i=1,2,\ldots,I-1\,, \tag{8}$$

where $\theta_{\cdot j}$ is unspecified (for $j=1,2,\ldots,J-1$). Methods similar to those described above to test hypothesis H′ can be applied also to test the hypothesis H* that model (8) holds true and that

$$\theta_{\cdot j^*}=1\,, \tag{9}$$

for a specified value of j^*. Since there are $(I-2)(J-1)$ df used in testing model (8),[41] the number of degrees of freedom used in testing hypothesis H* will be

$$(I-2)(J-1)+1\,. \tag{10}$$

The first component of hypothesis H* states that the distributions in the two specified columns j^* and j^*+1 are homogeneous; and the second component of H* states that the column-effect association model holds true

[39] See, e.g., line 3 in table 4 of Goodman (1979*b*).

[40] This follows from the formula on line 3 in table 4 of Goodman (1979*b*) applied to the $(I-1)\times J$ cross-classification.

[41] See, e.g., line 4 in table 4 of Goodman (1979*b*).

for the $I \times (J-1)$ cross-classification obtained by combining the corresponding entries in columns j^* and j^*+1.

To test the first component in the $I \times 2$ subtable formed from columns j^* and j^*+1, there will be $(I-1)$ df; and to test the second component in the $I \times (J-1)$ cross-classification, there will be $(I-2)(J-2)$ df. Note that the number of degrees of freedom pertaining to the sum of the two components will be

$$(I-1)+(I-2)(J-2)=(I-2)(J-1)+1\,, \tag{11}$$

which turns out to be equal to the number in (10).

Consider next, say, the association model that includes both row effects and column effects on the association in the $I \times J$ cross-classification.[42] This model states that

$$\log \Theta_{ij}=\phi_i.\phi_{.j}\,, \quad \text{for } i=1,2,\ldots,I-1\,;\ j=1,2,\ldots,J-1\,, \tag{12}$$

where $\phi_i.$ and $\phi_{.j}$ are unspecified and where log denotes the natural logarithm.[43] Methods similar to those described earlier in this section to test hypothesis H′ can be applied now to test the hypothesis M′ that model (12) holds true and that

$$\phi_{i'}.=0\,, \tag{13}$$

for a specified value of i'.[44] Since there are $(I-2)(J-2)$ df used in testing model (12),[45] the number of degrees of freedom used in testing hypothesis M′ will be

$$(I-2)(J-2)+1\,. \tag{14}$$

The first component of hypothesis M′ states that the distributions in the two specified rows i' and $i'+1$ are homogeneous; and the second component of M′ states that the association model which includes both row effects and column effects holds true for the association in the $(I-1) \times J$ cross-classification obtained by combining the corresponding entries in rows i' and $i'+1$.

To test the first component in the corresponding $2 \times J$ subtable, there will be $(J-1)$ df; to test the second component in the $(I-1) \times J$ cross-classification, there will be $(I-3)(J-2)$ df. Note that the number of degrees of freedom pertaining to the sum of the two components will be

$$(J-1)+(I-3)(J-2)=(J-2)(I-2)+1\,, \tag{15}$$

which turns out to be equal to the number in (14).

[42] We consider here Model II defined in Goodman (1979*b*). For some comments pertaining to the use of this model in the present article, see parenthetical remark at the end of n. 28.

[43] See, e.g., Goodman 1979*b*, 1981*a*, 1981*b*.

[44] Note that if condition (13) is satisfied, then condition (3) will be satisfied under model (12).

[45] See, e.g., line 6 in table 4 of Goodman (1979*b*).

Methods similar to those described above to test hypothesis M′ can also be applied to test the hypothesis M* that model (12) holds true and that

$$\phi_{\cdot j^*} = 0\,, \tag{16}$$

for a specified value of j^*. Similar methods can also be applied to test the hypothesis M′* that model (12) holds true and that conditions (13) and (16) are satisfied for a specified value of i' and a specified value of j^*, respectively.

The results described earlier in this section for hypotheses H′ and M′ indicate that we can examine certain kinds of hypotheses about the $I \times J$ cross-classification by (*a*) applying homogeneity criteria to the $2 \times J$ subtable formed from rows i' and $i' + 1$, and (*b*) applying structural criteria to the $(I - 1) \times J$ cross-classification obtained by combining corresponding entries in rows i' and $i' + 1$. The corresponding results for hypotheses H* and M* indicate that we can examine certain kinds of hypotheses about the $I \times J$ cross-classification by (*a*) applying homogeneity criteria to the $I \times 2$ subtable formed from columns j^* and $j^* + 1$, and (*b*) applying structural criteria to the $I \times (J - 1)$ cross-classification obtained by combining corresponding entries in columns j^* and $j^* + 1$. In a similar way, the corresponding results for hypothesis M′* indicate that we can also examine certain kinds of hypotheses about the $I \times J$ cross-classification by (*a*) applying homogeneity criteria to the table formed from the $I \times J$ cross-classification by deleting all entries that are not in either row i' or $i' + 1$ or in column j^* or j^*+1, and (*b*) applying structural criteria to the $(I-1) \times (J - 1)$ cross-classification obtained by combining rows i' and $i' + 1$ and by combining columns j^* and $j^* + 1$.

The results presented so far in this section pertain to the analysis of an $I \times J$ cross-classification in which none of the entries are deleted on a priori grounds. These results can be extended to the analysis of an $I \times J$ cross-classification in which a given set of entries are deleted on a priori grounds. We illustrate this extension now by reexamining the analysis of the 8×8 cross-classification (table 1A) and the related 7×7 cross-classifications (tables 1B and C).

From lines 1 and 2 of table 5B, we saw that the homogeneity criteria yielded a χ^2 value of 6.61 (with 5 df) when applied to rows 4 and 5 of table 1A, and a χ^2 value of 1.20 (with 5 df) when applied to columns 4 and 5.[46] When these criteria were applied both to rows 4 and 5 and to columns 4 and 5, we obtained a χ^2 value of $6.61 + 1.20 = 7.81$, with $5 + 5 = 10$ df.[47]

[46] The entries in cells (4,4) and (5,5) of table 1A were deleted in the calculation of the χ^2 values throughout this article.

[47] For the sake of simplicity, we focus now on the case in which the entries in cells (4,5) and (5,4) of table 1A are deleted (as well as the entries in cells [4,4] and [5,5] of the table). When the entries in cells (4,5) and (5,4) are not deleted, we obtain instead a χ^2 value of 7.87, with 11 df (see the bottom line of table 5B).

This χ^2 value can be viewed as the first component (the homogeneity component) obtained in an analysis of the 8×8 cross-classification (table 1A). We next consider the corresponding second component (the structural component) obtained in this analysis.

From line 6 of table 7C, we see that the χ^2 value is 22.83 (with 18 df) when the association model which includes row and column effects is applied to table 1C. This χ^2 value can be viewed as the second component in the present analysis of table 1A. The sum of the first and second components is

$$7.81 + 22.83 = 30.64, \tag{17}$$

with $10 + 18 = 28$ df. The sum in (17) serves to test the hypothesis that the association model which includes row and column effects holds true for table 1A, and that the odds-ratio parameters pertaining to the cells in rows 4 and 5, and to the cells in columns 4 and 5, are equal to one.[48] From (17) we see that this hypothesis is congruent with the data.[49]

The preceding paragraph showed how to combine structural criteria applied to table 1C and homogeneity criteria applied to a relevant subset of the data in table 1A in order to analyze a corresponding hypothesis about table 1A. In a similar way, we can combine structural criteria applied to table 1B and homogeneity criteria applied to a relevant subset of the data in table 1A in order to analyze a corresponding hypothesis about table 1A. Thus, from lines 1 and 2 of table 6B, we see that the corresponding homogeneity component yields a χ^2 value of $9.01 + 28.89 = 37.90$, with $5 + 5 = 10$ df;[50] and from line 6 of table 7B we see that the corresponding structural component yields a χ^2 value of 21.49, with 18 df. The sum of the first and second components is

$$37.90 + 21.49 = 59.39, \tag{18}$$

with $10 + 18 = 28$ df. The sum in (18) serves to test the hypothesis that the association model which includes row and column effects holds true for table 1A, and that the odds-ratio parameters pertaining to the cells in rows 5 and 6, and to the cells in columns 5 and 6, are equal to one.[51] From (18) we see that this hypothesis is not congruent with the data.

The results in this section show that, when the homogeneity and struc-

[48] The sum in (17) is obtained when this hypothesis is applied to table 1A with the entries deleted on the main diagonal and in cells (4,5) and (5,4).

[49] This hypothesis and some other related hypotheses will be discussed further in a later section.

[50] For the sake of simplicity, we focus our attention now on the case in which the entries in cells (5,6) and (6,5) of table 1A are deleted (as well as the entries in cells [5,5] and [6,6] of the table). When the entries in cells (5,6) and (6,5) are not deleted, we obtain instead a χ^2 value of 37.97, with 11 df (see the bottom line of table 6B).

[51] The sum in (18) is obtained when this hypothesis is applied to table 1A with the entries deleted on the main diagonal and in cells (5,6) and (6,5).

tural components are combined in the way described here, justification can be found for the replacement of table 1A by table 1C but not by table 1B.

We noted in an earlier section that the definition of the various occupational categories can sometimes provide partial guidance in directing the research worker toward the pairs of categories that might possibly pass the homogeneity tests. From the results presented in the present section, we can obtain a different technique that can also serve this purpose. This technique will be described next.

For two specified adjacent rows (say, rows i' and $i' + 1$), we noted earlier that the condition of homogeneity was equivalent to condition (3) pertaining to the relevant odds-ratios $\theta_{i'j}$ (for $j = 1, 2, \ldots, J - 1$). Similarly, with respect to two specified adjacent columns (say, columns j^* and $j^* + 1$), the condition of homogeneity is equivalent to a condition similar to condition (3) pertaining to the relevant odds-ratios θ_{ij^*} (for $i = 1, 2, \ldots, I - 1$). Thus, the estimated odds-ratio parameters pertaining to the cells in adjacent rows, and to the cells in adjacent columns, can provide partial guidance with respect to the possible outcome of the corresponding homogeneity tests.[52] If the estimated odds-ratio parameter pertaining to adjacent rows is quite different from one, then the corresponding adjacent rows will usually not be homogeneous (see [5]); and if the estimated odds-ratio parameter pertaining to adjacent columns is quite different from one, then the corresponding adjacent columns will usually not be homogeneous (see [9]). The magnitudes of the estimated odds-ratio parameters obtained for table 1A[53] are concordant with the overall results on homogeneity reported earlier in this article. When considering which pairs of occupational categories might possibly pass the homogeneity tests, the magnitudes of the estimated odds-ratio parameters could have been used by the research worker to direct him away from the pair of categories combined by Glass (1954) and toward the pair of categories combined in the present article in table 1C.

The results presented in this section can be viewed from the perspective of the partitioning of the total association in a cross-classification in terms of the possible component effects on the association. For an $I \times J$ cross-classification, the χ^2 statistic for testing the usual hypothesis of null association (with $[I - 1][J - 1]$ df) can be partitioned into a homogeneity component applied to a specified subtable of the $I \times J$ table and a component related to the structure of association in a corresponding table of smaller dimension. For the 8×8 cross-classification (table 1A), the usual χ^2

[52] The odds-ratio parameters in the association models considered here can be estimated using the methods introduced in Goodman (1979*b*).

[53] See, e.g., the estimated parameters in table 8A of Goodman (1979*b*). Estimates obtained under the association models on lines 5 and 6 of table 7 in the present article (Models II* and II in the 1979*b* article) can be helpful in the situation in which these models are needed to describe the association in the cross-classification.

statistic with $7 \times 7 = 49$ df can be partitioned into a homogeneity component pertaining to, say, occupational categories 4 and 5 (with 13 df),[54] and a component (with $6 \times 6 = 36$ df) related to the structure of association in a corresponding 7×7 table (table 1C).[55] Each of these two components can be partitioned further into appropriate subcomponents. If the relevant homogeneity subcomponents are negligible, then the total association in the $I \times J$ table can be described simply in terms of the subcomponents pertaining to the structure of association in the table of smaller dimension.[56] On the other hand, if the relevant homogeneity subcomponents are not negligible, the total association in the $I \times J$ table cannot be described simply in terms of the association in the table of smaller dimension.

The general perspective described above provides justification for the replacement of the 8×8 table (table 1A) by the 7×7 table (table 1C), and it can be applied further, using the homogeneity and structural criteria, to replace in turn the 7×7 table (table 1C) by a table of still smaller dimension. To save space, we leave this exercise for the interested reader.[57]

In the present section, we have shown how application of the homogeneity criteria and structural criteria can be combined in the analysis of the association in an $I \times J$ cross-classification. In some situations, application of the two kinds of criteria will lead to the same general conclusion; but in other situations, this will not be the case. We shall now briefly examine the possible conflict that can arise in situations in which the two kinds of criteria may not lead to the same general conclusion.

HOMOGENEITY CRITERIA VERSUS STRUCTURAL CRITERIA

When the general perspective described in the preceding section is applied in the analysis of the association in, say, an 8×8 cross-classification, we noted that the usual χ^2 statistic (with $7 \times 7 = 49$ df) for testing the hy-

[54] The χ^2 statistic applied to table 2C had 11 df (see the bottom line in tables 5A and B); but it would have had $11 + 2 = 13$ df if the entries in cells (4,4) and (5,5) of table 1A had not been deleted.

[55] The χ^2 statistic for testing the usual hypothesis of null association in the corresponding 7×7 table has $6 \times 6 = 36$ df; but the corresponding χ^2 in table 7C had $36 - 7 = 29$ df because the seven entries on the main diagonal of table 1C were deleted there.

[56] With respect to the replacement of table 1A by table 1C, the homogeneity subcomponent pertaining to occupational inheritance in cells (4,4) and (5,5) in table 1A needs to be taken into account. For table 1A with the entries deleted on its main diagonal and in cells (4,5) and (5,4), the application of the homogeneity criteria in the preceding section (see, e.g., n. 33) indicates that this table can be replaced by table 1C with the entries deleted on its main diagonal; none of the homogeneity subcomponents need to be taken into account in this case. (The case in which the entries on the main diagonal of table 1A are deleted but those in cells [4,5] and [5,4] are not will be discussed further in a later section.)

[57] For related material, see, e.g., Goodman (1968, 1979*b*) and Hauser (1979).

pothesis of null association in the cross-classification can be partitioned into a homogeneity component (with 13 df) and a component related to the structure of the association in a corresponding 7×7 table (with $6 \times 6 = 36$ df). The first component pertains to the association in a certain kind of subtable of the 8×8 table,[58] and the second component pertains to the association in a corresponding 7×7 table. To focus our attention now on the difference between these two components, let us consider two situations that are, in a certain sense, extreme opposites: (I) the situation in which the first component is negligible, and (II) the situation in which the second component is negligible.

In situation (I), the association in the 8×8 table can be described solely in terms of the association in the corresponding 7×7 table, since the association pertaining to the first component is negligible. In situation (II), the association in the 8×8 table can be described solely in terms of the association in the subtable of the 8×8 table,[59] since the association pertaining to the second component is negligible. In situation (I), attention can be focused on the analysis of the association in the corresponding 7×7 table; and in situation (II), attention can be focused on the analysis of the association in the subtable of the 8×8 table. Thus, in situation (I), the homogeneity test is passed by the subtable of the 8×8 table, but the structural criteria may or may not be satisfied in the analysis of the association in the corresponding 7×7 table; and in situation (II), an extreme test based on the structural criteria is passed by the corresponding 7×7 table,[60] but the homogeneity criteria may or may not be satisfied in the analysis of the association in the subtable of the 8×8 table.[61]

In some contexts, we may wish to combine categories in the 8×8 table in order to obtain a situation similar to situation (I); in other contexts, we may wish to obtain a situation similar to situation (II). In the former situation, the absence of association in the subtable of the 8×8 table and the kind of association present in the corresponding 7×7 table may be of

[58] The subtable would be similar in form to, say, table 2C when the entries in cells (4,4) and (5,5) are not deleted. (See related comment in n. 54.)

[59] See n. 58.

[60] The test based upon the null association model is passed by the 7×7 table. Since the null association test is equivalent to a test of homogeneity applied to the 7×7 table, we see that the 7×7 table would pass a "homogeneity test". (For expository purposes, we use the term "homogeneity" in this article only in the analysis of the subtable of the 8×8 table [not in the analysis of the corresponding 7×7 table], but we need not limit usage of the term in this way.)

[61] The association in the subtable of the 8×8 table can be analyzed using homogeneity criteria, but in some cases it may be preferable to apply association models (and/or other kinds of models) in the analysis of the subtable. In such cases we could apply "structural criteria" to the subtable. (For expository purposes, we use the term "structural criteria" in this article only in the analysis of the corresponding 7×7 table [not in the analysis of the subtable of the 8×8 table], but we need not limit usage of the term in this way.)

substantive interest; in the latter situation, the absence of association in the corresponding 7×7 table and the kind of association present in the subtable of the 8×8 table may be of substantive interest.

The analysis of the association in the 8×8 cross-classification is simplified by the absence of association in the subtable of the 8×8 table or in the corresponding 7×7 table; and the absence of either of these two forms of association may be of substantive interest. On the other hand, when these two forms of association are present, the simplification noted above cannot be achieved. In this case, some simplification of the analysis may still be possible if either of the two forms of association can be described in relatively simple terms (e.g., in terms of the uniform association model or one of the other association models considered here); and this description of either of the two forms of association may be of substantive interest.

The foregoing comment leads now to the following more general version of an earlier statement in this section: in some contexts, categories in the 8×8 table would be combined in order to obtain a situation analogous to situation (I), where now the association in the subtable of the 8×8 table has some specified property; and in other contexts, categories in the 8×8 table would be combined in order to obtain a situation analogous to situation (II), where now the association in the corresponding 7×7 table has some specified property.

The results described in this section were expressed in terms of the 8×8 table, but they could have been expressed more generally in terms of the $I \times J$ table. We did not consider in this section the situation in which certain entries in the cross-classification are deleted, but the results presented here can be extended to that situation. The results presented in the earlier sections for the situation in which certain entries in the cross-classification are deleted can contribute to the development presented in the present section.

ASSOCIATION IN THE $I \times J$ CROSS-CLASSIFICATION TABLE AND IN THE TABLE OF SMALLER DIMENSION

As we noted earlier, certain kinds of hypotheses about the association in the $I \times J$ cross-classification can be analyzed by applying (*a*) homogeneity criteria to a relevant subset of the data in the $I \times J$ table, and (*b*) structural criteria to a corresponding table of smaller dimension. We shall now discuss further models for the $I \times J$ table that constrain parameters in such a way as to take account of the hypothesized homogeneity in the relevant subset of the $I \times J$ table while conveying the hypothesized structure of association as it would appear in the table of smaller dimension.

For the sake of simplicity, we shall first consider again the model of null association in the 8×8 table (table 1A) with the entries deleted on the

main diagonal. This model states that the expected frequencies F_{ij} can be described as follows:[62]

$$F_{ij} = \alpha_i \beta_j v_{ij} , \tag{19}$$

where

$$v_{ij} = \begin{cases} v_i , & \text{for } i = j , \\ 1 , & \text{otherwise} , \end{cases} \tag{20}$$

and where α_i, β_j, and v_{ij} are unspecified positive constants, with the v_{ij} satisfying condition (20). This model can be tested using the χ^2 value on line 1 of table 7A. Next consider table 1A with the entries deleted on the main diagonal and in cells (4,5) and (5,4). The corresponding model states that the expected frequencies F_{ij} can be described by (19) with the v_{ij} satisfying the following condition:

$$v_{ij} = \begin{cases} v_i , & \text{for } i = j , \\ v_{45} , & \text{for } i = 4 , j = 5 , \\ v_{54} , & \text{for } i = 5 , j = 4 , \\ 1 , & \text{otherwise} . \end{cases} \tag{21}$$

This model can be tested using the same general method that was used in the calculation of the χ^2 value on line 1 of table 7A (but with the entries in cells [4,5] and [5,4] included now in the deleted set). On the other hand, since this model can be partitioned into (*a*) a component pertaining to homogeneity in a relevant subset of the 8×8 table (see lines 1 and 2 of table 5B), and (*b*) a component pertaining to the structure of association in a corresponding table of smaller dimension (see line 1 of table 7C), the χ^2 value for testing the model will be equal to the sum of the corresponding component χ^2 values:[63]

$$7.81 + 437.43 = 445.24 , \tag{22}$$

with $10 + 29 = 39$ df.

The model considered above includes two more parameters (v_{45} and v_{54}) than the model on line 1 of table 7A.[64] Thus, the number of degrees of freedom (namely, 39) for testing the former model was two less than the corresponding number (namely, 41) for testing the latter model. We shall consider next a model that includes only one more parameter than the model on line 1 of table 7A.

Let us consider now model (19) with the parameters v_{ij} satisfying condition (21) and with the following additional condition also satisfied:

$$v_{45} = v_{54} = v , \tag{23}$$

[62] See, e.g., Goodman 1968, 1979*b*.

[63] See, e.g., n. 33.

[64] Cf. (21) with (20). The introduction of the two parameters v_{45} and v_{54} in (21) led to the deletion of the entries in cells (4,5) and (5,4) in the homogeneity test applied to table 2C and to the deletion of these two entries (together with the entries in cells [4,4] and [5,5]) in the corresponding diagonal cell, which was formed from the four cells, in table 1C.

where v is an unspecified positive constant. In this case, the v_{ij} will satisfy the following condition:

$$v_{ij} = \begin{cases} v_i , & \text{for } i = j , \\ v , & \text{for } (i,j) = (4,5) \text{ and } (5,4) , \\ 1 , & \text{otherwise} . \end{cases} \tag{24}$$

This model is included within the general class of multiplicative models,[65] and the general methods developed for the analysis of these models can be applied here. On the other hand, since this model can be partitioned into (*a*) a component pertaining to homogeneity (see lines 1, 2, and 3 of table 5B), and (*b*) a component pertaining to structure (see line 1 of table 7C), the χ^2 value for testing the model will be equal to the sum of the corresponding component χ^2 values:

$$7.87 + 437.43 = 445.30 , \tag{25}$$

with $11 + 29 = 40$ df.

The model considered above includes one less parameter than model (19) with the v_{ij} satisfying (21), and it includes one more parameter than the model on line 1 of table 7A.[66] Thus, in testing the model considered above, the corresponding number of degrees of freedom (namely, 40) was one more than the corresponding number (namely, 39) for testing model (19) with the v_{ij} satisfying (21), and it was one less than the corresponding number (namely, 41) for testing the model on line 1 of Table 7A.

The models considered so far in this section were quasi-independence models (see [19], [20], and [21]) and a simple modification of a quasi-independence model (see [19], [23], and [24]). We shall now consider the corresponding association models that include row effects and column effects on the association. These models can be obtained from model (19) by introducing into (19) additional parameters that pertain to the row effects and column effects on the association:

$$F_{ij} = \alpha_i \beta_j v_{ij} e^{\mu_i \nu_j} , \tag{26}$$

where μ_i and ν_j are the additional parameters.[67]

[65] See, e.g., Goodman 1972. This particular model can serve as a simple example of a multiplicative model, and the quasi-independence models described above by (19) and (20) and by (19) and (21) can serve as still simpler examples.

[66] Cf. (24) with (21), and then (24) with (20). In contrast to the introduction of v_{45} and v_{54} in (21) (see n. 64), the introduction of v in (24) does *not* affect the homogeneity test applied to table 2C (it does *not* lead to the deletion of the entries in cells [4,5] and [5,4] in this homogeneity test); but it does lead to the deletion of the sum of the entries in these two cells (together with the entries in cells [4,4] and [5,5]) in the corresponding diagonal cell, which was formed from the four cells, in table 1C.

[67] See, e.g., Goodman 1979*b*, 1981*b*. When the v_{ij} in (26) are all equal to 1, model (26) is equivalent to model (12).

When the v_{ij} in (26) satisfy condition (20), the entries on the main diagonal are deleted in the cross-classification (table 1A) analyzed using the association model that includes row effects and column effects on the association. In this case, the model can be tested using the χ^2 value on line 6 of table 7A. We shall next compare this model with a related model considered earlier: the model tested using the χ^2 value (17).

As noted earlier, the χ^2 value (17) serves to test the hypothesis that the association model (26) holds true and that the odds-ratios pertaining to the cells in rows 4 and 5, and to the cells in columns 4 and 5, are equal to one, in the case where the entries are deleted on the main diagonal and in cells (4,5) and (5,4).[68] Under model (26), with the particular set of deleted entries noted above, the v_{ij} in (26) will satisfy condition (21); and when the particular set of odds-ratios noted above are equal to one, the parameters μ_i and ν_j in (26) will satisfy the following two constraints:[69]

$$\mu_4 = \mu_5 , \tag{27}$$

and

$$\nu_4 = \nu_5 . \tag{28}$$

Thus, the χ^2 value (17) serves to test the hypothesis that model (26) holds true with the v_{ij} satisfying condition (21) and the μ_i and ν_j satisfying the two constraints (27) and (28).

The model considered above includes two more v_{ij} parameters (v_{45} and v_{54}) than the model on line 6 of table 7A (model [26] with the v_{ij} satisfying [20]),[70] and the parameters in the former model are subject to the two additional constraints (27) and (28). The two additional parameters in the former model would decrease the number of degrees of freedom by two, and the two additional constraints in the former model would increase the number of degrees of freedom by two. Thus, the number of degrees of freedom in the former model is equal to the number (namely, 28) in the latter model. We shall consider next a model that includes only one more parameter than the model on line 6 of table 7A, and thus one less parameter than the model considered above.

Let us consider now model (26) with the parameters v_{ij} satisfying condition (24) and with the μ_i and ν_j satisfying (27) and (28). This model modifies the model tested using the χ^2 value (17) by replacing condition (21) with (24). The same kind of replacement was made earlier when the model tested using the χ^2 value (22) was modified to obtain the model tested using the χ^2 value (25). In a similar way, instead of using the χ^2 value (17), the model

[68] See, e.g., n. 48.

[69] See, e.g., Goodman 1979*b*, 1981*b*. Note that conditions (27) and (28) are equivalent to (13) and (16), respectively, with $i' = 4$ and $j^* = 4$.

[70] Cf. (21) with (20).

considered now can be tested using the sum of its corresponding component χ^2 values:[71]

$$7.87 + 22.83 = 30.70, \tag{29}$$

with $11 + 18 = 29$ df.

The χ^2 values (17) and (29) will be equal to the corresponding χ^2 values obtained by applying the general methods developed for the analysis of association models to the corresponding models considered here.[72] These general methods can also be applied when these models are modified by the replacement of conditions (21) and (24) by condition (20). In this case, we obtain a χ^2 value of

$$30.71, \tag{30}$$

with 30 df.[73]

The three models tested using the χ^2 values (17), (29), and (30) are modifications of the model on line 6 of table 7A. Similar kinds of modifications can also be made in the models on lines 2 through 5 in table 7A.[74] For example, by modifying the model on line 5 of table 7A, the three modified models thus obtained can be tested using the following χ^2 values:

$$7.81 + 24.72 = 32.53, \tag{31}$$

$$7.87 + 24.72 = 32.59, \tag{32}$$

[71] Cf. (22) and (25) with (17) and (29), respectively.

[72] The iterative procedures described, e.g., in Goodman (1979*b*) can be extended in a straightforward way for the analysis of models in which restrictions of the kind exemplified by (27) and (28) are imposed upon the μ_i and ν_j parameters, and restrictions of the kind exemplified by (23) are imposed upon the v_{ij}. Similar kinds of extensions were made, e.g., in Goodman (1972, 1974), where these kinds of restrictions were imposed in the models considered there. With respect to, say, restrictions (27) and (28) imposed in model (26) for the 8×8 table, the extension would consist of (1) modifying the eight equations in (5.6a) in Goodman (1979*b*) by deleting the equations for $i = 4$ and 5, replacing the two deleted equations by a single equation that equates the sum of the left sides of the two deleted equations and the sum of the right sides of these two equations, (2) modifying the eight equations in (5.6b) in a similar way, and (3) making a related kind of modification in the iterative procedure for estimating μ_i and ν_j described in appendix B of Goodman (1979*b*).

[73] This model is tested with one more degree of freedom than is the model tested using (29) because the latter model includes one more v_{ij} parameter (namely, v) than does the former model (cf. [24] with [20]). The former model is tested with two more degrees of freedom than is the model on line 6 of table 7A because the parameters in the former model are subject to the two additional restrictions (27) and (28).

[74] Modifications of the model on line 1 of table 7A were considered earlier in this section, and they could be tested using the χ^2 values (22) and (25). The models on lines 1 through 4 of table 7A, and the modified forms of these models, are all examples of loglinear models, and the general methods developed for loglinear models can be applied here; but the models on lines 5 and 6 of table 7A, and the modified forms of those models, are not loglinear (see, e.g., Goodman 1979*b*).

and

$$32.59,^{75} \tag{33}$$

with $10 + 23 = 33$ df, $11 + 23 = 34$ df, and 35 df, respectively.[76]

SOME FURTHER RESULTS ON THE HOMOGENEITY CRITERIA

As we noted earlier, application of the homogeneity criteria could be based upon the total χ^2 value obtained in testing homogeneity in the relevant subtable of the $I \times J$ cross-classification; but in some contexts this test might be replaced by a weaker test obtained by deleting some specified component in the total χ^2.[77] With respect to the 8×8 table (table 1A), to determine whether, say, occupational categories 4 and 5 are homogeneous, the relevant total χ^2 value would be tested with 13 df; but we delete two components when the occupational inheritance entries in cells (4,4) and (5,5) are deleted, thereby obtaining a total χ^2 value that would be tested with 11 df.[78] None of the components of the latter χ^2 value (with 11 df), and none of the components of the former χ^2 value (with 13 df), pertain to the term "internal homogeneity" as defined and applied by Breiger (1981).[79] On the other hand, if the entries on the main diagonal had not been deleted in Breiger's application of this term, one of the components (with 1 df) of the former χ^2 value (with 13 df) would have pertained to this term.[80] In the present context, the term "internal homogeneity" would pertain to one of the components (with 1 df) in the homogeneity criteria as defined herein, and the other components (with 12 df in total) in the homogeneity criteria would not pertain to this term.

A simple example will further illustrate the relationship noted above.

[75] The χ^2 values (30) and (33) were obtained by applying the general methods mentioned earlier (see, e.g., n. 72). The model tested using (30) is tested with one more degree of freedom than is the model tested using (29), and similarly the model tested using (33) is tested with one more degree of freedom than is the model tested using (32) (see, e.g., n. 73). The χ^2 value (30) is larger than the corresponding value (29), and similarly the χ^2 value (33) would be larger than the corresponding value (32), but to the order of accuracy reported in this article the two latter values turn out to be equal to each other.

[76] The model using (33) is tested with one more degree of freedom than is the model on line (5) of table 7A because the parameters in the former model are subject to one more restriction than are the parameters in the latter model. (Since the row and column effects are homogeneous in the model on line 5 of table 7A, the restrictions [27] and [28] are equivalent under this model; see, e.g., Goodman [1979*b*].)

[77] See, e.g., n. 24. Also, compare the homogeneity component in (17), (22), and (31) with the corresponding component (29), (25), and (32), respectively.

[78] See the bottom line in tables 5A and B, and also n. 54.

[79] See the introductory comments in the section on homogeneity criteria earlier in the present article.

[80] See nn. 6 and 7.

Consider the 4 × 4 cross-classification in table 8A with the homogeneity criteria applied to, say, categories 1 and 2. In this case, table 8B is the subtable of table 8A used in applying the homogeneity criteria. Table 8B can be partitioned into tables 9A through E when the entries on the main

TABLE 8

TABLES USED IN THE ANALYSIS OF THE HOMOGENEITY CRITERIA

A. A 4 × 4 CROSS-CLASSIFICATION

	Column Category			
Row Category	1	2	3	4
1.....	a	b	c	d
2.....	e	f	g	h
3.....	i	j	k	l
4.....	m	n	o	p

B. SUBTABLE OF TABLE 8A USED IN APPLYING HOMOGENEITY CRITERIA TO CATEGORIES 1 AND 2

	Column Category			
Row Category	1	2	3	4
1.....	a	b	c	d
2.....	e	f	g	h
3.....	ı	j	...	...
4.....	m	n	...	...

C. A 3 × 3 CROSS-CLASSIFICATION OBTAINED BY COMBINING CATEGORIES 1 AND 2 IN TABLE 8A

	Column Category		
Row Category	1+2	3	4
1+2.....	a+b+e+f	c+g	d+h
3.....	i+j	k	l
4.....	m+n	o	p

D. SUBTABLE OF TABLE 8C USED IN APPLYING HOMOGENEITY CRITERIA TO CATEGORIES 3 AND 4

	Column Category		
Row Category	1+2	3	4
1+2.....	...	c+g	d+h
3.....	i+j	k	l
4.....	m+n	o	p

TABLE 9

TABLES FORMED FROM TABLE 8B USED IN APPLYING HOMOGENEITY CRITERIA

A. Applied to Rows 1 and 2 in Cols. 1 and 2 and to Cols. 1 and 2 in Rows 1 and 2

a	b
e	f

B. Applied to Rows 1 and 2 in Cols. 3 and 4

c	d
g	h

C. Applied to Cols. 1 and 2 in Rows 3 and 4

i	j
m	n

D. Applied to Rows 1 and 2 in Cols. 1+2 and 3+4

a+b	c+d
e+f	g+h

E. Applied to Cols. 1 and 2 in Rows 1+2 and 3+4

a+e	b+f
i+m	j+n

F. Applied to Rows 1 and 2 in Cols. 1, 2, and 3+4 and to Cols. 1 and 2 in Rows 1, 2, and 3+4

...	b	c+d
e	...	g+h
i+m	j+n	...

diagonal of table 8B are not deleted;[81] and it can be partitioned into tables 9B, C, and F when the entries on the main diagonal of table 8B are deleted.[82] When the main diagonal is not deleted, there will be five components each with 1 df; and when the main diagonal is deleted, there will be three components each with 1 df. In the former case, the term "internal homogeneity"

[81] The general method for partitioning the χ^2 statistic illustrated in the simple example in n. 14 can be applied successively to table 8B to obtain the components presented here both in the case when the entries on the main diagonal are not deleted and in the case when they are deleted. The usual mathematical symbols used to denote the entries in a cross-classification table were replaced in tables 8 and 9 by a somewhat simpler notation to facilitate exposition here. The 16 lowercase letters, a–p, denote the 16 entries in the 4×4 cross-classification table 8A. (In particular, the lowercase letters i and j denote here particular entries in this table. These symbols as used here should not be confused with the subscripts i and j used to denote the ith row category and jth column category in the $I \times J$ cross-classification. A similar remark applies also to the lowercase letters k and l as used here in contrast to the subscripts k and l used later in this article to denote the kth row class and lth column class in a $K \times L$ cross-classification.)

[82] Table 9F is obtained when the homogeneity criteria are applied to entries (1,2) and (2,1) in table 8B with the entries deleted on the main diagonal. Note that table 9F can

would be limited to one of the components (the component for table 9A), and the other four components in the homogeneity criteria would not pertain to this term. In the latter case, none of the three components would pertain to this term.

Consider now table 8A with the homogeneity criteria applied to categories 1 and 2 in the situation where categories 3 and 4 may also be combined. In this case, with respect to the homogeneity of categories 1 and 2, table 8B is still the relevant subtable of table 8A, and this subtable can be partitioned as described above. However, in this situation in which categories 3 and 4 may also be combined, the term "internal homogeneity" would pertain to a somewhat different set of components than in the situation considered in the preceding paragraph. In the situation in which categories 3 and 4 may also be combined, the term "internal homogeneity" would pertain to the components for tables 9B and C, as well as to the component for table 9A. Thus, when the main diagonal is not deleted, this term would pertain to three of the components; and when the main diagonal is deleted, the term would pertain to two of the components.

By applying to table 8A the general perspective (which was described in an earlier section) pertaining to the partitioning of the total association in a cross-classification, we see that the usual χ^2 statistic for testing the hypothesis of null association in the 4×4 cross-classification (with $3 \times 3 = 9$ df) can be partitioned into a homogeneity component (with 5 df) to test the homogeneity of, say, categories 1 and 2,[83] and a component (with $2 \times 2 = 4$ df) to test the association in the 3×3 cross-classification (table 8C) obtained when the corresponding categories (categories 1 and 2) are combined. The homogeneity component can be calculated directly from table 8B (or from the corresponding tables 9A through E), or it can be calculated as the difference between the usual χ^2 value obtained in testing the hypothesis of null association in the 4×4 cross-classification (table 8A) and the corresponding χ^2 value obtained in testing this hypothesis in the 3×3 cross-classification (table 8C).

Having now applied the general perspective described in the earlier section to table 8A with the homogeneity component pertaining to categories 1 and 2, we shall next apply this perspective in the situation considered

be formed from the latter table by combining cols. 3 and 4 into a single column and combining rows 3 and 4 into a single row. When the entries on the main diagonal in table 8B are deleted, the homogeneity component for tables 9A, D, and E cannot be calculated, but the information in these three tables can be combined to provide the homogeneity component for table 9F. A similar kind of interpretation could have been given earlier for the homogeneity component for table 4C.

[83] As noted in the earlier section, in this case the homogeneity criteria are applied to table 8B, and this table can be partitioned into tables 9A through E when the entries on the main diagonal of table 8A are not deleted. For the sake of simplicity, we focus attention now and in the following discussion on the situation in which the main diagonal in table 8A is not deleted.

earlier in the present section in which categories 1 and 2 may be combined and categories 3 and 4 may also be combined in table 8A. When categories 1 and 2 are combined, this perspective can be applied to the corresponding 3×3 cross-classification (table 8C) to test the homogeneity of categories 3 and 4. In this case, table 8D is the subtable of table 8C used in applying the homogeneity criteria to categories 3 and 4.[84] The usual χ^2 statistic for testing the hypothesis of null association in the 3×3 cross-classification table 8C (with $2 \times 2 = 4$ df) can be partitioned into a homogeneity component (with 3 df) to test the homogeneity in table 8D, and a component (with 1 df) to test the association in the 2×2 cross-classification obtained when the corresponding categories (categories 3 and 4) are combined in table 8C. Thus, the usual χ^2 statistic for testing the hypothesis of null association in the 4×4 cross-classification table 8A (with $3 \times 3 = 9$ df) can be partitioned into a homogeneity component (with $5 + 3 = 8$ df) to test the homogeneity of categories 1 and 2 and the homogeneity of categories 3 and 4,[85] and a component (with 1 df) to test the association in the 2×2 cross-classification obtained when the corresponding categories are combined. The homogeneity component can be calculated directly from tables 8B and D (or from the corresponding partitioning of these tables), or it can be calculated as the difference between the usual χ^2 value obtained in testing the hypothesis of null association in the 4×4 cross-classification (table 8A) and the corresponding χ^2 value obtained in testing this hypothesis in the 2×2 cross-classification.

In calculating the homogeneity component directly from table 8B, the pattern of deleted cells in this table is such that the expected frequencies under the homogeneity model can be estimated by explicit formula without iteration.[86] This comment on the estimation of the expected frequencies also holds true in calculating the homogeneity component directly from table 8D. In testing the homogeneity of categories 1 and 2 and the homogeneity of categories 3 and 4, the corresponding total homogeneity component (with 8 df) can be partitioned into a subcomponent (with 5 df) calculated directly from table 8B and a subcomponent (with 3 df) calculated directly from table 8D, with the expected frequencies estimated by explicit formula in each subcomponent. Alternatively, this total homogeneity component can be calculated directly (not using the partitioning noted above) with the expected frequencies also estimated by explicit formula under this homoge-

[84] The χ^2 value obtained in testing homogeneity in table 8D can be partitioned into three components each with 1 df (see, e.g., Goodman 1968).

[85] The homogeneity component here is the sum of the corresponding components obtained from tables 8B and D.

[86] See, e.g., Goodman 1968. As noted in the last sentence of n. 83, we are concerned here with the situation in which the main diagonal in table 8A is not deleted. In this situation, when testing the homogeneity of categories 1 and 2, the pattern of deleted cells is as noted in table 8B.

neity model. We shall now describe this explicit formula, first introducing some additional notation and terminology that will be used in the description.

In the 4×4 cross-classification of row categories and column categories (table 8A), when categories 1 and 2 are combined the 3×3 table thus obtained (table 8C) can be viewed as a 3×3 cross-classification of row "classes" and column "classes." Similarly, when categories 1 and 2 are combined and categories 3 and 4 are combined in the 4×4 cross-classification, the 2×2 table thus obtained can be viewed as a 2×2 cross-classification of row "classes" and column "classes." In the former case, the four categories were replaced by three classes; in the latter case, the four categories were replaced by two classes. More generally, in the $I \times J$ cross-classification of I row categories and J column categories, the I row categories can be partitioned into K row classes (where each row class combines the categories in a given subset of the row categories), and the J column categories can be partitioned into L column classes (where each column class combines the categories in a given subset of the column categories). From the $I \times J$ cross-classification of row categories and column categories, we thus obtain a $K \times L$ cross-classification of K row classes and L column classes (see, e.g., tables 10A and B).

For the $I \times J$ cross-classification, let f_{ij} denote the observed frequency in the ith row category and jth column category ($i = 1, 2, \ldots, I; j = 1, 2, \ldots, J$); and for the $K \times L$ cross-classification, let f^*_{kl} denote the corresponding frequency in the kth row class and lth row class ($k = 1, 2, \ldots, K; l = 1, 2, \ldots, L$). Each f^*_{kl} is a sum of a given set of f_{ij}, and each f_{ij} is a summand in a given f^*_{kl} (see, e.g., table 10B). Let $f_{ij,kl}$ denote the observed frequency in the ith row category and jth column category, and in the kth row class and lth column class (see, e.g., table 10C). The additional subscript k simply indicates that the ith row category is included in the kth row class, and the additional subscript l indicates similarly that the jth column category is included in the lth column class. Let $F_{ij,kl}$ denote the expected value of $f_{ij,kl}$ under some model.

Using the same general methods applied in, for example, Goodman (1968, 1970) to obtain explicit formulae for the maximum-likelihood estimate of the expected frequency (in cases where iteration is not necessary), we see that, under the homogeneity model considered in the present article, the $F_{ij,kl}$ can be estimated by the following formula:[87]

$$\hat{F}_{ij,kl} = f_{i\cdot} f_{\cdot j} f^*_{kl} / (f^*_{k\cdot} f^*_{\cdot l}) , \qquad (34)$$

[87] The homogeneity model considered in the present article will be discussed further later in this section both for the situation in which none of the cells in the $I \times J$ cross-classification are deleted and for the situation in which some of the cells are deleted. Formula (34), and the corresponding formulae (39) and (40), pertain to the former situation. Formula (34) for the former situation is also presented in Béland (1978).

where

$$f_{i\cdot} = \sum_{j=1}^{J} f_{ij}\,, \quad f_{\cdot j} = \sum_{i=1}^{I} f_{ij}\,,$$
$$f^*_{k\cdot} = \sum_{l=1}^{L} f^*_{kl}\,, \quad f^*_{\cdot l} = \sum_{k=1}^{K} f^*_{kl}\,. \tag{35}$$

With this explicit formula for $\hat{F}_{ij,kl}$, we can see how $\hat{F}_{ij,kl}$ is related to the usual estimate $\hat{F}_{ij}$ of the expected value of f_{ij} under the hypothesis of null association in the $I \times J$ cross-classification, and to the estimate $\hat{F}^*_{kl}$ of the expected value of f^*_{kl} under the hypothesis of null association in the $K \times L$ cross-classification. Under the hypothesis of null association in the $I \times J$ cross-classification, the expected value of f_{ij} can be estimated simply by the usual formula

$$\hat{F}_{ij} = f_{i\cdot} f_{\cdot j}/N\,, \tag{36}$$

TABLE 10

TABLES USED IN APPLYING HOMOGENEITY CRITERIA TO CATEGORIES 1 AND 2 AND TO CATEGORIES 3 AND 4

A. A 4 × 4 CROSS-CLASSIFICATION OF CATEGORIES

	Column Category			
Row Category	1	2	3	4
1	f_{11}	f_{12}	f_{13}	f_{14}
2	f_{21}	f_{22}	f_{23}	f_{24}
3	f_{31}	f_{32}	f_{33}	f_{34}
4	f_{41}	f_{42}	f_{43}	f_{44}

B. A 2 × 2 CROSS-CLASSIFICATION OF CLASSES

	Column Class	
Row Class	1	2
1	$f^*_{11}=f_{11}+f_{12}+f_{21}+f_{22}$	$f^*_{12}=f_{13}+f_{14}+f_{23}+f_{24}$
2	$f^*_{21}=f_{31}+f_{32}+f_{41}+f_{42}$	$f^*_{22}=f_{33}+f_{34}+f_{43}+f_{44}$

C. A CROSS-CLASSIFICATION OF CATEGORIES WITHIN A CROSS-CLASSIFICATION OF CLASSES

	Column Class	
Row Class	1	2
1	$\begin{matrix} f_{11,11} & f_{12,11} \\ f_{21,11} & f_{22,11} \end{matrix}$	$\begin{matrix} f_{13,12} & f_{14,12} \\ f_{23,12} & f_{24,12} \end{matrix}$
2	$\begin{matrix} f_{31,21} & f_{32,21} \\ f_{41,21} & f_{42,21} \end{matrix}$	$\begin{matrix} f_{33,22} & f_{34,22} \\ f_{43,22} & f_{44,22} \end{matrix}$

where

$$N = \sum_{i=1}^{I} \sum_{j=1}^{J} f_{ij} ; \tag{37}$$

and under the hypothesis of null association in the $K \times L$ cross-classification, the expected value of f^*_{kl} can be estimated by

$$\hat{F}^*_{kl} = f^*_{k\cdot} f^*_{\cdot l} / N . \tag{38}$$

From (34), (36), and (38), we see that

$$\hat{F}_{ij,kl} = \hat{F}_{ij} f^*_{kl} / \hat{F}^*_{kl} , \tag{39}$$

and

$$\hat{F}_{ij} = \hat{F}_{ij,kl} / (f^*_{kl} / \hat{F}^*_{kl}) . \tag{40}$$

With the χ^2 statistic for testing the hypothesis of null association in the $I \times J$ cross-classification, the observed frequency f_{ij} is compared with the estimated expected frequency $\hat{F}_{ij}$ given simply by (36). If the $\hat{F}_{ij}$ in the usual formula for the likelihood-ratio χ^2 statistic is replaced by the equivalent term on the right side of (40), this χ^2 statistic can then be partitioned directly into a component comparing the observed frequency $f_{ij,kl}$ with $\hat{F}_{ij,kl}$, and a component comparing f^*_{kl} with $\hat{F}^*_{kl}$. The first component can be used to test the homogeneity model, and the second component can be used to test the hypothesis of null association in the $K \times L$ cross-classification. Thus the likelihood-ratio χ^2 statistic for testing the hypothesis of null association in the $I \times J$ cross-classification can be partitioned into a component to test the homogeneity model, and a component to test the hypothesis of null association in the $K \times L$ cross-classification. The homogeneity component can be calculated directly comparing the observed frequency $f_{ij,kl}$ with $\hat{F}_{ij,kl}$, or it can be calculated as the difference between the χ^2 value obtained in testing the hypothesis of null association in the $I \times J$ cross-classification and the corresponding χ^2 value obtained in testing this hypothesis in the $K \times L$ cross-classification.[88]

From formula (34) we see that $\hat{F}_{ij,kl}$ is a product of three factors: (1) a factor pertaining to the ith row category (namely, $f_{i\cdot}$), (2) a factor pertaining to the jth column category (namely, $f_{\cdot j}$), and (3) a factor pertaining to the kth row class and lth column class (namely, $f^*_{kl} / [f^*_{k\cdot} f^*_{\cdot l}]$). This is a consequence of the fact that the homogeneity model considered in this article, when expressed in the terminology of the present section, states that the expected frequency $F_{ij,kl}$ is a product of a row category factor, a

[88] We also find that the number of degrees of freedom for testing the homogeneity model can be calculated as the corresponding difference $(I - 1)(J - 1) - (K - 1)(L - 1)$. This formula pertains to the situation in which none of the cells in the $I \times J$ cross-classification are deleted. (For the situation in which some of the cells are deleted, see nn. 89 and 90.) The above difference formula for the former situation is also presented in Béland (1978) and Béland and Fortier (1981).

column category factor, and a factor pertaining to the row class and column class:

$$F_{ij,kl} = \alpha_i \beta_j \gamma_{kl} . \tag{41}$$

Model (41) is included within the general class of multiplicative models introduced in Goodman (1972), and the general techniques presented in that article can be applied directly to this model. (This model was also considered in Breiger [1981], but he preferred a different one.) When none of the cells in the $I \times J$ cross-classification are deleted, formula (34) can be used to calculate $\hat{F}_{ij,kl}$ under this model; but this formula cannot be used when some of the cells are deleted (e.g., the cells on the main diagonal). When some of the cells are deleted, the $\hat{F}_{ij,kl}$ can be calculated using the general iterative procedure described in Goodman (1972).[89] In this case, the results presented in the present section pertaining to the partitioning of χ^2 also need to be modified.[90]

Before closing this section, we mention again that the application of the homogeneity criteria could be based upon the total χ^2 value obtained in testing homogeneity, but in some contexts this test might be replaced by a weaker test obtained by deleting some specified components in the total χ^2.[91] We also remind the reader that, in the earlier section on homogeneity criteria versus structural criteria, when considering whether the I row categories and J column categories would be replaced by K row classes and L column classes, respectively, we noted that in some contexts the replacement would be made in order to obtain a situation in which the association in the $I \times J$ table (or in an appropriate subtable of the $I \times J$ table) has some specified property (e.g., the association would fit the homogeneity

[89] When some of the cells are deleted, the number of degrees of freedom for testing the homogeneity model can be calculated by modifying the formula in n. 88. The term $(I-1)(J-1)$ in this formula would be reduced to take account of the deleted cells, subtracting from this term the number of cells deleted in the case in which the pattern of the deleted cells in the $I \times J$ cross-classification table does not make the table separable (see, e.g., Goodman [1968] for discussion of both the inseparable and separable case); and the term $(K-1)(L-1)$ in the formula in n. 88 would also be reduced to take account of the effect of the deleted cells on the dimensionality of the row $\times$ column interaction term γ_{kl} (for $k = 1, 2, \ldots, K$; $l = 1, 2, \ldots, L$) in model (41).

[90] The partitioning results presented in this section for the case in which none of the cells are deleted (see, e.g., formulae [39] and [40] and the paragraph directly following these formulae) can be extended directly to the case in which some of the cells are deleted, when row category i and column category j are in row class k and column class l, respectively, and the pattern of deleted cells is such that, if cell (i,j) is deleted in the $I \times J$ table, then all the other cells in this table that are in cell (k,l) in the $K \times L$ table are also deleted in the $I \times J$ table. (See, e.g., the discussion of the χ^2 value [22] in the earlier section.) When the pattern of deleted cells is as described above, the formula in n. 88 for the number of degrees of freedom can also be extended directly. When the pattern of deleted cells is not as described above, the partitioning results need to be modified further (see, e.g., the earlier discussion of the χ^2 value [25]); and the number of degrees of freedom can be calculated as in n. 89.

[91] See, e.g., nn. 24 and 77.

model considered in this article or a specified generalization of this model); and in other contexts, the $I \times J$ table would be replaced by a $K \times L$ table in order to obtain a situation in which the association in the $K \times L$ table has some specified property.

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Economic Segmentation, Worker Power, and Income Inequality[1]

Arne L. Kalleberg, Michael Wallace, and Robert P. Althauser
Indiana University

How economic segmentation generates income inequality constitutes a central question for theories of economic and social organization and of socioeconomic achievement. Previous research emphasizes two sources of the structural variation in income: (1) employers with large amounts of resources, for a variety of reasons, may find it in their interests to pay workers higher wages; and (2) some workers are able to acquire power against their employers as well as against other workers and can therefore extract higher earnings. In this paper, we investigate several issues involved in the links among economic segmentation, worker power, and income inequality. We argue that the structure of economic segmentation is multidimensional and reflects such distinct concepts as concentration, economic scale, state intervention in the market, capital intensity, and organization size. Worker power also is derived from diverse sources, such as union membership, occupational skill and licensing, class position, and tenure with an employer. We construct measures of these two sets of concepts and examine their relationships and effects on income with data from two national samples of individuals. We also relate economic segmentation to issues raised by the socioeconomic achievement literature; in particular, we find that the effect of education on income differs among firm and industrial contexts. Our analyses illustrate the utility of an economic segmentation approach for explaining structural sources of income inequality.

Recent sociological explanations of income inequality have turned to theories of economic segmentation in an effort to illustrate the organizational bases of stratification and to underscore the belief that socioeconomic achievements of individuals are conditioned by differences in the social and economic organization of production (e.g., Bibb and Form 1977; Beck, Horan, and Tolbert 1978; Hodson 1978; Wendt 1978; Kluegel 1978;

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Spilerman 1977; Tolbert, Horan, and Beck 1980; Wallace and Kalleberg 1981; Stolzenberg 1978; Althauser and Kalleberg 1981; Sørensen and Kalleberg 1981; Granovetter 1981). Empirical research on economic segmentation has often confounded two interrelated but conceptually distinct phenomena: the differentiation of firms and industries in production systems and the segmentation of the labor force which is in part derived from these organizational differences. In this paper, we use the term *economic segmentation* in a narrow sense to refer to those features of the economic organization of production among firms and industries which are relevant to the jobs and socioeconomic experiences of individual workers. By *worker power,* on the other hand, we mean the attributes gained by persons, individually and collectively, in the labor force which increase their ability to obtain valued rewards from work.

A useful theoretical framework for conceptualizing the structure and consequences of economic segmentation is dual economy theory (e.g., Averitt 1968; Galbraith 1973; Bluestone 1970; O'Connor 1973). Dual economy theory posits that, as a result of the dynamics of the capital accumulation process in U.S. industry, there has been a historical trend toward the development of two polarized economic sectors—monopoly and competitive. The theory also points out that these processes of economic segmentation fractionalize the labor force and labor market along occupational, class, and other dimensions (e.g., Edwards 1979). This perspective offers a starting point for understanding how individual differences in income are related to characteristics of employers and industrial organization. It further suggests a means for interpreting many forms of worker power.

Research based on the dual economy perspective has raised a number of questions that must be answered before a coherent structural theory of economic stratification can be developed. One issue concerns the conceptualization and measurement of economic segmentation. Some argue that the structure of economic segmentation can be represented by a single dimension of industrial differentiation, while others contend that a multidimensional model is more accurate. A second issue concerns the mechanisms by which firm and industrial differences affect the distribution of income. That some employers, because of comparative advantages in market power and economic resources, can afford to pay wages above competitive market levels is well known and documented. What needs to be explained is why such employers are motivated or constrained to pay higher wages and salaries to their workers.

In this paper, we argue that the structure of economic segmentation is multidimensional and must therefore be conceptualized and measured in terms of its distinct components: concentration, capital intensity, state-

sponsored production, establishment size, and economic scale. We also argue that worker power is important for understanding why employers' resources are translated into higher incomes for workers. Like economic segmentation, worker power is multidimensional. It is obtained from union membership, occupational licensing, occupationally based skill requirements, class position, and tenure with a specific employer. In this paper, we construct measures of these economic segmentation and worker power concepts and examine their relationships and effects on income with data from two national samples of the labor force. Our analyses provide empirical evidence on several issues raised by recent research on economic segmentation, issues to which we now turn.

ISSUES RAISED BY PREVIOUS RESEARCH

Conceptualization and Measurement of Economic Segmentation

Dual economy theory maintains that the historical trend toward economic segmentation has produced two major sectors in the private domain of the U.S. economy: a "monopoly" (or "core") sector composed of large, oligopolistic firms with a great deal of control over their product markets and the economy in general; and a "competitive" (or "periphery") sector that consists mostly of small, single-product firms with few extramarket resources and minimal control, economic or otherwise. These organizational differences, it is argued, have a substantial impact on employment conditions. Workers in the monopoly sector generally have higher earnings, more stable employment, and better nonwage benefits than their counterparts in the competitive sector (Hodson 1978). These tendencies toward labor market segmentation and the unequal distribution of job rewards are assumed to be inevitable consequences of the uneven development of U.S. capitalism in the 20th century (Edwards 1979).

From dual economy theory and the literature on economic segmentation, we can hypothesize that income differences are in part attributable to several key features of economic organization (for a discussion, see Wallace and Kalleberg [1981]). Firms in industries with high *concentration,* because of their greater market power and control over pricing mechanisms, pay employees more than firms in competitive industries (see, e.g., Dalton and Ford 1977; Garbarino 1950; Ross and Goldner 1950; Yordan 1961). Firms in industries which sell goods to the *state* have an assured market for their goods and can therefore pay workers higher wages (O'Connor 1973). Firms of large *size* are more likely than small firms to engage in long-term planning (Galbraith 1973) and to experience stable demand for their products and services; consequently, they have the resources to

afford higher wage rates (see, e.g., Masters 1969; Haworth and Rasmussen 1971; Freedman 1976; Stolzenberg 1978). Firms in *capital intensive* industries are more productive and efficient than firms with relatively high labor costs and are therefore able to pay high salaries (e.g., Averitt 1968; O'Connor 1973; Edwards 1979). Finally, firms in industries with high *economic scale* have the resources to pay workers high incomes. Economic scale (indexed by high assets, sales, and/or profits) partly derives from high concentration, government contracts, large firm size, and capital intensity and may help explain why these four concepts are related to income inequality.

Dual economy theory suggests that these attributes strongly affect income differences among workers. In contrast, neoclassical economic theory regards such features of economic organization as exogenous and thus relatively unimportant sources of economic inequality. For example, orthodox economists might argue that concentration or state purchases produce labor markets that are not competitive. But these are depicted as transitory aberrations in an otherwise competitive labor market. They also interpret these factors as reflections of differences in marginal productivity, an assumption which holds better for some of these concepts (e.g., capital intensity) than for others (e.g., concentration; see Gordon 1972).

Some recent sociological research in the dual economy tradition has combined these organizational and industrial differences into a single dimension. Industries are rank ordered according to "capacity for oligopolistic behavior" (e.g., Tolbert et al. 1980) or, from a Marxist perspective, according to "centralization and concentration of capital" (e.g., Hodson 1978). Such definitions assume that the covariation among different organizational and industrial concepts is sufficiently high to be represented by a single factor, an assumption disputed by several recent empirical studies (Hodson and Kaufman 1981; Kaufman, Hodson, and Fligstein 1980; Wallace and Kalleberg 1981). The unidimensional approach obscures the differential impact of organizational and industrial variables on income inequality; consequently, it is difficult to assess precisely which aspects of economic segmentation are responsible for the generation of income differences. In view of these limitations, we adopt a multidimensional approach to economic segmentation in this paper.

An additional issue concerning the conceptualization of economic segmentation is whether the variables measuring the dimension(s) are discrete or continuous. Several studies dichotomize industries into monopoly and competitive sectors, which are assumed to represent qualitatively different types of employment situations in the private sector of the economy (e.g., Bibb and Form 1977; Beck et al. 1978). Others (Kaufman et al. 1980)

use cluster analysis to identify 16 discrete sectors that more accurately reflect economic divisions. Such discrete or categorical measures have their merits. They provide relatively parsimonious representations of the structure of economic segmentation that have many uses. For example, discrete measures facilitate the assessment of interactions, since they can be used as variates in analyses of covariance. An alternative strategy, which we employ in this paper, is to conceptualize economic segmentation as a set of continuous variables. This procedure does not enable us to identify specific sectors or industrial groups, but it has specific advantages for studies of income inequality. In particular, it facilitates the use of contextual analysis to assess the unique effects of different components of economic segmentation on income. It also permits us to examine directly the ways in which these effects are mediated by our worker power measures within the context of a structural equation model.

A final issue in the measurement of economic segmentation concerns the appropriate unit of analysis. Since firms are the key actors in the economy, much of the theoretical literature on economic segmentation concerns the firm (e.g., Averitt 1968; Baron and Bielby 1980). Yet most empirical studies in the dual economy tradition use industries, or aggregations of firms, as the units of analysis (Beck et al. 1978; Bibb and Form 1977; Tolbert et al. 1980; Hodson 1978; Kaufman et al. 1980; Wallace and Kalleberg 1981). This procedure presents certain difficulties. Industry-level variables provide imperfect measures of the resources available to employers, since they represent aggregations of information for all firms in an industry. In addition, firms may often operate in more than one industry, and a measure based on industrial information may have limited applicability to the economic success of an individual in such firms.

Despite the limitations of using industry-based data, this procedure has some theoretical support. O'Connor (1973) and Hodson (1978) argue that industries are appropriate units of analysis in studies of economic segmentation. Firms must operate within certain constraints imposed by industrial structure. In addition, Hodson points out that many industries are accessible only to monopoly firms and that state intervention in the economy is often industry specific. He also notes that the growth of industrially eclectic conglomerates is a relatively new development and represents a considerably smaller proportion of American business activity than is generally assumed. This is underscored by the fact that specific conglomerate branches are necessarily constrained by the organization of the specific industries in which they operate.

In view of these theoretical arguments, and despite the potential limitations, we use four industry-based measures in our operationalization of

economic segmentation: concentration, economic scale, capital intensity, and state purchases. To offset somewhat the limitations of these industrial level variables, we also use a firm-level measure of segmentation: the size of the establishment for which the individual works. Since size is strongly and causally related to several dimensions of organization structure relevant to economic segmentation (see Stolzenberg [1978] for a discussion), it serves as a useful indicator of the firm's capacity and/or willingness to pay high wages.

Economic Segmentation and Worker Power

Explanations of income inequality based solely on these aspects of economic segmentation are incomplete in that they ignore those attributes of workers that contribute to income differences. Persons in similar firm and industrial contexts, for example, may often obtain unequal rewards. Emphasizing as it does the supply side of the labor market, neoclassical economic theory can help explain a portion of these differences: some workers have greater skills than others and will therefore exhibit greater levels of marginal productivity. Skill differences only partly account for why some workers earn more than others, however, and the neoclassical theory has been extensively criticized for ignoring other sources of worker power (see, e.g., Kalleberg and Sørensen 1979). Dual economy theory, in contrast, regards worker power as central for explanations of income inequality. Further, the dual economy perspective suggests a link between economic segmentation and worker power, that is, that "the economic stratification of enterprises is matched by the organizational power of occupational groups" (Bibb and Form 1977, p. 978). Such correspondence would make little sense in neoclassical terms, since attributes such as human capital are assumed to have similar effects on income in dissimilar firm and industrial contexts.

Worker power refers to the ability of employees, individually or collectively, to obtain an advantaged position in the stratification system. In this respect, worker power represents the differential "market capacity" of workers (cf. Parkin 1971; Giddens 1973), which, in turn, leads to better wage and nonwage benefits. Worker power is a relational concept, and workers may obtain power relative to other workers as well as vis-à-vis employers (cf. Lukes 1977). This view of worker power focuses on the effects and processes of power and infers the existence of power from the differential ability of workers to realize their interests through participation in labor markets (see also Form and Huber 1976). In this paper, we use the term "worker power" to refer to those attributes acquired by

workers *after* entry into the labor force that enhance their monetary benefits. This distinguishes these attributes from those that are obtained by workers before entering the labor force (e.g., education, intelligence).

The dual economy literature has focused primarily on those types of worker power that are specific to an employer or industry and has generally assumed that such forms of worker power are likely to exist in contexts where firms have high resources (e.g., Bibb and Form 1977). These forms of power are established through collective, institutionalized arrangements between employers and workers (e.g., unions, internal labor markets) and represent protectionist strategies by workers against the wage-depressing effects of employers' attempts to lower costs and against competition from other workers (Kerr 1954; Freedman 1976). Such firm- and industry-specific forms of worker power are likely to help explain why economic segmentation is related to income inequality: workers are able to acquire power in relation to employers, which induces them to translate their resources into employee economic benefits.

Unions and internal labor markets do not exhaust all relevant forms of worker power. Less emphasized by dual economy theory, but nonetheless crucial for explanations of income inequality among individuals, are two additional dimensions of stratification among employees: occupational differences (e.g., skill and licensing) and class position. These forms of worker power are exercised in relation to other workers in labor markets and will therefore have important effects on income determination processes. Since these types of worker power denote mainly relations among employees, however, there is little reason to expect them to be specific to given firms and industries; instead they should be relatively transferable across dimensions of economic segmentation. Although they are important determinants of income inequality, then, such attributes of worker power are less likely than the others to help account for the relationships between economic segmentation and income. In the following sections, we discuss the links between economic segmentation and the various types of worker power in greater detail.

Unions.—Good reasons exist for expecting unions to locate and acquire power in contexts defined by each of our five dimensions of economic segmentation. Unions are more likely to organize in firms with a large number of employees than in small firms. Further, once industrial markets become concentrated, unions often collaborate with powerful employers to create higher entry barriers for nonunion sources of labor (Levinson 1967; Dalton and Ford 1977). High volumes of profits, assets, and sales (i.e., economic scale) encourage unionization by creating a larger economic pie for employers and workers to divide, thus providing an incentive for worker

organization. Unions are also important mechanisms for ensuring that workers benefit economically from increases in productivity which may be due to more capital intensive forms of production. Finally, industries that engage in state-sponsored production are likely to be unionized because of the existence of a relatively stable supply of labor in those industries, a precondition for effective organization (O'Connor 1973).

It is clear that the effects of economic segmentation on earnings must be interpreted in terms of the impact of unions. A number of studies, for example, have argued that the effects of concentration on income must be assessed in view of the relationship between concentration and unionization (see, e.g., Segal 1964; Levinson 1967; Weiss 1966; Dalton and Ford 1977). In particular, Weiss and Dalton and Ford provide evidence for a negative interaction effect between concentration and unionization.[2] Nevertheless, the literature remains inconclusive regarding the interplay between economic segmentation and unionization in generating income differences.

Employer tenure.—Workers may also acquire organizationally specific power by accumulating tenure with a specific employer. The longer a worker has been employed by the same firm, the more specific knowledge of the organization he or she is likely to have acquired, and thus the more likely it is that the employer is dependent on the worker for a certain level of performance. This dependence, as well as the skills that the worker has acquired through firm-specific experience, should be manifested in higher earnings.

The link between economic segmentation and this form of worker power may be found in the notion of a firm internal labor market; employer tenure reflects one direct consequence of this important concept. That is, while tenure is acquired individually by workers, it is derived from collective features of the social relations of production in the form of internal labor markets. These markets serve important functions for both employers and workers (see, e.g., Edwards 1979; Doeringer and Piore 1971; Freedman 1976; Althauser and Kalleberg 1981). For employers, internal labor markets both ensure a regular competent supply of labor to fill organizational positions and reinforce work discipline by creating a hierarchical arrangement of status and advancement. For workers, the primary bene-

[2] This suggests that high levels of industrial concentration, while aiding organizational efforts, may actually hinder the ability of unions to extract higher wages from employers. This finding is difficult to interpret, in view of the fact that both concentration and unionization are positively related to income. It should be noted, however, that empirical research on this issue has relied on cross-sectional data; longitudinal data on changes in the relations among concentration, unionization, and economic inequality for a variety of industries are necessary in order to gain greater insight into the meaning of this negative interaction effect.

fits are the guarantee of stable employment, the acquisition of human capital through on-the-job experience, and the perceived opportunity for advancement. Firm internal labor markets have been argued to correspond closely to several dimensions of economic segmentation, especially large organization size (e.g., Stolzenberg 1978, p. 818) and concentration (e.g., Beck et al. 1978, p. 707).

Occupations.—Occupational structure approximates a technical division of labor that is common to many firms and industries (Kalleberg and Griffin 1980). We would therefore expect only a modest relationship between our five concepts of economic segmentation and the distribution of occupations. All industries need certain types of labor power such as managers and laborers. Other occupations, however, such as professionals, clerical workers, and production workers, may tend to be more unequally distributed across firms and industries. The first aspect of occupational structure we will investigate, occupational *skill,* is closely related to this theoretical conception of occupation. Since occupations are positions whose incumbents perform similar technical activities in a division of labor, they should have similar skill and training requirements. This dimension reflects occupational power in the sense that possession of scarce skills by workers constitutes a potential source of employer dependency. Skilled workers thus have greater power than unskilled workers to obtain rewards from labor market participation.

While occupational skill may provide a basis for certain forms of collective organization such as craft unionism or licensing groups, it may often be exercised by workers individually and diffusely throughout the economic structure. Thus it comes as no surprise that a clear relationship between skill and one or more dimensions of economic segmentation has not been demonstrated (e.g., Bibb and Form 1977).

Our second form of occupational power, *licensing,* is clearly related to occupational skill. However, licensing refers to the ability of workers engaged in certain specialized activities to control collectively the supply of labor entering those occupational groups. Licensing allows occupational incumbents to maintain some control over labor skills (e.g., Freedman 1976; Form and Huber 1976). While licensing reflects an occupationally based source of power, the specialized nature of the task involved typically implies that the activity takes place in specific institutional contexts. Thus, licensing furnishes a more probable example than skill of economic segmentation being matched by the presence of worker power. We would expect, for example, that licensing is a prominent feature of industries which are engaged in research and development projects and thus employ large numbers of skilled professional, semiprofessional, and blue-collar workers.

Class.—Class represents positions in the social relations of production and, within a system of production, is manifested by variations in control over the appropriation of the product as well as the activities of labor. Class differences, therefore, are conceptually distinct from occupational structure and affect the distribution of income independently (Kalleberg and Griffin 1980; Wright and Perrone 1977). The main dimension of class that stratifies employees is control over labor power or authority; this leads to the definition of manager and worker classes (Wright 1979; Robinson and Kelley 1979).

Social relations of production relate to economic segmentation since class positions are located within firms. Our operational definition of class refers to these objective positions; this conforms to the Marxist notion of class in itself, as opposed to organizations within classes, or class for itself (see, e.g., Wright 1980). There is little reason, however, to expect that the measure of authority relations we use in our subsequent analyses will be strongly related to our economic segmentation indicators, since such social relations characterize all forms of organization of capitalist production. While the specific nature of these relations may well differ in diverse institutional contexts, these differing class structures are likely to possess more complexity than reflected by the crude distinction used here (see also Wright 1979).

Economic Segmentation and Socioeconomic Achievement

Our conceptualization of economic segmentation and worker power provides a framework for examining the impact of economic organization on income inequality. Part of this framework, referring to capital intensity, internal labor markets, and occupational skill, is compatible with both economic segmentation and neoclassical perspectives; other parts, such as concentration, unions, licensing, class, and state intervention, violate central tenets of neoclassical theory (see Gordon 1972). Combining the economic segmentation and worker power perspectives provides the basis for a useful alternative theory to that of neoclassical economics for understanding the distribution of income. Such a merger would supplement and qualify neoclassical and status attainment models of income inequality in important ways. In particular, we may be able to specify better the institutional contexts which condition the impacts on economic success of attributes acquired before labor force entry. In order to study how firms and industries may affect one important aspect of socioeconomic achievement, we consider the interplay between economic segmentation and education in generating income inequality.

The positive relationship of education to income has been well docu-

mented (e.g., Sewell and Hauser 1975). Economic segmentation could condition this effect of education in two main ways. First, education allocates people to firms and industries in which employers have differing levels of resources. In addition, it sorts people into occupations with differing levels of worker power. Thus, part of the effect of education on income may be explained on the basis of differences in levels of worker power and in firm and industrial characteristics. In this view, the effects of economic segmentation on income would be partly spurious, owing to their common dependence on education (e.g., Kalachek and Raines 1976; Weiss 1966).

Of greater interest in recent economic segmentation literature is the second way in which firm and industrial characteristics condition the effects of education on income. The economic returns to education may differ in various structural locations, so that similar inputs of education may result in different incomes depending on the firm and industrial context in which a person is employed. Several studies have argued for the presence and importance of such interaction effects, though these findings have yielded inconclusive results regarding the effects of education (cf., e.g., Stolzenberg 1978; Bluestone, Murphy, and Stevenson 1973; and Beck et al. 1978).

Our empirical investigation of the relationships among economic segmentation, worker power, education, and income inequality will examine them separately for males and females. A number of studies have suggested that labor markets are segmented by sex and that the impact of economic segmentation on income will differ for males and females (e.g., Edwards 1979; Bibb and Form 1977). Unfortunately, most of the empirical studies of these issues have restricted their analyses to males, precluding an assessment of potential sex differences.

GENERAL HYPOTHESES

Our discussion of economic segmentation, worker power, and income inequality has suggested many relationships among these concepts. These links constitute central research hypotheses for empirical investigations of the structural bases of stratification. Our subsequent analyses will address four general issues raised by our discussion of the literature; each hypothesis assumes that other relevant variables have been controlled.

1. We have argued that economic segmentation affects income differences and that these effects are not reducible to one aspect of firm and industrial structure but are multidimensional in origin. We therefore expect that workers in industries characterized by high concentration, high economic scale, high capital intensity, and/or high volumes of sales to

the state will have higher incomes than workers in industries with low levels of these attributes. Further, workers in larger firms should earn more than workers in small firms.

2. Our conceptualization of sources of worker power suggests that each of these dimensions should have an independent effect on income inequality. Thus, we expect that workers who are union members, who have greater employer tenure, who are members of occupations with high levels of skill or licensing, and/or who exercise authority over the labor power of others will have greater power than individuals without these attributes and will therefore obtain higher incomes.

3. Our discussion has implied that the relationships between economic segmentation and worker power are more complex than is generally assumed in the dual economy literature. Dual economy theory would hypothesize that, in particular firm and industrial contexts (denoted especially by high concentration, large establishment size, and high purchase of goods by the state), workers are able to acquire certain forms of power (i.e., via unionization, employer tenure, and perhaps licensing). These three aspects of worker power should therefore be primarily responsible for explaining the impact on income of the firm and industrial variables. In contrast, occupational skill and class position are forms of worker power that are less specific to particular firms and industries and thus should be only weakly related to the dimensions of economic segmentation. These types of worker power are not well explained by dual economy theory and point to gaps where the theory needs to be supplemented by concepts derived from other theoretical frameworks.

4. Our discussion has suggested that the economic segmentation concepts denote contexts wherein the determinants of income inequality differ. We therefore hypothesize that some of the worker power measures, especially union membership, should interact with the economic segmentation variables in the prediction of income. In addition, we expect that the effects of education on income should also differ for several dimensions of economic segmentation, particularly establishment size and industrial concentration. This general hypothesis concerns the degree to which economic segmentation produces differences in the processes by which worker power and education are related to income.

A CONCEPTUAL MODEL

These hypotheses address a number of issues derived from dual economy theory, as well as from more individualistic explanations of income inequality, such as human capital and status attainment approaches. Our conceptual framework thus attempts to synthesize a variety of concepts

from diverse theories of income determination. While many of our hypotheses refer to structural relationships among nonindividual units such as firms, industries, and organized groups of workers, we are here primarily interested in the impacts of such structures on individual income inequality. Consequently, we estimate models based on data derived from individuals.

Our empirical investigation of these general hypotheses will be guided by the conceptual model represented by the following system of equations:

$$\$ = f(I, C, ES, WP), \tag{1}$$

where $\$ =$ (log) income, I is a vector of individual characteristics (education, sex, race), C is a vector of control variables such as region and age, ES is a vector of our five economic segmentation variables, and WP is a vector of the worker power indicators. Further,

$$WP = f(I, C, ES), \tag{2}$$

and

$$ES = f(I, C). \tag{3}$$

Our conceptual model assumes that education, sex, and race are exogenous variables in the sense that they are acquired by individuals before their entry into the labor force. Further, the worker power variables are assumed to be endogenous to the measures of economic segmentation; that is, economic segmentation is causally prior to worker power. This latter assumption is based on a structural argument rather than simply a temporal one; it is consistent with the view that firms and employers are the key actors in the economy and that workers must contend for power with the firms that already exist in an industry. This does not imply, of course, that there is not a dynamic, reciprocal relationship between economic segmentation and worker power. Edwards (1979), for example, argues that, historically, control systems were developed by employers in response to the actions of labor. A serious assessment of such potential reciprocal effects between economic segmentation and worker power requires longitudinal data on macro units such as industries and firms, data which we do not have at present. However, for the purpose of assessing the impact of economic segmentation on individual earnings, these dynamic relationships are reasonably represented by the cross-sectional, structural model proposed here.

Given this causal specification, we can analyze empirically the general hypotheses raised by our review of the literature. The direct effects of all variables on income can be derived from equation (1). Further, the relationships between economic segmentation and worker power can be

obtained from equation (2). The assessment of the indirect effects on income of economic segmentation via worker power can be made from the information contained in equations (1) and (2). Finally, we can assess the presence of interaction effects between economic segmentation, and education and worker power, by adding multiplicative terms to these equations. To assess interactions by sex, we perform several of our analyses separately for males and females.

DATA AND VARIABLES

For the most part the analyses to be presented in this paper are based on data from the 1969–70 Survey of Working Conditions (SWC) and the 1972–73 Quality of Employment Survey (QES). These surveys are representative of the national employed civilian labor force in those years. Persons eligible for interview were household members at least 16 years old and gainfully employed for 20 or more hours per week. The data were collected by the Institute for Social Research at the University of Michigan (see Quinn and Shepard [1974] for a description of the surveys and sampling procedures). The sizes of these samples were 1,533 (SWC) and 1,496 (QES). To increase the sample size for analytic purposes, we pooled the two surveys to create a single data file.

We have restricted the sample in several ways. First, we excluded self-employed persons, since our arguments regarding employer effects on income refer to employees. Second, since some of our economic segmentation concepts (e.g., concentration, economic scale, purchase of goods by the state) are defined only for the private sector of the economy, we excluded employees of federal, state, and local governments from the analysis. While such organizational variables as size may have similar effects on worker power and income in private and public sectors, we will not be able to evaluate this possibility here. These restrictions reduced the size of our pooled sample to 2,268. Missing data on some variables further reduced the sample used in most of our analyses to 2,132; there are 1,326 men and 806 women in this final sample.

Economic Segmentation Measures

In order to construct measures of our industrial level economic segmentation concepts (concentration, economic scale, capital intensity, state purchase of goods), data were collected on these variables from various sources for 68 major industry groups; these groups were constructed from the 1967 Standard Industrial Classification (SIC) two-digit codes (see Wallace and Kalleberg [1981] for a list of these industries). This classification was chosen for two main reasons. First, SIC classifications are the basis

for reporting corporate income tax returns and other data which are used in the construction of the variables. Second, the 1967 SIC index was the most recent one before the 1969–70 SWC data were collected. Our industrial data on concentration, economic scale, capital intensity, and the state purchase of goods come from a period circa 1970 which coincides fairly closely with the period during which the individual data sets were collected. These industrial variables were assigned to individuals in the SWC and QES data by matching the 1967 SIC codes to the 1960 census industry codes, which were already coded onto the individual level data sets.

Our measures of the four industrial economic segmentation concepts are linear composites of variables designed to tap these dimensions. Combining items to form scales for these concepts was further justified by an oblique factor analysis of the individual items (see Wallace and Kalleberg 1981). Information on scale construction, a full description of the items, and a list of data sources are given in the Appendix. Briefly, the concentration measure reflects the share of the product market controlled by an industry's most powerful firms; economic scale is an indicator of the average profits, assets, and sales receipts in an industry (these indicators are very highly correlated with each other, which led us to combine them into a single scale); the state purchase of goods measure represents the proportion of goods produced by the industry that are sold to federal, state, and local governments; and capital intensity is the amount of non-labor-production costs per worker in an industry.

Data on our final economic segmentation concept, size, are available from a question in the SWC and QES, which asks the respondent: "About how many people work at the location where you work—I mean all types of workers in all areas and departments?" This is the only employer size measure available in these data, and while it is an indicator of establishment size as opposed to the perhaps theoretically preferable firm size, its use as a proxy for the concept appears reasonable given current work on the topic; that is, small employer size is a sufficient condition for small establishment size and large establishment size is sufficient for large employer size (see, e.g., the discussion in Stolzenberg [1978], pp. 818–19).[3]

[3] An oblique factor analysis of these five economic segmentation variables across individuals in the SWC-QES data (not reported) yielded two factors with eigenvalues greater than one, accounting for 60.1% of the total variance. Concentration, economic scale, and capital intensity loaded on the first factor, with establishment size and concentration making up the second factor. The correlation between the factors was .233. The state purchase measure loaded weakly on both factors and generally shared little common variance with the other economic segmentation variables. This analysis, which is consistent with that reported by Wallace and Kalleberg (1981) at the industry level, supports our assumption that the structure of economic segmentation is multidimensional and cannot be adequately represented by a single factor.

Worker Power Measures

Our five concepts of worker power are measured in the following ways. The union measure indicates whether the respondent is a member of a union or other employee's association (0 = nonmember; 1 = member). Employer tenure is a measure of the respondent's length of service with his or her current employer (1 = less than 30 days; 2 = 30–90 days; 3 = 3–12 months; 4 = 1–3 years; 5 = 3–5 years; 6 = 5–10 years; 7 = 10–20 years; 8 = more than 20 years). The skill level of the employee's occupation is measured by the *Dictionary of Occupational Titles's* assessment of the relative training time required for adequate occupational performance, the Specific Vocational Preparation or SVP scores (see, e.g., Temme 1975; Kalleberg and Griffin 1980; Bibb and Form 1977).[4] Our measure of occupational licensing is the percentage of people in each of 270 cells of an occupation-by-industry matrix whose occupations are licensed in at least 30 states (Freedman 1976). Occupations with high degrees of licensing include professional, semiprofessional, sales, and some craft and service occupations. This measure does not refer to a person's response to questions on the SWC or QES surveys but corresponds instead to his or her position in Freedman's occupation-by-industry matrix. Thus, persons in the same occupation-industry group are assigned the same licensing score. Finally, our measure of manager class position indicates whether an individual supervises the labor power of others (1 = has supervisory authority; 0 = has no supervisory authority; see Wright and Perrone 1977).[5]

[4] SVP measures the amount of training acquired in vocational schools or on the job; such training is most relevant to the performance of the specific occupational activity. Occupational skill levels, however, are also influenced by the amount of general intellectual ability required for the performance of the activity. Such skill is acquired primarily through formal education and can be measured by the *Dictionary of Occupational Titles's* (DOT) General Educational Development (GED) scores. We assessed the implications of restricting our measure of occupational skill to SVP by reestimating the models reported in table 2 in several ways. We used the QES data only, as these data contain both SVP and GED measures assigned to DOT occupation titles. First, we combined SVP and GED into a single measure using Z scores; the results paralleled those reported in table 2. Second, we included both SVP and GED as regressors in the equation. Again, the estimates for the economic segmentation and other worker power variables corresponded to those presented in table 2. However, the high collinearity between the SVP and GED measures (.831 for males, .845 for females) impeded an assessment of their unique effects (e.g., for males, SVP was significant but GED was not; for females, the reverse was true). We conclude from these results that our interpretations are not seriously affected by using only SVP as a measure of occupational skill.

[5] An oblique factor analysis of the five worker power variables across individuals in the SWC-QES data (not reported) produced three factors with eigenvalues greater than one, accounting for 75.3% of the total variance. The two occupational measures, skill and licensing, constitute the first factor; union membership and employer tenure

Other Measures

The other variables used in our analyses were based on information contained in the SWC and QES data sets. The dependent variable of primary interest in this analysis is the natural logarithm of the respondent's annual income from his or her main job.[6] Education is coded in years of schooling completed; sex is coded 0 = female and 1 = male; and race is coded 0 = nonwhite and 1 = white. Finally, we use two variables for control purposes; they are regarded as exogenous, as they are not assumed to depend on any of the other measures in the model. These are region (0 = non-South, 1 = South) and age, a rough indicator of total labor force experience as well as life cycle differences more generally.[7]

Let us now turn to the empirical results.

EMPIRICAL ANALYSES

Before turning directly to the analyses designed to examine our general hypotheses, we first assess differences in labor force composition associated with our dimensions of economic segmentation and worker power. Table 1 presents the results of regressions of the economic segmentation variables on the exogenous variables (eq. [3]) as well as reduced-form estimates (col. 1) and estimates of the direct effects (col. 2) of these exogenous variables on the worker power measures. This analysis suggests that there are differences in the distribution of the labor force among contexts defined by our economic segmentation and worker power measures, differences that are broadly consistent with previous work on socioeconomic stratification. First, education is positively related to all attributes of worker power except union membership. In addition, workers with more education are more likely (relative to those with less education) to be employed in larger establishments and in industries which sell goods to the state and less likely to work in concentrated and/or capital intensive

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loaded on the second; and our manager-class variable characterized the third. The correlations among factors were: —.031 (1 and 2), —.240 (1 and 3), and —.042 (2 and 3).

[6] To adjust for inflation, income in the QES sample was multiplied by .8763, a ratio measuring 1972 income in 1969 dollars. The measure is based on the consumer price index for both years.

[7] The QES data set contains a direct measure of the number of years the respondent has been in the labor force; this is not the case for the SWC data. Further, owing to the discontinuities which characterize the careers of women, estimates of work experience based on formulas such as age — education — 5 are poor proxies for the actual work experiences of females. Because of these inconsistencies across samples and difficulties in constructing proxy measures, we use the person's age as a general control for total work experience.

TABLE 1

STANDARDIZED COEFFICIENTS OBTAINED FROM REGRESSIONS OF ECONOMIC SEGMENTATION AND WORKER POWER MEASURES ON EXOGENOUS VARIABLES, SWC AND QES DATA (N = 2,132)

	DEPENDENT VARIABLE														
INDEPENDENT VARIABLE	Concentration	Economic Scale	State	Capital Intensity	Size of Establishment	Union		Employer Tenure		Occupational SVP		Occupational Licensing		Manager	
						(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Education	−.167**	−.018	.179**	−.210**	.051*	−.049*	−.072**	.027	.048**	.386**	.370**	.463**	.392**	.180**	.181**
Sex (1 = male)	.205**	.020	.079**	.247**	.048*	.151**	.119**	.099**	.076**	.144**	.139**	.100**	.147**	.154**	.165**
Race (1 = white)	.057**	−.024	−.024	.063**	−.053**	−.079**	−.072**	.001	.001	.086**	.085**	−.026	−.007	.021	.022
South (=1)	−.067**	−.032	.006	−.012	−.083**	−.157**	−.134**	−.025	−.005	−.0001	−.004	.001	−.007	−.0001	−.002
Age	−.059**	.012	.034	−.035	−.017	.045*	.051**	.554**	.565**	.089**	.083**	.111**	.094**	.149**	.148**
R^2 (adjusted)	.072	.000	.033	.106	.012	.053	.140	.316	.365	.174	.200	.206	.263	.066	.066

NOTE.—Col. 1: regressions of worker power measure on five exogenous variables only; col. 2: regressions of worker power measure on exogenous variables and economic segmentation variables (coefficients for economic segmentation variables not presented).

* $P \leq .05$.

** $P \leq .01$.

industries. Second, men are more likely than women to have all five attributes of worker power as well as to work in contexts where employers have high resources. The sole exception is economic scale, which is unrelated to any of our indicators of labor force composition. Finally, we observe that race differences in composition are relatively small (owing perhaps to the relatively few nonwhites in these samples); that workers in the South are less likely to work in large establishments, to work in concentrated industries, and to be union members; and that older workers, while less likely to be employed in concentrated industries, are more likely than younger workers to possess all five types of worker power. In general, we note that the worker power measures are more strongly related to these exogenous variables than are the economic segmentation indicators.

Analysis of Income Inequality: Additive Effects

In this section, we address the issues raised by our first two general hypotheses. Our model here assumes that the effects of economic segmentation and worker power variables on income are additive and linear; we will examine interaction effects later in the paper. The results of these regressions are presented in table 2. Since a number of our independent variables interact with sex in the prediction of (log) income (see col. 5, which refers to the coefficients in col. 3), we present our results in this and subsequent tables separately for males and females.[8]

We note first that both economic segmentation and worker power measures significantly add to our ability to account for income differences among individuals in these data. Taking the R^2 (adjusted) for our exogenous variables as a baseline (.186 for males, .210 for females), for example, we find that the addition of the economic segmentation variables as a group increases the explained variance by about half for both males (.269) and females (.298). Moreover, the addition of the worker power measures increases (adjusted) R^2 to .330 (males) and .396 (females).

Looking more closely at the effects of the economic segmentation variables, we find that concentration and establishment size have relatively strong effects on income for both males and females. Moreover, males (and, to a lesser degree, females) benefit economically from employment in industries which sell goods to the state. These findings provide support for our first general hypothesis.

[8] The overall F-test for increment to (unadjusted) R^2 ($F_{14,2116} = 4.637$) is significant beyond the .01 level. This suggests that the processes of income determination differ by sex, which leads us to estimate our equations separately for males and females. We note, however, that a systematic analysis of sex differences is tangential to our purposes in this paper.

TABLE 2

UNSTANDARDIZED COEFFICIENTS OBTAINED FROM REGRESSIONS OF (log) INCOME ON VARIABLES DESCRIBED IN TEXT, SWC, AND QES DATA
(N = 1,326, Males; N = 806, Females)

INDEPENDENT VARIABLE	DEPENDENT VARIABLE = LOG (Income)							
	(1)		(2)		(3)		(4)	(5)
	Males	Females	Males	Females	Males	Females	Pooled Equation	F-Statistic for Sex Interaction
Sex (1 = male)	...	...	...	...	...	...	.486 (.378)**	...
Education	.127 (.368)**	.189 (.452)**	.128 (.371)**	.193 (.462)**	.100 (.289)**	.122 (.291)**	.107 (.253)**	1.52
Race (1 = white)	.164 (.089)**	−.011 (−.007)	.166 (.090)**	−.009 (−.006)	.136 (.074)**	−.013 (−.008)	.063 (.032)*	5.87**
South (=1)	−.133 (−.114)**	−.097 (−.081)**	−.102 (−.088)**	−.074 (−.062)*	−.093 (−.079)**	−.041 (−.034)	−.074 (−.054)**	1.39
Age	.008 (.206)**	.003 (.080)**	.008 (.203)**	.005 (.116)**	.003 (.075)**	.001 (.024)	.002 (.036)***	1.25
Concentration	...	...	.033 (.171)**	.029 (.142)**	.024 (.127)**	.032 (.156)**	.023 (.105)**	.57
Economic scale	...	...	−.011 (−.025)	−.023 (−.042)	−.014 (−.031)	−.054 (−.100)**	−.024 (−.043)**	3.26***
State	...	...	.043 (.129)**	.033 (.075)*	.037 (.110)**	.007 (.016)	.031 (.075)**	3.32***
Capital intensity	...	...	.020 (.034)	−.005 (−.008)	.034 (.056)*	.013 (.022)	.034 (.050)**	.66
Size of establishment	...	...	.045 (.163)**	.073 (.235)**	.041 (.149)**	.066 (.212)**	.048 (.145)**	4.21*
Union (=1)	...	...	...	...	.083 (.075)**	.195 (.153)**	.133 (.101)**	5.60**
Manager (=1)	...	...	...	...	.128 (.119)**	.091 (.075)**	.113 (.089)**	.69
Occupational SVP	...	...	...	...	.045 (.138)**	.094 (.233)**	.062 (.156)**	9.72**
Occupational licensing	...	...	...	...	.001 (.045)***	.003 (.095)**	.002 (.051)**	2.14
Employer tenure	...	...	...	...	.042 (.148)**	.040 (.124)**	.044 (.129)**	.025
Intercept	8.036	7.486	7.847	7.191	7.643	6.915	7.104	6.915
R^2 (adjusted)	.186	.210	.269	.298	.330	.396	.504	.512
Standard error of estimate	.485	.493	.459	.464	.440	.431	.440	.436

NOTE.—Col. 1: reduced-form estimates of the effects of the exogenous variables; col. 2: reduced-form estimates for the economic segmentation variables; col. 3: direct net effects of all variables on income. Standardized coefficients are in parentheses.

* $P \leq .05$.

** $P \leq .01$.

*** $P \leq .10$.

In contrast, capital intensity and economic scale are only weakly related to income. Capital intensity has a significant positive direct effect for males, although this is small relative to the effects of concentration, establishment size, and state purchase of goods. Moreover, economic scale has no net effect on income for males ($r = .09$); although it has a significant net effect on income for women. This surprising small negative effect may have a substantive interpretation. It may be, for example, that economic scale is an indicator of the ability of employers to exploit women by giving them a relatively smaller share of the available economic pie.[9]

One way of evaluating the ability of our model to account for income differences across industries is to compare it with an equation which controls for any and all SIC industry differences. That is, we enter a variable which represents the mean level of (log) income for each industry group as a regressor, a procedure that is equivalent to an analysis of covariance. An equation containing the exogenous variables and the industry mean yields (adjusted) R^2 values of .297 (males) and .351 (females); thus, our SIC industry categories explain 11.1% and 14.1% of the variance in (log) income for males and females, respectively. In comparison, an equation containing the exogenous variables in addition to our four industry-based measures of economic segmentation produces (adjusted) R^2 values of .248 (males) and .251 (females), which represent increments of 6.2% (males) and 4.1% (females). Thus, our four industry variables account for about 56% of the industrial differences in income for males and 29% of these differences for women.

Turning now to the worker power measures, we note that all five attributes of worker power are positively and significantly related to (log) income for both males and females. This provides considerable support for our second general hypothesis. It is interesting to note that two of these worker power measures—union membership and skill—interact with sex in the prediction of income, and both have greater effects for women. Thus, while women are less likely to belong to unions and to have high occupational skill levels, their average economic returns to these attributes of worker power are greater than for men.

While a number of variables interact with sex, we interpret such inter-

[9] To assess further the plausibility of our interpretation of this negative effect, we reestimated our (table 2) equations for a highly select group of employees: those who are simultaneously not union members, not in the manager class, and not in professional, semiprofessional, and managerial occupations ($N = 318$ males and 367 females). Our reasoning was that this group should be particularly vulnerable to exploitation by employers regardless of sex. These results (not shown) replicated those observed earlier: economic scale had a nonsignificant effect on income for males but a significant negative effect for females. While not inconsistent with our substantive interpretation, our finding of a negative effect of scale only for women is also consistent with the argument that the female equation may be misspecified.

actions with caution. Our use of individual F-tests to detect them may raise problems of simultaneous inference, as some of these significant effects may have been observed by chance. The strongest interaction of an economic segmentation variable with sex is establishment size ($P = .04$), which indicates that women may receive more economic benefit than men from employment in larger organizations (see also Bibb and Form 1977).

Economic Segmentation and Worker Power

An overall view of the relationships between economic segmentation and worker power in these data is provided by the results (not shown) of a canonical correlation analysis of these two sets of concepts across individuals in the SWC-QES data. Two main canonical variates were extracted: the first (canonical correlation = .349) linked industries which sell goods to the state and industries low in capital intensity to the degree to which a worker's occupation is licensed, while the second (.347) related employment in concentrated industries and larger establishments to membership in unions. A third, significant, canonical variate (.152) linked large establishments to low occupational skill levels.

Table 3 presents the coefficients obtained from regressions of all worker power measures on the economic segmentation variables (as well as the exogenous variables). These results[10] are generally consistent with many of our expectations regarding the relationships between economic segmentation and worker power. The results for union membership, for example, indicate that being a member of a union or employee's association is positively related to industrial concentration (for males only), state purchase of goods, and establishment size. Less expected are the negative relationships between unions and economic scale (for men) and capital intensity. In any event, we note that these two negative effects are relatively small.

The results for employer tenure also largely confirm our expectations. Establishment size and concentration (for males) are significantly and positively related to employer tenure. State purchase of goods, however, is negatively related to employer tenure. Our interpretation for this negative effect rests on the observation (see n. 11 below) that industries which sell goods to the state are associated with high proportions of professional and craft occupations. Individuals in these occupations are more likely to view the occupation, rather than the employer, as a source of career ad-

[10] In tables 3 and 4, we test the significance of the difference between males and females with respect to a particular coefficient by means of a t-test. Since we reported differences significant at a P value of .05 or less, this should be multiplied by the number of contrasts made in an equation to determine the overall significance level. For table 3, this is $.05 \times 5 = .25$; for table 4, $.05 \times 6 = .30$.

TABLE 3

STANDARDIZED COEFFICIENTS OBTAINED FROM REGRESSIONS OF WORKER POWER MEASURES ON ECONOMIC SEGMENTATION VARIABLES (Eq. [2] in Text), SWC AND QES DATA (N = 1,326, Males; N = 806, Females)

Economic Segmentation Variable	Worker Power Variable[a]										Proportion Mediated[b]	
	Union		Employer Tenure		Occupational SVP		Occupational Licensing		Manager			
	M	F	M	F	M	F	M	F	M	F	M	F
Concentration	.211**	.005[c]	.125**	.066	.069**	.008	−.061*	−.166**	.023	−.127**[c]	.257	−.082
Economic scale	−.064*	.051[d]	−.027	.061[d]	.085**	.124**	.088**	.104**	−.003	.045	.162	.367
State	.127**	.147**	−.088**	−.023	.127**	.121**	.133**	.208**	−.008	−.115**[d]	.147	.787
Capital intensity	−.083**	−.025	.004	−.014	−.033	−.078*	−.193**	−.081*[c]	−.029	.013	−.282	−.577
Size of establishment	.181**	.199**	.157**	.152**	−.116**	−.098**	−.015	−.095**	−.053	.078*[c]	.086	.098
R^2 (adjusted)	.159	.099	.376	.325	.171	.227	.245	.306	.046	.052	...	...

[a] Equations control for education, race, south, age.

[b] Proportion mediated = indirect effect/|direct effect|+|indirect effect|. Direct plus indirect effects are calculated from cols. 2 and 3 in table 2.

[c] t-test for sex interaction: $P \leq .01$.

[d] t-test for sex interaction: $P \leq .05$.

* $P \leq .05$.

** $P \leq .01$.

vancement and are therefore expected to change employers frequently as they participate in occupational internal labor markets (see Kerr 1954; Althauser and Kalleberg 1981).

This interpretation is supported by our results for the occupational measures. Industries which sell goods to the state are associated with higher levels of occupational skill and licensing. Moreover, establishment size has a negative effect on both skill and licensing; this may be interpreted as reflecting the consequences of the structuring of work in large organizations, which often results in a detailed division of labor (e.g., Edwards 1979; Braverman 1974). Economic scale has a positive effect on both occupational dimensions, while capital intensity has negative effects on skill (for females) and licensing. Finally, concentration has a positive effect on skill (for males) but a negative effect on licensing.[11]

Finally, we note that our indicators of economic segmentation are only weakly related to membership in manager-class positions. This is consistent with our expectations. We find, however, that women in concentrated and state-supported industries have lower probabilities than men do of being in managerial positions, while women in larger establishments are more likely than are men to be managers.

The R^2 values for the worker power equations indicate that a considerable portion of the variance in the worker power measures is unrelated to the economic segmentation variables. This supports our argument that the correspondence between these sets of concepts is not as strong as has generally been assumed in much of the dual economy literature. While the relationships between economic segmentation and worker power are intrinsically interesting, however, our primary purpose for investigating them

[11] In order to examine the relationships between economic segmentation and occupational structure more closely, we assessed the association between our economic segmentation measures and membership in various occupations. That is, we divided the sample into 10 occupational groups: professionals, semiprofessionals and technicians, managers, office clericals, nonoffice clericals, sales workers, craftsmen, operatives, service workers, and laborers (see Freedman [1976] for details of this classification). Then, we regressed a dichotomous variable, indicating whether a respondent was a member of the particular occupational group, on the five economic segmentation variables. This analysis, not presented here for reasons of space, yielded the following results. First, industries whose firms sell more goods to the state have higher proportions of professionals and craftsmen and lower percentages of managers and nonoffice clerical, sales, operative, and service workers. Second, establishment size was positively associated with professionals, semiprofessionals, and operatives but negatively related to managers, sales workers, craftsmen and laborers. Third, concentration was positively related to office and nonoffice clerical, craft, and operative occupations. It was negatively associated with professional, semiprofessional, sales, and service occupations. Fourth, economic scale was associated with professionals and semiprofessionals, but negatively related to operatives and laborers. Finally, capital intensity had positive effects on membership in managerial, sales, craft, operative, and laborer occupations; and negative effects on office clerical and service occupations.

in this paper is to be able to assess the ways in which these sets of concepts combine to influence income inequality. One possible mechanism is suggested by our conceptual model, which implies that part of the effects on income of the economic segmentation variables may be indirect via the worker power measures. We can compute these indirect effects from the information contained in tables 2 and 3. These indirect effects are summarized in the last column of table 3, which indicates the amount of the total effect of an economic segmentation variable that is mediated by all of the worker power measures (compare cols. 2 and 3 of table 2). A negative sign for this quantity indicates that the indirect effect is negative.

We draw several conclusions from this analysis. First, about 25% of the effect of concentration on income (for males) is mediated by the worker power measures, primarily by unionization. Second, the vast bulk of the total effect of state purchase on income (for females) is indirect via the attributes of worker power (about 79%). Finally, we observe that relatively little of the total effect of establishment size is mediated by the worker power variables.

Effects of Economic Segmentation on Income Inequality: Interaction Effects

In our final analysis, we modify our additive model to include interactions involving the economic segmentation measures. These interactions are presented in table 4. Our procedure for assessing interactions involves adding multiplicative terms to our basic additive model (col. 3 of table 2). Interaction terms are computed as the products of an economic segmentation measure with education and the worker power variables. Each equation thus contains six interaction terms as well as the main effects of the variables represented in the third column of table 2. We examine interactions involving each economic segmentation variable separately; a simultaneous consideration of interactions involving all five segmentation measures is precluded because of the excessive collinearity among the variables, with its consequences for the standard errors of coefficients and for statistical power.

The results of three types of statistical comparisons among coefficients are presented in table 4. We assessed directly the significance of the coefficients for the six interaction terms within each equation by a series of t-tests. We also report the F values for tests of the global significance of the six interaction terms for each equation; these are based on a comparison of the (unadjusted) R^2 values for the additive and interactive models. Finally, we present the results of a series of t-tests comparing differences between males and females with respect to the six multiplicative terms for

TABLE 4

UNSTANDARDIZED COEFFICIENTS FOR INTERACTIONS INVOLVING ECONOMIC SEGMENTATION VARIABLES IN SWC AND QES DATA
(N = 1,326, Males; N = 806, Females)

INDEPENDENT VARIABLE	ECONOMIC SEGMENTATION MEASURE[a]									
	Concentration		Economic Scale		State		Capital Intensity		Size of Establishment	
	M	F	M	F	M	F	M	F	M	F
Intercept	7.677	7.042	7.667	6.948	7.582	6.970	7.645	7.037	7.604	6.727
Education	.097 (.010)**	.096 (.018)**	.094 (.010)**	.118 (.015)**	.101 (.010)**	.105 (.016)**	.101 (.010)**	.093 (.018)**	.089 (.018)**	.168 (.029)**
Education × ES measure	−.006 (.003)***	−.011 (.006)*	−.012 (.010)	.002 (.014)	−.006 (.006)[b]	.052 (.012)**	.004 (.010)[b]	−.042 (.018)**	.002 (.005)	−.017 (.008)*
Economic segmentation measure[c]	.059 (.023)**	.092 (.030)**	.039 (.065)	.050 (.097)	.144 (.035)**	−.182 (.066)**	−.094 (.066)	.183 (.094)*	.065 (.035)***	.143 (.050)**
Union (= 1)	.082 (.028)**	.152 (.041)**	.062 (.029)*	.201 (.039)**	.078 (.031)**	.198 (.042)**	.086 (.028)**	.157 (.043)**	.259 (.056)**	.389 (.085)**
Union × ES measure	−.018 (.010)***	−.026 (.013)*	−.075 (.029)**[b]	.015 (.040)	.015 (.016)[d]	−.032 (.030)	.030 (.033)[d]	−.053 (.044)	−.051 (.014)**	−.058 (.021)**
Manager (= 1)	.122 (.027)**	.091 (.040)*	.116 (.028)**	.096 (.037)**	.110 (.029)**	.100 (.036)**	.129 (.027)**	.077 (.039)*	.118 (.027)**	.127 (.073)***
Manager × ES measure	−.004 (.010)	−.002 (.014)	−.027 (.028)	.025 (.042)	.013 (.017)	−.016 (.028)	−.028 (.031)	−.035 (.038)	−.0001 (.014)	−.010 (.021)
Occupational SVP	.044 (.009)**	.094 (.013)**	.047 (.009)**	.094 (.014)**	.058 (.010)**	.100 (.013)**	.045 (.009)**	.098 (.015)**	.070 (.017)**	.118 (.025)**
Occupational SVP × ES measure	.0008 (.003)	.0005 (.005)	.004 (.009)	.005 (.014)	−.016 (.005)**	−.015 (.011)	.027 (.010)**	.007 (.015)	−.008 (.005)***	−.008 (.007)
Occupational licensing	.0009 (.0008)	.002 (.001)	.001 (.0007)	.003 (.001)*	.001 (.0008)	.003 (.001)**	.001 (.0007)***	.002 (.002)	−.0007 (.001)	−.0002 (.002)
Occupational licensing × ES measure	.0003 (.0003)[d]	−.0005 (.0005)	−.0007 (.0008)	−.002 (.002)	.0004 (.0005)[b]	−.002 (.001)***	−.0005 (.0008)	−.002 (.002)	.0005 (.0004)[d]	.001 (.0008)
Employer tenure	.042 (.008)**	.039 (.012)**	.043 (.008)**	.036 (.011)**	.044 (.009)**	.032 (.011)**	.042 (.008)**	.038 (.012)**	.027 (.014)*	.010 (.020)
Employer tenure × ES measure	−.001 (.003)	−.0006 (.003)	.004 (.007)[b]	−.021 (.010)*	−.004 (.004)[d]	.015 (.008)*	−.003 (.008)	−.003 (.010)	.005 (.004)	.009 (.005)***
R^2 (adjusted)	.331	.401	.332	.399	.335	.407	.332	.400	.337	.404
F-test for increment to R^2[e]	1.29	1.77	1.51	1.49	2.60*	3.50**	1.45	1.74	3.00**	2.71*
Standard error of estimate	.439	.429	.439	.430	.438	.427	.439	.429	.438	.428

NOTE.—Standard errors are in parentheses.

[a] Equations control for race, south, age, and the four economic segmentation variables not involved in interactions.

[b] t-test for sex interaction: $P \leq .01$.

[c] Represents economic segmentation measure involved in interactions.

[d] t-test for sex interaction: $P \leq .05$.

[e] Test based on comparison of unadjusted R^2s; degrees of freedom for F-tests: 6,1311 (males), 6,791 (females).

* $P \leq .05$.

** $P \leq .01$.

*** $P \leq .10$.

each economic segmentation measure. Let us turn to an examination of these interactions.

We observe initially that the interaction terms, taken as a whole, generally add little explained variance to that accounted for by the additive model (see global *F*-tests). This lends support to our previous analyses, which assumed additive effects of the economic segmentation and worker power variables on income; this is, furthermore, not surprising in view of the low statistical power to detect interactions that undoubtedly characterizes the specification of the model.

Nevertheless, a number of theoretically important interactions are statistically significant in these equations. These would often be overlooked by a global (nonsignificant) *F*-test, although individually they represent important relationships consistent with our previous discussion and the concerns expressed in hypothesis 4. Union membership, for example, interacts negatively with concentration (Weiss 1966; Dalton and Ford 1977) and establishment size for both males and females, and negatively with economic scale for males. This suggests that in these contexts, employers may have (and be willing to use) sufficient power to resist union wage pressures. Additionally, such firms may not be concerned about further unionization and, thus, have no incentive to pay high wages to forestall future worker organization. It is interesting that occupational skill interacts positively with capital intensity for males. The latter relationship may reflect the observation that since capital intensity implies greater investments by employers in nonlabor factors of production, a particular level of skill may involve a relatively greater responsibility over the production process for workers in capital intensive industries and thus lead to greater rewards (e.g., Edwards 1979).

Relatively surprising is the finding that employer tenure interacts only weakly with establishment size, our closest indicator of (firm) internal labor markets. Further, employer tenure does not interact with concentration, which is also related to the presence of internal labor markets. Taken together with our previous results, these findings suggest that size and concentration produce greater levels of employer tenure and thereby increase income; little support is provided here for the notion that employer tenure provides greater economic returns in larger organizations and/or concentrated industries (see also Alexander 1974).

Of considerable interest here are the interactions between education and the economic segmentation variables. We note that education interacts positively with state purchase of goods in the prediction of log income for women. On the other hand, all the other significant interactions of education with the economic segmentation variables are negative.

Our most surprising finding here, in view of Stolzenberg's (1978) pre-

vious analysis of the QES data for white males, is that education interacts negatively with size for females and does not interact with size for males. To assess whether this discrepancy is due primarily to differences in equation specification, we reestimated our models for white males ($N = 1,201$) and white females ($N = 687$): for white males, the education $\times$ size interaction was positive (unstandardized coefficient/standard error = .010/.005) and significant at a P level of .036 when this term was added to our basic additive model; for white women, the education $\times$ size interaction remained negative and significant.[12] Thus supplementary analysis suggests that the argument that education provides greater economic returns in larger establishments holds only for white males. For females (and perhaps nonwhites), in contrast, education appears to pay off more in smaller establishments. These divergent results point to the need for further research on how economic segmentation conditions the relationship of education to economic success.

CONCLUSIONS AND IMPLICATIONS

How organizational and industrial differences generate income inequality is a central problem for theory and research on economic and social organization and on socioeconomic achievement. In this paper, we addressed several issues concerning the consequences of economic segmentation for earnings. We argue that the structure of economic segmentation is multidimensional and is derived from distinct factors such as concentration, state intervention in the market, organization size, capital intensity, and economic scale. Our empirical results indicate that our measures of three of these concepts (concentration, state intervention, and establishment size) have significant net positive effects on income, suggesting the utility of our multidimensional model. We also argue that worker power comprises a distinct set of concepts from economic segmentation and is derived from diverse sources such as union membership, tenure with an employer, occupational skill and licensing, and manager-class position. In support of our argument that these attributes constitute differing dimensions of worker power, our analysis found that measures of all of these concepts had significant net positive effects on income.

We then examined the links between economic segmentation and worker power. Consistent with dual economy theory, we found that the economic

[12] Wright and Perrone (1977) found a positive interaction between manager class and greater economic returns to education in these data. We tested for such an interaction by adding a manager $\times$ education term to our equations. This term was nonsignificant for both men and women, reflecting primarily, we assume, differences in equation specification between ourselves and Wright and Perrone.

stratification of firms and industries was associated with some forms of worker power, particularly union membership, employer tenure, and occupational skill and licensing. The signs of these effects varied, however, as did the magnitudes of the relationships. However, for males, manager-class position was unrelated to our economic segmentation measures. These observed patterns highlight the complexities involved in the relationships between economic segmentation and worker power. Dual economy theory provides an explanation for the correspondence between economic segmentation and only some forms of worker power; an understanding of the sources of other worker power attributes requires incorporation of insights from other approaches. Our results suggest a need for extending the dual economy perspective on this issue and indicate the types of relationships that require further research (e.g., how class positions vary among firm and industrial contexts).

Our analyses also revealed the presence of several interaction effects between economic segmentation and worker power measures in the prediction of income. These were generally small, however, supporting our additive specification of these relationships. In particular, union membership interacted negatively with concentration, size, and economic scale (for males). We also found that education interacts negatively with several dimensions of economic segmentation, especially concentration and establishment size (for females). These interaction effects support the view that the income determination process differs in diverse firm and industrial contexts, although further research is needed to specify more exactly the nature and interpretation of these nonlinear relationships. We also assessed differences in the relationships among economic segmentation, worker power, and income inequality for males as opposed to females. We found evidence that the processes of income determination differ by sex; our investigation of these interaction effects by sex has been largely exploratory, however, and more systematic research is required.

In general, our empirical results illustrate the utility of an economic segmentation approach for explaining structural sources of income inequality. Furthermore, our conceptual model suggests a number of important hypotheses that constitute an agenda for future research to increase our understanding of these structural bases of stratification. Our analysis has been grounded in insights derived from dual economy theory, but we have supplemented this theory with ideas obtained from other approaches to the problem of income inequality, such as status attainment research, Marxist class analysis, organization theory, and institutional economics. Thus, our conceptual model is consistent with a variety of theoretical interpretations. An advantage of our framework is that it suggests numerous empirical relationships that provide evidence on the ability of concepts de-

rived from various paradigms to account for the determinants of economic inequality. These include links among employers, between employers and employees, and among employees themselves.

Several limitations of our analysis should be considered when evaluating our results. First, we would have preferred to have measured all economic segmentation concepts at both industrial and firm levels of analysis and to have had more precise information on authority, occupational position, and internal labor markets. Second, the investigation of the issues raised would benefit considerably from the analysis of longitudinal data, both on jobs and job structures and on the career income trajectories of individuals. Third, our analyses have been restricted to employees in the private sector of the economy. Investigations of the interplay among economic segmentation, worker power, and income inequality should also consider the public sector. Fourth, our measure of inequality was limited to income earned from the job; determinants of other forms of economic inequality, such as fringe benefits and unearned income, also require investigation. Additionally, it is necessary to examine the effects of economic segmentation and worker power on noneconomic job rewards such as those intrinsic to the performance of the job activity.

However, our principal purpose has been to make explicit some key questions in the analysis of economic segmentation and its consequences for inequality. We leave many issues unresolved. These include the ways in which jobs and job structures should be conceptualized with respect to income determination, and how these structures are related to organizational, industrial, and macroeconomic dimensions of production systems. Links between these structures and the careers of individuals, as well as race and sex differences, also require further research. We have argued that explanations of such phenomena require insights from several theoretical perspectives and have based our analysis on ideas drawn from dual, radical, and neoclassical models (Gordon 1972). Integration of these perspectives is necessary if we are to account for the interplay among economic segmentation, worker power, and individual inequality, a topic of increasing importance for understanding the nature of stratification systems.

APPENDIX

Construction of Economic Segmentation Variables

The Appendix table describes the construction of the four industrial level economic segmentation variables. These measures were originally used by Wallace and Kalleberg (1981). For each of the variables, we list their

components (which were combined with the use of Z scores) as well as the sources for each component.

Indicator	Description	Source
Concentration:		
CONRECT	Sales receipts by firms with over $100 million assets divided by total sales (1970)	1
ASSRECT	Sales receipts by firms with over $5 billion in sales divided by total sales	1
FOURFIRM	Four-firm concentration ratio (1966)	4
Economic scale:		
AVGASST	Average assets for all firms (1970)	1
AVGRECT	Average sales receipts for all firms (1970)	1
AVGUPROF	Average undistributed profits for all firms (1970)	3
State purchase of goods:		
PCTTOTAL	Percentage of total industry goods and services purchased by the state (1967)	2
PCTFINISH	Percentage of finished industry goods and services purchased by the state (1967)	2
Capital intensity:		
CAPINT	(Total cost of sales and operations *minus* total labor costs) divided by (number of employees)	1, 3

SOURCES.—1: Department of the Treasury, *Statistics of Income: Corporation Returns, 1970.* (Washington. D.C.: Government Printing Office, 1974). 2: Department of Commerce, *Input-Output Structure of the U.S. Economy, 1967* (Washington, D.C.: Bureau of Economic Analysis, 1974). 3: Department of Commerce, *National Income and Product Accounts of the United States, 1929–1974: Statistical Tables* (Washington, D.C.: Bureau of Economic Analysis, 1976). 4: William G. Shepherd, "Market Power and Racial Discrimination in White Collar Employment," *Antitrust Bulletin* 14 (1969): 401–32.

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Toward a Theory of Mobility and Behavior in Organizations: An Inquiry into the Consequences of Some Relationships between Individual Performance and Organizational Success[1]

David Jacobs
University of Maryland, Baltimore County

In this paper I will show that various positions within organizations put different limits on the relationship between individual performance and organizational success. Some of the consequences of these curvilinear relationships will be illustrated by describing three common types. After specifying the major determinants of these three types, I will use them to help explain four outcomes. The relationships provide an explanation for important aspects of mobility systems within organizations. Additional consequences include the probability that workers will be sympathetic to collective bargaining and horizontal movements between organizations. The three relationships between individual and organizational performance also act as a primary determinant of control processes within organizations and the individual adaptations that result.

It has become a truism that we live in an organizational society in which social mobility and other rewards are determined largely by processes within formal organizations. Until recently, however, most of the research that has been done on mobility and stratification has ignored organizational mobility systems and concentrated on other matters. For example, the statistical relationships between background characteristics and current position have been intensively investigated (Blau and Duncan 1967; Duncan, Featherman, and Duncan 1972), but the organizational processes that intervene between starting points and final positions have been given comparatively little attention. For the most part, studies in the Blau and Duncan tradition have treated mobility systems within organizations as unmeasured intervening variables which could be safely ignored.

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There are some notable exceptions to this generality, of course. Recently, Pfeffer (1977), Halaby (1978), and Rosenbaum (1979*a*, 1979*b*) have sought to fill this gap in the literature with empirical examinations of mobility processes in organizations. This work was preceded by attempts to test stochastic models of mobility with data on organizational careers (White 1970*a*, 1970*b*). Other partial exceptions can be found in the literature on occupations and professions. Descriptive studies of careers and mobility within organizations are both numerous and useful (Goldner 1965; Roth 1963; Becker and Strauss 1956; Martin and Strauss 1956; Berlew and Hall 1966; Smigal 1965).

Unfortunately, there has been little or no use of organizational theories in any of these traditions. In fact, as Bucher (1977) has pointed out, we do not have a framework which provides a link between investigations of occupational careers and our theories about how complex organizations will behave. This gap is unfortunate for additional reasons. A good theory of mobility and stratification within organizations should also help to explain the actions of organizations and the people in them. Mobility, after all, is an important incentive for individuals. Therefore, a more complete theoretical understanding of various mobility systems within complex organizations should tell us more about what people in organizations are likely to do and how organizations will respond.

In this paper I will attempt to develop a theoretical explanation of some aspects of mobility and stratification processes in complex organizations. I will also show how this framework can be used to explain other aspects of organizational behavior. The argument will proceed as follows. First, I will spell out three relationships between the performance of individuals and organizational success. Next, I will describe some attributes of organizations that lead to these three relationships. After that, the relationships between individual performance and organizational success will be used to explain some characteristics of (1) internal mobility systems, along with (2) internal allocative processes and collective bargaining, and (3) the control processes and individual adaptations that follow from these three sets of constraints on organizational positions and the people who work within them. Finally, the needs of organizations and societies may often diverge. This divergence can be used to show how the theory has wider implications. One implication which will be dealt with at the end of the paper concerns a fundamental weakness in the functional theory of stratification.

Before beginning these tasks two caveats are in order. First, because I am trying to develop a new theory and demonstrate its utility in a limited number of pages, it will not be possible to devote much space to the organizational processes that are explained by the model. This omission does not mean that I am advocating a single-factor explanation. Instead,

even when it is not explicit, the reader should always assume that ceteris paribus assumptions are in force. Second, to demonstrate the utility of the model I will often use examples, but these examples are only illustrative. While no empirical test will be provided, some ways that the model can be operationalized will be discussed at the end of the paper (see n. 18 below).

THREE RELATIONSHIPS BETWEEN INDIVIDUAL AND ORGANIZATIONAL PERFORMANCE

In an innovative short article Stinchcombe (1963*b*) argued that "in some industries individual talent is more of a *complementary* factor of production in the sense that it makes other factors more productive; in others it is more frequently additive" (p. 805). Much of his subsequent discussion of this distinction was confined to illustrations. Research positions within universities or starring roles in movies, for example, were said to be talent complementary because an outstanding individual performance can increase the success of the enterprise by a factor as great as two or three. On the other hand, the best performance of an outstanding house painter or teacher can add only two or three percentage points to the total productivity of the enterprise.

Stinchcombe's imprecision here left room for criticism. Huaco (1966), for example, argued that Stinchcombe confused training with functional importance. Moreover, his emphasis on the distinction between interactive effects (talent complementarity) and additive ones is probably misleading. It is easy enough to imagine a strategically placed individual in a large organization whose outstanding individual achievements add a significant increment to the performance of the total enterprise, but it is unlikely that his unique contribution will double or triple the productivity of such a large collectivity (see Stinchcombe and Harris [1969] for a correction of some of these difficulties along with an attempt to formalize the idea with a mathematical model).

While there are difficulties with Stinchcombe's initial formulation, some modifications can widen its applicability. If the interactive aspect is dropped and emphasis is clearly placed on positions within organizations, an expanded version becomes even more useful. It is best to begin this expansion by putting Stinchcombe's original idea into propositional form. Therefore, it can be said that *an exemplary individual performance in some positions adds a significant increment to an organization's total performance, but an exemplary individual performance in other positions will have little effect on the total performance of the organization.*[2] For exam-

[2] My use of the term organizational performance follows Hannan and Freeman (1977), who appear to restrict this concept to an important measurable output. For examples

ple, Stinchcombe implies that in academic positions within research universities, the scholar who wins a Nobel prize will make a significant contribution to his institution's ability to command scarce resources from its environment. Someone in a quite different position, however, such as the instructor in a junior college whose teaching is just as superlative as the Nobel prize winner's research, will have little effect on his organization. Figure 1*a* and *b* illustrates these two relationships.

Stinchcombe's insight can be expanded by looking at the opposite relationship between individual performance and organizational accomplishments. In many organizational positions an outstanding individual performance has a weak or nonexistent relationship with organizational performance, but if an occasional individual performance is well below average, the organization will find that total production is significantly lower or important goals are jeopardized. The airline pilot who misses a landing or

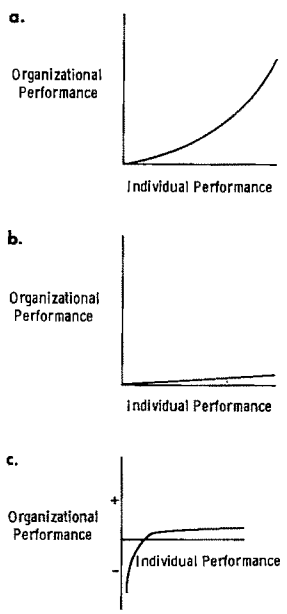


FIG. 1.—The three relationships between individual performance and organizational success. (These relationships have been deliberately exaggerated to illustrate their differences.)

of empirical studies consistent with this usage see Pennings (1975, 1976), Pfeffer (1972), and Lieberson and O'Connor (1972). These researchers used such indicators as total production, stockholder's return on equity, and market share. The performance of nonprofit organizations has been measured by prestige (Cartter 1966). For a more complete discussion of these issues see Goodman and Pennings (1977) and Steers (1975).

the operative who inadvertently blocks a long assembly line will produce rather destructive effects, but an outstanding performance in either position will be of little consequence for the organization. Thus, it can also be said that *in some positions an exemplary individual performance can add little to an organization's total performance, but the occasional individual performance that is well below average will reduce the total performance of the organization by a significant amount* (see fig. 1*c* for an illustration of this relationship).[3]

In what follows I will attempt to show that the three relationships in figure 1 have numerous consequences for organizations. Because the utility of the model is at issue and space is limited, it will be most useful to concentrate on an inventory of effects. Therefore, I will not be able to provide an extensive analysis of either the prior causes of these three relationships or the additional factors that cause those aspects of organizations that are to be explained (see Blalock [1969, p. 41] for a justification of this strategy and some examples). But if the argument is not to appear tautological, it will be useful to delineate the major factors that lead to the three linkages depicted in figure 1.

Determinants of the Relationship between Individual and Organizational Performance

One relevant factor is the type of interdependence within organizations. Many positions are linked together sequentially so that one actor's output is input for another who passes his output to a third (Thompson 1967). As long as alternative paths do not exist, sequential interdependence between positions means that the chain of productivity can only be as strong as its weakest link. The exceptionally able worker on the assembly line, for example, can only produce at the pace of the slowest unit on that line, although an individual performance that is well below average will result in significantly less collective output. But when a number of actors perform identical tasks at the same time so that interdependence between positions is pooled (Thompson 1967), the negative effect of a bad performance will be less damaging to the internal productivity of an organization. Here the productivity of the group will not be limited by the perfor-

[3] At least two authors besides Stinchcombe have suggested that the relationships in fig. 1*a* and *b* have important consequences. Blalock (1961) argued that in sports and entertainment the costs of discrimination are high because there is a close relationship between individual performance and collective success in both endeavors. In a similar vein, Thurow (1975) claimed that the marginal productivity of positions was more important than conventional neoclassical explanations in economics which emphasize individual productivity differences. As far as I know, however, there is no detailed treatment of the opposite relationship that occurs when individual contributions to organizational success are limited but a poor performance will be costly.

mance of any one actor. It follows that sequential interdependence leads to the linkage described in the last proposition (see fig. 1*c*), but pooled interdependence makes that relationship less likely.[4]

If we look at the probability of individual success in different organizational positions, another determinant can be isolated. In some positions properly qualified individuals rarely fail so there can be little difference between an average performance and a superlative one. When most incumbents succeed 98% or 99% of the time, it will be quite possible to fall well below this norm, but there will be a mathematical ceiling on the difference between exemplary accomplishments and the median performance. If important organizational work flows pass through such positions (see Hickson et al. [1971] for treatment of this factor), the occasional individual performance that is well below average will probably be rather costly for the organization. In any case it will be difficult for individuals to add significantly to organizational accomplishments in positions where the average rate of success is very high. Even if the professional pilot never makes any mistakes in a long career, his company's profits will not go up, but one error may be quite costly to his airline.

By the same token, if the average success rate in an organizational position is low so that most incumbents can succeed only 5% or 10% of the time, a superlative performance can be much greater than the norm. If such positions are also strategically located so that crucial organizational uncertainties and dependencies can be controlled, a superb individual performance will make a significant contribution to the success of the entire organization (for a delineation of environmental dependencies see Jacobs [1974]; for a discussion of internal uncertainties and dependencies see Hickson et al. [1971]). For example, someone in a sales or academic position whose achievements reach levels which are rarely attained will often add an important increment to his organization's total performance. Note also that in positions where success is infrequent, organizations will be able to tolerate performances that are below the mean as long as a few heroic individuals exceed this norm by a greater amount. It follows that as long as the distribution of individual performances is skewed or log normal (and this will often be the case when success rates are low), a suboptimal performance will not be very costly for the organization. Of

[4] That specific aspects of an organization's technology are partial causes of the relationships in fig. 1 does not mean that these particular technological choices must have been selected because they led to greater efficiencies. Both Braverman (1974) and Pfeffer (1978) show how decisions about technology can be made because a particular outcome heightens the political resources of one set of actors even though net efficiency is reduced. But regardless of the reasons, once a particular technological option is selected and it becomes costly to change, this choice will often have a significant effect on the relationship between individual and organizational performance and its consequences.

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course, positions where success is infrequent must be arranged so that interdependence is pooled if a few gifted individuals are to be able to make up for inferior accomplishments of others.

Thus, it can be said that positions with high success rates put limits on the contribution that individuals can make to organizational success (fig. 1*b*), but if important work flows pass through these positions, a very poor performance will be costly for organizations (fig. 1*c*). In positions where success is comparatively infrequent, however, a close relationship between superlative individual accomplishments and organizational performance is likely (fig. 1*a*) as long as the position is located so that it can absorb crucial organizational dependencies. It is also probable that a poor individual performance will not be very costly under these conditions.

The degree to which collective efforts are subject to measurement is a third determinant. Positions where an individual performance can only hurt an organization are often found in organizations that must operate under similar constraints. Many nonprofit organizations are unable to receive much credit for an exemplary performance because it is largely immeasurable. Little can be known about the comparative success of a specific police department, prison, or welfare agency. Mistakes, however, are often quite visible. Thus, the occupant of a position in such organizations will find that it is difficult to make a positive contribution to collective success, although a subpar performance may lead to disastrous results for the whole collectivity (see fig. 1*c*).

The relationship between individual and organizational performance is also contingent on the presence of large subunits or departments within organizations. If individuals can have only an indirect effect on organizational performance because of their impact on subunits, the relationship between subunit performance and the performance of the total organization becomes important. This second relationship may be attenuated by several conditions: the particular tasks performed by subunits may not be crucial to the performance of the total organization (Hickson et al. 1971), or performance may be immeasurable at the subunit level. One reason for the latter possibility is that mutually dependent relationships with other units can make success or failure difficult to gauge. For example, when two subunits use the same facility, the precise allocation of costs may present an insurmountable problem.[5] But regardless of the reasons, as long as influence must pass through subunits, the impact of individuals on the

[5] Performance at the individual level also may be difficult to assess, although this condition is less likely when interdependence between positions is pooled. Note, as well, that when performance is measurable at a higher level, it may be possible to substitute different incumbents and examine the resulting changes in productivity. This practice is frequently used in team sports like rowing because the only measure of an individual's unique contribution is comparative.

total performance of an organization will be limited by the relationship between subunit and organizational performance.

Thus, four factors which lead to the relationships between individual and organizational performance have been isolated. These are (1) whether interdependence between positions is pooled or sequential, (2) whether individual success in various organizational positions is common or infrequent, (3) whether positions are located in organizations which cannot receive much credit for an exemplary performance, and (4) whether positions are located within subunits whose performance has limited effects on the performance of the total organization.

With this treatment of the determinants of the relationships in mind it is now possible to explore some consequences of the model. It is best to start with the connections between the three relationships and individual mobility.

CONSEQUENCES FOR MOBILITY AND STRATIFICATION

Career Mobility

One generality which emerges from studies of decisions about employment is that the future performance of individuals is very difficult to predict. Many researchers have found that employers use such crude factors as education and race to screen potential employees because accurate predictors are not available (Doeringer and Piore 1971; Berg 1970; Arrow 1973; Collins 1971). In the great majority of cases the only reliable indicator of future performance is past performance in a similar position.

This uncertainty has important consequences for careers in organizations, but these consequences depend on the relationship between individual performance and organizational success. For positions where a suboptimal performance can do a great deal of harm but the effects of an exemplary performance are limited (fig. 1*c*) the potential costs of promoting an inappropriate candidate are great. Because mistakes can be extremely costly and because direct experience with an individual's daily behavior is the only reliable predictor of his future performance, the rational strategy for an organization under these constraints is to minimize risk by promoting only after an extended look at each candidate's record. Remember as well that the rate of success in such positions will often be high so that even the most unreliable individual will make infrequent errors. It follows that long evaluation periods before promotions are the best insurance that unreliable individuals will not be advanced to positions where they can do more harm to collective endeavors.

When organizational positions are subject to these constraints so that mistakes are extremely expensive but a superlative performance does not

matter, lateral moves between organizations will be unlikely for the same reasons. Organizations that ignore these conditions and fill responsible positions from the outside will have no direct experience with new employees and little reliable information about their past. As a result, it will be easy to fail to reject an inappropriate candidate who is prone to costly errors. Therefore, if other factors are held constant, it can be said that *to the extent that a poor individual performance can hurt collective endeavors but an exemplary individual achievement will not matter* (fig. 1*c*), *careers will begin at the lowest positions and progress slowly.*

For example, consider positions in metropolitan police departments. Police chiefs are not fired because they fail to control crime; they are fired because of scandal (Wilson 1971).[6] It follows that a patrolman's capacity for harm far outweighs his ability to help the career of supervisors. Corruption that is documented by outsiders or the visible use of inappropriate force may undermine a department's reputation and do serious damage to a supervisor's career (Daley 1973). As one would expect from the previous discussion, supervisors operating under these constraints invariably promote from within after a long look at a candidate's behavior. Within work groups, the neophyte patrolman must undergo an extensive informal probation before he is trusted by his immediate supervisor (Rubenstein 1973). The result is that a typical career in police administration involves long periods spent at each level in a department. Even though chiefs may occasionally move from one department to another, the great majority of top administrators have spent many years toiling in the ranks of the department they now lead.

Perhaps the role occupied by airline pilots is the most extreme example of a position where the potential for harm is greater than an ability to further collective efforts. Because it is vital to ensure that performance in these positions will be free from errors, pilots tend to be evaluated on the basis of mistakes and rule violations (Roth 1963). It follows that the occupants of these positions are forced to spend many years under the immediate direction of a more experienced pilot before uncertainties about their reliability and consistency can be completely resolved. Roth also notes that there has been a growing tendency to stretch out the earlier

[6] Many factors combine to give police administrators little control over the amount of crime in their districts. The police have infrequent access to private areas (Stinchcombe 1963*a*), and their number will always remain small compared with the number of potential offenders. These handicaps make the discovery of illegal behavior or its intent very difficult because a patrolman can make decisions about who should be stopped only on the basis of clearly apparent anomalies in a suspect's behavior (Rubenstein 1973). Such cues are much too crude to interfere with the activities of most law breakers. Therefore, if he is to make an arrest, the policeman must arrive on the scene of a crime within seconds. Otherwise, the typical offender will quickly disappear (Jacobs 1979). It follows that department policies have almost no effect on the number of crimes in an area.

phases of a pilot's career. Of course, exogenous factors such as the industry's slower growth in recent times may account for some of this effect. But as long as an inept performance counts more than one that is exemplary, it is to be expected that organizations will attempt to minimize risk by moving to longer evaluation periods before they promote.[7]

When the constraints on positions are reversed so that an exemplary individual achievement has a significant impact on organizational performance but a poor individual performance does not matter (fig. 1*a*), careers within organizations will be quite different. Because the ability to predict future performance is limited but a weak performance will not be terribly costly, the organization which seeks to maximize the probability that goals will be attained will evolve a system in which many are called but few are chosen. As long as candidates are not scarce, to maximize the probability of finding the best, many will be given the opportunity to demonstrate their skills but only the most successful will be given any guarantee of future security.

Because organizations dominated by these positions are engaged in a continuous search for talent and poor performances do not matter, lateral movements between such organizations also should be common. It follows that second chances with a different organization will be more likely under these conditions, but this will not be true when the constraints on positions are reversed and mistakes become expensive. Therefore, if other factors are held constant, it can be said that *to the extent that an exemplary individual achievement can help collective efforts but a poor individual performance will not matter* (fig. 1*a*), *organizations will hire many individuals but attempt to retain only those few whose performances reach the highest levels.*[8]

Consider, for example, positions in sales. Because the incumbents of these positions control an environmental dependency that is crucial for

[7] For a third example consider positions in organizations employing continuous-process technologies, such as steel mills or oil refineries, where the primary technology is sequentially interdependent. According to Doeringer and Piore (1971), promotion seems to occur only from within in both industries. These examples provide more evidence for my argument since individuals can only hurt the productivity of a group when interdependence is sequential. In both instances accidents can be very costly for other reasons, but this fact also supports the argument. Some additional examples include umpires in professional baseball and most positions where incumbents are responsible for the day-to-day operation of expensive but delicate machinery. Here the previous hypothesis also seems to explain career progressions.

[8] One exception to this hypothesis occurs when position incumbents have such a strong professional association that they can conceal their mistakes (Goode 1957). In this case an exemplary individual achievement may have a significant effect on organizational performance, although mistakes will not matter (fig. 1*a*). But since the employing organization is weak with respect to the professional association, it will not be able to give many individuals a tryout and discard the majority who are not able to reach the highest standards.

many organizations (Jacobs 1974), the first-rate salesman can often add a significant increment to the performance of his organization while his inferior will not impose unacceptable costs. But it is almost impossible to predict success in this endeavor. Therefore, automobile dealers, insurance companies, and stock brokerage houses respond to these constraints by taking on new personnel fairly indiscriminately, but the novice salesman is given only a limited time to produce. The result is that there tends to be a continuously rotating pool of newcomers who stay with the organization for short periods of time, while those few who manage to be successful receive large rewards and some guarantee of future security.[9]

It should come as no surprise that the same argument also applies to academic positions in universities. Here the productive scholar adds a significant increment to organizational prestige, and prestige is the primary tool that elite universities use to extract vital resources from their environment (Riesman 1956). But productivity per staff member evidently is not the most important consideration. The prestige rankings compiled by Glenn and Villemez (1970) show that departments with a few highly productive scholars seem to do much better than departments with higher per capita publication rates but no superlative performers. It follows that as long as a few highly productive scholars are in residence, some weak performances can be tolerated without any loss in prestige. Thus, since research prowess cannot be estimated in advance, the most rational strategy for the ambitious department is to use the revolving door, hiring many assistant professors but allowing only the very best to achieve tenure. If this strategy is ruthlessly pursued there will be some unknowns on the faculty at any given time, but the probability that a few famous scholars will always be in residence will be maximized.[10] Therefore, in positions governed by these constraints, the below-average performer will always impose opportunity costs. These costs will be minimal, however, compared with the gains that will be realized from the rare individual with stellar abilities who can be found only by giving many individuals a chance.

[9] The fact that incentives are often designed to facilitate this turnover does not invalidate the argument.

[10] For another example of the explanatory power of the previous hypothesis consider Peterson and Berger's (1971, p. 102) description of positions in the record industry. "Like recording artists, the new producer is given a short term contract while the company retains first option to renew his contract; in this way the company can keep the successful producer from going to a rival company while quickly getting rid of those who lose the magic touch. The producer ordinarily receives a salary around $15,000 but this can increase fivefold if his records are successful in the market. Thus, the successful producer is handsomely rewarded and the ineffective producer can be (and often is) discharged on short notice." Moreover, since interdependence between these positions is pooled, the less successful producer will not impose heavy costs. Some additional examples of this kind of position are grants administrators in large organizations supported by soft money and hairdressers in neighborhood studios.

Other attributes of mobility structures follow from these two sets of paired constraints on organizational positions. A major reason for locating positions next to one another in mobility clusters is to maximize training so that in the ideal case each job in a mobility route offers complete preparation for tasks at the next higher level (Doeringer and Piore 1971). Organizations also construct career routes in order to minimize uncertainties about future performance. This testing strategy is particularly important when a suboptimal individual performance can jeopardize the performance of the total collectivity (fig. 1*c*). But when the constraints on positions are reversed and an exemplary individual performance has a close relationship with organizational success (fig. 1*a*), organizations will often create holding points for older personnel who no longer wish to be under the extreme pressure of enforced productivity. Thus older faculty and sales personnel may drift into administrative positions near the end of their careers.

The consequences of another relationship can be delineated by noting that when individual achievements have little effect on collective performance (fig. 1*b*), ability will matter less. It is in these positions that nonmeritocratic outcomes will be most probable. Here the inept can receive the most protection (Goode 1967) and discrimination will be virtually costless (Blalock 1961). It also follows that political considerations can receive more emphasis under this condition. For example, the practice of ensuring total loyalty on the part of one's subordinates by deliberately promoting incompetents to positions where allegiance is useful will be less costly when there is a negligible relationship between individual performance and the performance of the collectivity. Therefore, if other factors are held constant, it can be said that *to the extent that collective performances are unresponsive to the success or failure of individuals* (fig. 1*b*), *decisions about personnel will be governed by considerations other than merit.*

But there is another situation in which purely political considerations may receive the greatest emphasis. Recall that the relationship between individual and organizational performance will be attenuated when positions are located within subunits that have little effect on the performance of the total organization. It also follows that if the connection between subunit and organizational performance is limited or immeasurable, the allocation of resources between units will often depend on the political resources of the contestants. This, in turn, makes allegiance more valuable than ability. Moreover, the elevation of inept supporters to build alliances will be relatively costless because subunit administrators will be under less pressure to maximize productivity when this condition holds. Therefore, if other factors are held constant, *to the extent that there is a negli-*

gible relationship between subunit and organizational performance so that organizational performance will be relatively unresponsive to the achievements of individuals (a special instance of fig. 1*b*), *political considerations will be weighed more heavily than ability when decisions about personnel must be made.*

The relationships between individual and organizational performance have additional implications for stratification systems within organizations. They help to explain how rewards will be distributed and when organizations must negotiate with groups rather than with individual employees. We can begin with a brief discussion of the distribution of rewards and then go on to discuss collective bargaining in more detail.

Rewards and Collective Bargaining

Stinchcombe (1963*b*) has already dealt with the distribution of rewards within positions. His examples suggest that in positions where an exemplary individual accomplishment makes a significant addition to organizational performance (fig. 1*a*) and where individual accomplishments are measurable (Blalock 1961), organizations will distribute rewards unequally according to differences in productivity. But in positions where an outstanding individual attainment has little effect on collective performance (fig. 1*b*), differences in individual productivity will be of less consequence and the distribution of rewards should be compressed.[11]

We can begin to build on these insights by noting that in positions where an outstanding individual performance cannot add much to an organization's output, an adequate performance will be sufficient. Individual accomplishments that go well beyond this level will be almost superfluous because they will add little to organizational performance. It follows that under these conditions, the worker who gives an exemplary performance will receive rewards which are not much greater than those received by peers whose work is only satisfactory. Thus, the superb school teacher or bank teller receives less pay than fellow workers who have more seniority.

Because an adequate performance will be sufficient in these positions, organizations also need not offer workers the promise of future promotions to induce exceptional diligence.[12] From the worker's point of view, large

[11] It should be noted that, although there is a close relationship between individual and collective success in academic positions in research universities, the distribution of monetary rewards is not very unequal. (According to the *Chicago Tribune* a double Nobel laureate in physics received $50,000 from his university in 1974. Many full professors who published infrequently received salaries more than half as large in that year.) But since the primary goal of academic researchers appears to be prestige and since this resource is distributed very unequally (see Zuckerman [1970] for a statement of both points), this example does not challenge Stinchcombe's hypothesis.

[12] Remember that in positions where individual performances can only hurt organizational endeavors (fig. 1*c*), promotions happen only after an extended period of time

changes in effort will correspond to small changes in reward, and this means that individual paths to increased utility will be largely blocked for those in positions where an exemplary performance does not matter.[13] Under such conditions if levels of utility are to be increased the only available strategy will be a collective one. As one early student of organizations put it: "When employers herd their men together in classes, pay members of each class nearly the same wages and offer none of them inducements to work harder or do better than the average, the only possible remedy for the men comes in combination; and frequently the only possible answer to encroachments on the part of their employers is a strike" (Taylor 1911, p. 186). Therefore, as long as other factors are held constant *it is likely that preferences for collective bargaining will be strongest in positions where an exemplary individual accomplishment can add little to the performance of the collectivity* (see fig. 1*b* and *c*).

This assertion is supported by survey evidence which shows that workers were most likely to favor collective bargaining when they felt that opportunities for pay increases were blocked (Goldthorpe et al. 1968).[14] Note, however, that the hypothesis speaks only to the issue of when workers will be most sympathetic to unions. Because there are other important factors that facilitate or handicap organizational efforts, the previous hypothesis will be a better predictor of collective bargaining when the unionization process is regulated closely so that workers need only vote to bring in a union. But when this is not the case, the additional factors that help or handicap organizational efforts will become more important. It follows that this hypothesis must be augmented if we are to explain the early organization of skilled labor and other groups which represents a partial exception to the rule.

By modifying an insight of Marshall's (1920), a general exchange the-

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to a few careful individuals. They do not result from the necessity to reward exceptional effort. Instead, promotions in organizations dominated by these positions generally occur because replacements must be made from within.

[13] In the rare instance where piece rates are still in force (Doeringer and Piore 1971), management generally controls the actual rate. This means that the rates are often adjusted so that over the long run increased effort by individuals will still not result in an equivalent increase in compensation.

[14] For additional evidence consider faculty unionization in higher education. In positions in institutions where the primary emphasis is on teaching, an exemplary individual achievement cannot add much to organizational performance, but when institutions stress research, a heroic individual achievement will often add a significant increment to organizational resources and prestige. It follows that faculty unionization should be common in teaching institutions, but it should be unlikely where research is emphasized. In fact, the list of colleges and universities where the faculty is unionized shows that this generality is correct. Very few research universities have a collective bargaining agreement with their faculties, but many teaching organizations do (Semas 1973).

ory can be advanced that explains this anomaly. First, to the extent that it is costly to substitute around or do without an occupational group, it will be necessary to give ground in disputes about representation and wages. Second, to the extent that the number of covered employees is small, it will be less costly to allow collective bargaining and salary increments. Third, to the extent that it is costly for consumers to substitute for an organization's product and alternative supplies are scarce, organizations can allow unions and the wage increases that result because a greater percentage of these costs can be passed on to consumers or to third parties which fund operations. Thus, because it is difficult to substitute for skilled labor or their products and since the number of skilled workers generally is not large, it was much easier for craftsmen to be the first to organize at a time when this process was not facilitated by government regulation.[15] It follows that even though there may be some relationship between individual performance and organizational accomplishments in skilled positions, these additional factors helped craftsmen to organize before other employees who had stronger reasons to prefer unionization.

Thus, it can be seen that the three relationships between individual accomplishments and organizational performance in figure 1 have important implications for mobility and the ways that behavior is rewarded in organizations. These same relationships can also be used to explain the methods that organizations use to control their employees and the individual adaptations that result. We can begin a discussion of these matters by talking about control.

SOCIAL CONTROL AND INDIVIDUAL ADAPTATIONS WITHIN ORGANIZATIONS

Control Systems

The literature is quite explicit about the technological conditions which lead to bureaucratic organizations. To the extent that there is one best way to accomplish tasks which do not vary (Perrow 1967; Stinchcombe 1959) or to the extent that there is agreement about what should be done and how to go about it (Thompson and Tuden 1959), a bureaucratic structure will be most appropriate. But there are exceptions. Why, for

[15] Of course, other factors make it easier for a comparatively small group to unionize when conditions are hostile. Olson (1968) argues that large latent groups which seek an indivisible benefit will find it more difficult to organize than will smaller ones. By law, benefits to unionization are indivisible since a worker who does not participate in organizational efforts will still receive all benefits if the union wins. Thus, because large groups are more likely to be hampered by apathetic "free riders," it follows that the more numerous unskilled workers would find that it was more difficult to unionize even though they had stronger reasons to prefer collective bargaining agreements (for empirical support for Olson's argument see Gamson [1975]).

example, do police organizations tend to follow a military-bureaucratic model? There is no uniquely successful way to accomplish law enforcement tasks and the work is quite variable (Wilson 1971; Bittner 1975) so the internal organization of police departments is difficult to explain with technological theories.

Bureaucracies, however, are also useful because they put constraints on behavior. Rules, standard operating procedures, and hierarchical relationships are generally imposed because these devices are the best available ways to ensure that behavior will not deviate from acceptable standards. It follows that when protection from the inept or immoral is most vital, a bureaucratic format will be common even though core technologies may not be consistent with this adaptation. Thus, if other factors are held constant, *in positions where there are limits on the relationship between an exemplary individual performance and organizational success but mistakes can jeopardize important collective goals* (fig. 1*c*), *organizations will attempt to impose a bureaucratic format even if this adaptation is not consistent with technological conditions*. In such organizations inappropriate rules, inflexible standard operating procedures, and an overcentralized chain of command will all be likely because the superordinate goal of top administrators is control.

On the other hand, in positions where there is a close relationship between exemplary individual attainments and organizational performance but a suboptimal performance will not be costly (fig. 1*a*), then the interests of organizations and their employees will tend to overlap. Under these conditions social control need not be explicitly incorporated into structural arrangements. Instead, Adam Smith's metaphor of the invisible hand will be a better representation of organizational life. Thus, academic entrepreneurs labor for their own status needs but universities also benefit from these endeavors. This congruence provides the basis for mutually accommodative relationships between the successful researcher and his institution. Here lateral interactions will be common while hierarchical relationships will be infrequent. It also follows that images of a complex organization as a negotiated order (Strauss et al. 1963; Bucher 1977) will be most tenable where many positions fit these constraints. Therefore, if other factors are held constant, *in positions where an individual can only help his organization* (fig. 1*a*), *incumbents will face less stringent controls and more egalitarian relationships*.

In a paper on sanctions, Baldwin (1971) provides an additional set of reasons of these outcomes. He argues that the use of either rewards or punishments is costly to control agents, but these costs vary according to the probability that either inducement will succeed. Coercion, for example, will be most costly to control agents when it fails because expensive steps must be taken to punish the disobedient. Rewards, on the other

hand, will be most costly to control agents when they succeed because expensive steps must be taken to reward those who comply. It follows that when the probability of success is high, punishments will be the least costly way to control behavior because they need not be invoked frequently. But if the probability of success is low, rewards will become less costly to control agents because they need be given only infrequently.[16] Because of the lengthy adjudicative proceedings that are necessary to ensure due process (Selznick, Nonet, and Volmer 1969), even the use of such punishments as fines will be costly to modern organizations. Therefore Baldwin's distinctions can be applied to contemporary organizations if they are combined with the different relationships between individual attainments and organizational performance.[17]

We can begin this process by remembering that as long as an outstanding individual accomplishment cannot add much to organizational performance (fig. 1*b* and *c*), an average performance will be sufficient while individual performances that go beyond this level will be superfluous for the organization. Thus, because most men can perform at this average level, the great majority of employees will conform to acceptable standards, so the probability of success for controllers will be high. Therefore, it would not be cost effective to use rewards in these positions. Instead, since the probability of success is high, punishments will be the least costly inducement because they need not be frequently invoked. Therefore, if other factors are held constant, *in positions where an exemplary individual accomplishment does not add much to organizational performance* (fig. 1*b* and *c*), *organizations will have relatively flat pay scales and emphasize punishments as the predominant way to control their employees.*

Of course, because it will be more important to eliminate mistakes, the severity of punishments will be heightened in positions where an exemplary performance does not matter but a poor performance can hurt organizational endeavors (see fig. 1*c*). This increase in sanctions will be particularly evident when behavior that is costly to the organization benefits those who hurt collective efforts. Thus, police departments emphasize

[16] For a concrete example consider law enforcement. Because most people obey the laws most of the time, it would not be cost effective to give rewards for lawful behavior. On the other hand, it would be just as expensive to punish large numbers of citizens who are determined to disobey an unpopular law. In both instances authorities wish to put an end to behavior, but rewards are clearly inappropriate when success is likely while punishment will be expensive in the second case, where success is not likely.

[17] It is worth noting that Baldwin's argument becomes less effective as the costs of rewards and punishments diminish. Moreover, it is likely that the benefits of compliance for organizations will often outweigh the costs of incentives, but as long as organizations seek to minimize their costs Baldwin's argument should still apply.

sanctions more than airlines because the police officer gains from corruption while the airline pilot who jeopardizes organizational goals also puts his own life at risk. Bittner (1975) provides evidence for this assertion when he points out that police supervisors can only punish without being able to help their subordinates. Furthermore, because lateral movements between departments are improbable and pension eligibility occurs only after a lengthy period of service, the policeman who loses his job loses more than most workers who have been fired (Reiss 1971). Thus, if other factors are held constant, *in positions where individuals can only hurt organizations* (fig. 1*c*), *punishments will be more severe than those used in positions where there is little relationship between individual accomplishments and organizational success but a poor individual performance will not be expensive* (fig. 1*b*). But in either case controls will be punitive.

The last implication of Baldwin's argument is that *when these constraints are reversed and an exemplary individual accomplishment adds a significant increment to collective performance but a poor individual performance will not be costly* (fig. 1*a*), *primary reliance will be placed on positive inducements*. Here, as long as superlative performances are comparatively infrequent, cost effectiveness can be maximized by using rewards that are proportional to individual accomplishments. Thus, the possibility of winning large payoffs will always be present in these positions, but since few will be able to reach the highest levels of performance such costly inducements need not be provided often. It also follows that life in an organization dominated by such positions will take on the character of an unfettered market where there are few explicit controls, but some do not survive while others go on to receive considerable rewards.

As one would expect, these two ways to control behavior also lead to different adaptive strategies. The next section provides a brief treatment of the methods that individual employees will use when they must deal with these opposite constraints and the organizational behavior that results.

Individual Adaptations: Secrecy, Communications, and Innovative Behavior

Among other things, the previous discussion leads to an expectation that secrecy will be a virtual necessity in positions where an exemplary individual performance does not matter but a below-average performance can jeopardize collective attainments (fig. 1*c*). Where these circumstances are in force, employees will find that they are evaluated on the basis of mistakes and rule violations. Hence, silence about potential violations will be a vital survival technique. In this regard Bittner (1975) and others

have been unable to explain why police officers are so secretive, but when the constraints on this position are made explicit this adaptation is not surprising. As one would expect, people in similar positions, such as airline pilots (Roth 1963) and prison guards (Sykes 1958), use the same protective strategy.

Because these circumstances lead supervisors to control with the threat of sanctions it will also be irrational for subordinates to provide information about matters that are not proceeding according to plan. Since there is little to gain and much to lose in these positions, prudence requires that superiors be told only what they wish to hear. Thus, as long as other factors are held constant, we can expect that *when organizations have many positions where individuals can only subtract from collective efforts* (fig. 1*c*), *incumbents will be more secretive and this secrecy will interfere with the upward flow of negative information.* Under these conditions useful communications from the lower ranks will not be likely.

On the other hand, in positions where mistakes will not be very costly but an exemplary accomplishment has a significant impact on organizational performance, there is little reason to withhold information. Since much can be gained from individual success, those who labor under these conditions will face strong incentives to share information about difficulties. Thus, salesmen, academics, and record producers are in constant communcation with the organizational elements which support their efforts. Therefore, if other factors are held constant, we can expect that *when organizations are dominated by positions where individuals can only add to collective performance* (fig. 1*a*), *administrators should hear much useful corrective information about organizational endeavors.*

Finally, it should be remembered that, even in positions where mistakes are not very costly, as long as superlative individual accomplishments have limited effects on collective attainments (fig. 1*b* and *c*), organizations will tend to have flat pay scales and to control with sanctions. In such positions there will be little reward for heroic achievements. This means that it will be foolish to take risks in order to be innovative. Instead, because employees are evaluated on the basis of infractions and below-average performances, it will be quite reasonable for the incumbents of these positions to be preoccupied with organizational rules. Thus when sociologists use phrases like bureaucratic personality or ritualistic behavior (Merton 1968) to describe these adaptations, an important element of reality is concealed. In such positions risk-averse behavior is quite rational. It seems irrational only to those who see the world from the unique perspective of the university.[18]

[18] The hypotheses that have been advanced in this paper are subject to empirical test. This operationalization is possible because the relationships in fig. 1 can be repre-

IMPLICATIONS AND SUMMARY

Before I provide a summary of these conclusions, a disclaimer is necessary. In both articles on the subject, Stinchcombe (1963*b*; Stinchcombe and Harris 1969) argued that the relationships depicted in figure 1*a* and *b* could be used as an example of the functional theory of stratification which would be subject to empirical verification. But it seems that the relationships in figure 1*b* and *c* reveal an important weakness in the functional model if it is applied to individual rewards within positions as Stinchcombe suggests. One logical implication of the previous discussion is that an exemplary individual performance in some organizational positions may be very useful to society but not as useful to organizations. For example, neither the superlative high school teacher who consistently develops great talents nor the diplomatic policeman who forestalls several riots is likely to receive tangible rewards in proportion to his social value. This injustice occurs because in both positions an exemplary performance has little relationship to organizational success even though these achievements are quite beneficial to the larger society.

At this point it should be noted that, in contrast to Stinchcombe, Davis and Moore (1945) considered only differences between positions without trying to apply their theory to rewards for individuals in the same position. But the unavoidable fact that many tasks are performed within organizations also points to a weakness in their version of functionalism. Even if important positions are filled by "scarce, diligent and competent individuals" who receive higher rewards for their contributions to organizational success, that need not mean that this relationship is "functional" for the larger society.[19] As Bronfenbrenner (1971) has pointed out, many

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sented statistically by regression coefficients. After computing slopes to gauge how much an organization's performance will respond to individual performances in various positions, these coefficients can be used to see if they predict the dependent variables in the previous hypotheses. This procedure would not be tautological because the question is whether the different relationships between individual and organizational performance predict the organizational outcomes I have stipulated. Of course, these tests cannot be performed using any set of organizations because performance at both the positional and organizational levels must be measurable. It will also be necessary to ensure that extreme individual performances appear in the sample, but this is unproblematic since it is legitimate to oversample for an independent variable. Therefore, as long as other important causes are controlled, the results of these properly specified equations should provide a convincing test of my arguments.

[19] It should be emphasized that individual rewards are not always in direct proportion with unique contributions to organizational success. Particularly if there is organizational slack, occasional significant deviations from this norm can be tolerated. For example, because large corporations are generally subject to the weak discipline of oligopolistic markets, they have much slack. In such firms ownership is almost invariably separated from control so that in the absence of countervailing power, top executives have much to say about their own rewards. Because it is difficult to detect any differences in corporate success when top administrators are changed (Lieberson

organizations seek goals that are either irrelevant or harmful to the collective interest. Thus, it is quite possible that all those in a particular organizational position may be paid much more than their "functional" contribution to society warrants because their efforts help their organization much more than they help society. As a result, it can be said that behavior that is very useful to organizations need not be very "functional" to society while behavior that is very "functional" to society need not be very useful to organizations. Of course, since rewards will not be equal to social contributions in either case, it follows that functional theories often lead to incorrect predictions.

Summary

It can be seen that a number of important consequences follow from the three relationships between individual and organizational performance. First, to the extent that an organization has positions where inept individual performances hurt collective efforts more than superlative performances help, promotions can be expected to occur from within after extended periods of time. Under these conditions lateral movements between organizations will be unlikely. But in positions where an exemplary individual achievement adds an important increment to organizational performance but mistakes will not be costly, organizations will create a system whereby many impermanent employees try out for a limited number of starring roles. Here lateral movements between different organizations should be common. Second, in positions where there is a strong relationship between a superlative individual performance and organizational success, such performances will be highly rewarded. Where this is not the case, increased effort will be less productive for both individuals and organizations. Under these conditions, individual paths to increased rewards will be blocked and this increases the probability that employees will prefer collective bargaining.

Third, to the extent that an organization has positions where inept individual performances matter more than exemplary ones, the necessity for control will lead to a bureaucratic structure even if this adaptation does not fit technological conditions. A reversal of these constraints, however, will result in the absence of internal controls and more internal freedom. Fourth, controls based on rewards will be most efficient when superlative individual performances matter more than those which are inept. But in

and O'Connor 1972) and because executive compensation is related more clearly to size than to profits (Scherer 1980; Roberts 1959), a good part of this group's very high income can probably be attributed to power rather than to their functional importance to the organization (for an equivalent analysis see Broom and Cushing [1977]).

positions where inept performances are more important or even where the relationship between exemplary individual accomplishments and organizational performance is limited, controls based on sanctions will be least costly to organizations. Fifth, to the extent that inept individual performances matter more than exemplary ones, position incumbents will be secretive about job-related matters and this secrecy will interfere with the upward flow of communications. A reversal of these constraints, however, will lead to voluminous communications and useful corrective information about organizational endeavors. Finally, the argument can also be used to illuminate some important difficulties with the functional theory of stratification.

On a more general note, the theory that has been partially developed in this paper has at least three uses. First, it provides an explanation for many outcomes within organizations. Second, it provides the beginnings of a contingency approach to occupational careers in an organizational society. Third, this concentration on the ways that positions limit or accentuate the effects of individual performances can be used to provide a bridge between the literature on occupations and our theories about the behavior of organizations.

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Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

COMMENT ON SAMUEL AND LEWIN-EPSTEIN[1]

In "The Occupational Situs as a Predictor of Work Values" (*AJS* 85 [November 1979]: 625–39), Samuel and Lewin-Epstein hypothesized that persons employed in 10 situses (Morris and Murphy 1959) would hold particular configurations of work values. On the basis of a multiple classification analysis of survey data, they concluded "that occupations which share a common socioeconomic function are likely to form similar work values among their incumbents" (p. 637). Significant situs differences in work values were found in a representative sample of Israeli workers which were not attributable to socioeconomic or other social characteristics.

The investigators do not postulate a single causal mechanism explaining the value differences but, citing Kalleberg (1977, pp. 141–42), note three possible contributing factors: "experiences prior to the individual's entry to the labor force," "nonwork social roles," and "work experiences." The emphasis of their paper, however, is on adult work experiences associated with the situs of employment; they predict that workers will "adopt those valuations which are consistent with the particular social functions

[1] The analyses described in this paper were supported by a grant from the Center for Studies of Metropolitan Problems of the National Institute of Mental Health (MH26421) and the Computer Center of the University of Minnesota.

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implied by their occupations" (p. 629). This assertion is consistent with our earlier findings regarding the effects of the employment sector on political liberalism (Lorence and Mortimer 1979).

In a previous issue of the *Journal* (see Mortimer and Lorence 1979), we had reported evidence that both selection and socialization processes account for differences in occupational reward values in a group of 512 men studied in their senior year and a decade after college graduation. Although the three value dimensions we analyzed (extrinsic, intrinsic, and people oriented) were significantly stable over the 10-year period, work autonomy was found to increase the salience of intrinsic and people-oriented rewards; income, in contrast, diminished the strength of these values and led to positive evaluations of extrinsic rewards over time. On the basis of this pattern, we concluded that "occupational reward values develop by means of simple processes of generalization or reinforcement. . . . The rewards of work . . . increase in salience and value over time" (p. 1378).

Juxtaposing the findings of these studies raises two questions: (1) Did we overlook situs differences in values because of our exclusive emphasis on work experiences as sources of value change? (2) To what extent can significant situs-related value differences, if they are found in our data, be attributed to selection processes (e.g., persons with certain work values choose employment in particular situses) or to the distinctive work experiences and rewards in the situses?

To answer these questions, we have reanalyzed our data, using methods as similar as possible to those employed by Samuel and Lewin-Epstein. Occupational value scales were constructed by summing items with high loadings on the three dimensions in a previous exploratory factor analysis. These values appear to correspond well to Samuel and Lewin-Epstein's materialism, individualism, and collectivism (see table 1). We use a single item concerning freedom from supervision which is quite similar to their humanistic dimension. Note that in their operationalization the choice of one option in each pair necessitates the repudiation of the other, but our measures permit the four values to vary independently.

Because of the highly educated character of our sample, the men were concentrated in relatively few situses. Therefore, the respondents' occupations were coded in the following five situses: legal, economic (including finance and records, manufacturing, transportation, extraction, building and maintenance, and commerce), education and research, health and welfare, and aesthetics and entertainment. Although this classification does not distinguish all 10 situses, those that are collapsed in the "economic" category were expected by Samuel and Lewin-Epstein to be associated with the same pattern of values (p. 630).

TABLE 1

MEASURES OF OCCUPATIONAL VALUE DIMENSIONS

Samuel and Lewin-Epstein	Mortimer and Lorence (1979)
Lead question: With which of the two following opinions do you agree most?	Lead questions: Ten years following college graduation: If you were offered another occupational position, how important would you consider each of the following work characteristics in deciding whether to accept it? (Responses: not too important, fairly important, very important, crucially important; range 1–4) Senior year of college: How important would you say the following things are in your decision about whether to go into this kind of work?*
First pair: Collectivism: (*a*) it is very important that work would give one the opportunity to do something useful for others Individualism: (*b*) it is very important that work would enable one to learn new things and develop his own talents	People-oriented values: the chance to be helpful to others or useful to the society; a chance to work with people rather than things Intrinsic values: opportunities to exercise my abilities and skills; opportunities to express my interests; opportunity to be creative and original.
Second pair: Humanism: (*a*) it is very important to be employed in a kind of job which allows considerable discretion Materialism: (*b*) it is very important to be employed in a job which provides considerable income	Freedom from supervision: freedom from supervision—the chance to make my own decisions Extrinsic values: how highly people regard the job; opportunities for advancement; high income

* There were minor differences in the wording of the value items in the senior year of college.

The first step in the reanalysis was to ascertain, by a one-way analysis of variance, whether there are significant differences in values among the occupants of the five situs categories which conform to these investigators' predictions. The *F*-ratios provided only partial support for their thesis: there were significant situs differences in extrinsic (materialism) and people-oriented (collectivism) values, but no differences in intrinsic (individualism) values ($P > .35$) or in concern with freedom from supervision (humanism, $P > .12$). And even in the two instances where significant situs effects were found (see table 2), the value patterns did not fully conform to the investigators' predictions (p. 630). For example, while they predicted that persons employed in the legal, health and welfare, and education and research situses would be high in collectivism, we find that persons employed in the latter situs fall below the total sample mean on the people-oriented value dimension. Moreover, while Samuel and Lewin-Epstein expected that persons in the economic situses would be outstanding in materialism, those in the legal sector have an even higher mean extrinsic value score.

In table 3, we present the results of a multiple classification analysis of the effects of situs, controlling for senior work values and four work dimensions: occupational prestige (Hauser and Featherman 1977, appendix B), income, and scales representing work autonomy and the social content of work (see Mortimer and Lorence [1979] for the constituent work experience items). This analysis was undertaken to discover whether the two significant sector effects could be attributed to selection, that is, to early work values which influence situs choice and persist over time, or to the distinctive work experiences and rewards that may be associated with situs placement. However, we present the same analyses for the other values to detect whether a common causal pattern seems to underlie the development of all four value dimensions.

First, it should be noted that the two significant situs effects, shown in table 2, disappear when the other variables are controlled. Additional analyses (not shown) revealed that the effect of situs on people-oriented values is due to selection processes: situs was no longer significant ($P > .129$) when senior-year values were included in the equation. Situs continues to have a significant influence on the evaluation of extrinsic rewards when this earlier value dimension is controlled, but the effect becomes insignificant when the occupational variables are entered in the analysis.

Second, it is important to observe that of all the variables incorporated in the analysis, earlier values have the most consistent effects on values a decade later. Furthermore, in agreement with our earlier analysis using a more powerful statistical method, work autonomy is found to reinforce intrinsic and people-oriented values. It also increases the salience of free-

TABLE 2

ONE-WAY ANALYSIS OF VARIANCE OF OCCUPATIONAL VALUES BY SITUS OF EMPLOYMENT

Situs	Work Values								
	Extrinsic		Intrinsic		People Oriented		Freedom from Supervision		
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	N^a
Legal	7.86	1.85	9.39	1.94	5.47	1.67	2.97	.84	(95)
Economic	7.76	1.91	9.48	1.88	4.62	1.41	2.91	.85	(138–140)
Aesthetics and entertainment	7.54	1.90	9.91	1.57	5.16	1.12	2.77	.53	(19–22)
Health and welfare	7.29	2.35	9.65	1.81	5.83	1.57	3.12	.83	(112–113)
Education and research	6.72	1.98	9.83	1.84	4.99	1.78	2.88	.85	(127–129)
Total	7.40	2.07	9.61	1.85	5.18	1.65	2.96	.83	(492–498)
F-value	6.030*		1.101		10.341*		1.846		...

[a] Because N's vary somewhat owing to missing data, the range is given.

* $P < .001$.

TABLE 3

MULTIPLE CLASSIFICATION ANALYSIS OF WORK VALUES

	Work Values											
	Extrinsic			Intrinsic			People Oriented			Freedom from Supervision		
	η	Beta	P	η	Beta	P	η	Beta	P	η	Beta	P
Situs	.190	...	...	.090	...	...	.270	...	...	.120	...	...
	...	.120	.261	...	.090	.441	...	.180	.084	...	.150	.130
Occupational value in senior year	...	.337	.001	...	.206	.001	...	.368	.001	...	.197	.001
Prestige	...	.006	.895	...	−.095	.169	...	−.125	.062	...	−.199	.005
Income	...	.210	.001	...	−.087	.085	...	−.044	.354	...	.056	.261
Work autonomy	...	.068	.137	...	.257	.001	...	.180	.001	...	.187	.001
Social content	...	.002	.980	...	.030	.642	...	.060	.338	...	.095	.145
R^2	.235		...	.128		...	.239		...	.111		...

dom from supervision. Income, a major extrinsic reward and indicator of socioeconomic achievement, reinforces the importance of extrinsic rewards.

Taken together, these findings appear to contradict the hypothesis that occupational situs makes an independent contribution to work values. Instead, investigators using cross-sectional data should be sensitive to the probability that work-related differences in values (e.g., those associated with socioeconomic status, situs, and occupational experiences) are largely due to selection processes. In our data, three of four value dimensions (all except the intrinsic one) measured in the senior year of college were significantly related to situs placement 10 years later. Finally, the analysis underscores the significance of work experiences, particularly work autonomy and income, for value development.

But given the special character of our sample (i.e., its restriction to male college graduates), the two questions that prompted this analysis cannot be considered fully resolved. To assess definitively the contributions of hierarchical (socioeconomic) and nonvertical (including situs and occupational experiences) work dimensions to the socialization of adult work values, longitudinal study of a representative sample of the labor force is necessary.[2]

JEYLAN T. MORTIMER AND JON LORENCE

University of Minnesota

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[2] Samuel and Lewin-Epstein were invited to reply to this comment.—Ed.

Review Essay: Parsons's Last Testament[1]

Action Theory and the Human Condition. By Talcott Parsons. New York: Free Press, 1978. Pp. xii+464. $15.95.

R. Stephen Warner
University of Illinois at Chicago Circle

This is the last book published by Parsons during his lifetime. Along with *Social Systems and the Evolution of Action Theory* (1977), it contains papers from Parsons's last decade. I am told that there will be published posthumously a collection of essays on the sociology of knowledge and a monograph on the "American societal community," and it is to be hoped that other selections from his writings will be published in new formats in the years to come. But *Action Theory and the Human Condition* stands as the conclusion to, although not a summary of, a long and influential career. In it, Parsons returned to some of the most pressing concerns of his professional youth, including epistemology and the intersection of religion and rationality. It is not a polished book, and it contains evidence of having been rushed into print. Nonetheless, it will be of interest to sociological specialists in the study of medicine, higher education, the professions generally, the family, and especially religion. Some of its essays would enlighten the general reader, and I would like to interest our political pundits in Parsons's work. The book is essential, however, for the student of Parsons himself, and one supposes that the old master here decided to reveal himself at the deepest intellectual level.

The book takes its title and its internal outline from the last and longest of its 15 chapters and the only previously unpublished essay, "A Paradigm of the Human Condition." The chapters are arranged in four parts—on health and illness, higher education, religion, and "the human condition"—and they are thoughtfully integrated by a series of introductions. Part 1 contains some revisits to Parsons's seminal work on the doctor-patient relationship and the "sick role," but its highlight is chapter 2, "Research with Human Subjects and the 'Professional Complex,' " originally published in *Daedalus* in 1969. Here Parsons outlines the rights and responsibilities of medical and other professionals vis-à-vis their clients, students, and research subjects, and he argues, as elsewhere in his recent work, that generalized trust is an essential ingredient in the functioning of an ad-

[1] In preparing this essay, I have had the help, in various forms, of Jeffrey Alexander, Dean Gerstein, Barclay Johnson, Bennetta Jules-Rosette, Sally Kilgore, Victor Lidz, Neil Smelser, and Norbert Wiley.

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vanced society. This essay should be required reading for those concerned with professional ethics.

The papers in part 2 are all postscripts to Parsons's and Platt's *The American University* (1973), and they can be read with profit as introductions to that important and difficult book. Most notable to me was chapter 7, in which Parsons responds to Neil Smelser's critique of *The American University* (which appeared as an "epilogue" in the book itself) and defends his idealization of the structurally autonomous but functionally diffuse typical American comprehensive university. As a sociologist of religion, I found part 3 most rewarding for my substantive interests, including as it does the magisterial encyclopedia article "Christianity" (chap. 9); an eloquent conference paper, "Belief, Unbelief, and Disbelief" (chap. 11); a poignant article on death (written with Renée Fox and Victor Lidz), "The 'Gift of Life' and Its Reciprocation" (chap. 12); and some sage advice offered to proponents of new religions, "Religion in Postindustrial America: The Problem of Secularization" (chap. 13). The consistent theme of these essays on religion is that Christianity, far from being irrelevant to today's scientific world, has been largely institutionalized in the normative system of our society.

It is to part 4, on the human condition, that one's curiosity is drawn. "A Paradigm of the Human Condition," written in the context of an extended faculty seminar at the University of Pennsylvania during Parsons's years of active retirement in the mid-1970s, is an attempt to conceptualize the most comprehensive aspects of our existence. Using his familiar four-function scheme, Parsons subsumes his earlier "general system of action" as one subsystem of the "human condition," just as he had before made the "social system" a subsystem of the general system of action. At the social system level, *A*, *G*, *I*, and *L* were, respectively, the economy, the polity, the societal community, and the fiduciary. On the level of the general system of action, *A* became the "behavioral organism" (in the present book, Parsons follows a suggestion of Charles Lidz and Victor Lidz to call this now the "behavioral system"); *G* the "personality system"; *I* the "social system"; and *L* the "cultural system." In an effort finally to incorporate in his theory the ultimate environments of the action system (first appearing only as "ultimate reality" and the "physical-organic environment" in his book on evolutionary theory [1966]), Parsons here takes the step of putting the action system into the *I* cell of the human condition, where *A* is the "physio-chemical system"; *G* is the "human organic system"; and, ultimately, *L* is the "telic system."

Those familiar with Parsons's efforts of the past 20 years will find these new designations broadly familiar and appropriate to his thinking, and they will be unsurprised to find also that four media of interchange and six intersystem exchange pairs are postulated and outlined (see Turner [1980] for more details). Since the new paradigm moves beyond the realm of "action," which is the realm of meaning, the newly designated media and interchanges are not, as are the action-level media, symbolic. It might

seem, though the surmise turns out to be misleading, that Parsons here incorporated "real" reality and "ultimate" being. Since, however, the human condition is a deliberately and necessarily anthropocentric concept, his final step was not to develop a cosmology in which the human drama would occupy, as it were, but a few moments of cosmic time, but an ultimate anthropology in which the empirical and nonempirical limits of our experience are brought into the conceptual arena.

To those not familiar with Parsons's theories (and to those familiar and wary), the whole effort may well seem ludicrous. Granted the heuristic value of having four reference points for the analysis of social phenomena, granted also the importance of distinguishing among personality, social, and cultural levels of analysis, does one really need yet another level of analysis, yet more categories, yet more boxes, the naming and filling of which were probably more enjoyable in the doing than in the reading?

Surely Parsons here reached his most outlandish extreme. Categories multiply geometrically. Arguments by elimination and by analogy masquerade as deductive logic. The works of earlier theorists—especially Freud's *Interpretation of Dreams,* Durkheim's *Elementary Forms,* and Kant's *Critiques*—are unabashedly marshalled as contributions to the evolving scheme. Farfetched analogies abound, and intellectual correspondences, between Parsons and his mentors, between him and various distinguished contributors to *Daedalus,* and even between products of two stages of his own thinking, are triumphantly presented as confirmations. In a genuinely blind refereeing process, much of this book, and especially its centerpiece, would probably be dismissed as the work of a charming and brilliant crank.

From many statements in this book and elsewhere in his last years, it is clear that Parsons knew he faced ridicule and incredulity, and in a defensive mood he lets the reader know more than once that, suspicions of wild speculativeness notwithstanding, he has done his homework and is deserving of our attention. He goes far to disarm the skeptic with his playfulness about his own ideas, his generosity to collaborators and critics both, and his sophisticated optimism (even, it appears in several of these essays, in the face of his own mortality). He was indeed tenacious and dogmatic, but he was also astute and knowledgeable. In the end, his astounding intellectual egocentricity was tempered and his system enriched by his openness to a cosmopolitan intellectual environment peopled by his earliest teachers and his later *Daedalus* collaborators but most especially by his several generations of students. Over 30 years ago, Robert Merton praised Parsons as a teacher who generated intellectual enthusiasm rather than creating slavish disciples, and this last book makes clear that he not only kept teaching that way but also reaped huge rewards from doing so. Just as his earlier work gave evidence of the influence of such students as Merton himself, Kingsley Davis, Wilbert Moore, and Robin Williams (Martel 1979), so *Action Theory and the Human Condition* shows how much Parsons owed to Neil Smelser on the analysis of differentiated social

structures, to Renée Fox on the consequences of uncertainty in medical practice, to Victor Lidz on the analysis of symbol systems, and to Robert Bellah on the institutionalization of religious ideals, among others. As represented in this book, he even had the great satisfaction of drawing on the work of such intellectual grandchildren as Jeffrey Alexander, Harold Bershady, and S. M. Lipset. It is true enough that Parsons's reading and intellectual permeability were highly selective (an attribute of systems that he stressed), but at the same time it is also true that the world he thereby made sense of was as comprehensive as that of any social thinker of our time.

It is remarkable that few of those who learned from him have remained within the technical confines of his four-function theory. They have used pieces and images from his world view—his developmental sequences, his cybernetic hierarchies, his interdependent wholes, and most especially his dichotomous pairs—in such a way that he could, in turn, use their results (see Alexander 1978). His world view was complex and embracing, but it was hardly formless or merely eclectic. To my mind, the most important contribution of *Action Theory and the Human Condition* is that it shows us not so much that his thinking was rich but why it was both enormously rich and finally limited.

Parsons's great intellectual power appears in this book to be a combination of analytic and synthetic capacities harnessed to a scientific conscience. His syntheses, as has often been remarked, were as allusive and metaphorical as they were systematic, and his analyses—it becomes obvious from reading his revisits to Freud (chap. 4), Durkheim (chap. 10), and Kant (chap. 15) and his final ruminations on death in the Western world (chap. 14)—were profoundly dualistic. The most striking intellectual feature of the human condition paradigm is the variety of transformations on the fundamental pairs of norms and conditions (ideal/material, integrative/adaptive); ends and means (consummatory/instrumental, telic/physical); universalism and particularism (categorical/sensory, genotype/phenotype); and, perhaps the source of them all, sacred and profane (spiritual/temporal, soul/body). Not since *The Structure of Social Action* have these concerns been so apparent in Parsons's work (see Münch 1981). "From the *human* point of view," Parsons writes, "the phenomena of cybernetic ordering and of evolution *must* be considered to be limited within a finite conception of a 'universe.' And, if the universe is to be considered 'two-ended,' not 'one-ended' and thus 'circular,' there must be two, not one, *kinds* of limiting conceptions of 'order' . . ." (p. 366). Variations on this fundamental dualism have long served as axes of differentiation in Parsons's discussions of structural patterns (church/state, office/incumbent, diffuse/specific), and he was adept at combining them into the fourfold cells of systems (Gerstein 1975). That the pairs empirically overlap and outline dimensions rather than fixed poles is another way of saying that they are analytical dualisms, and this, combined with their capacity

for interpenetration (most fully realized in the myth of the divine and human Christ), is what makes Parsons's use of them profound rather than simplistic. He insisted that there were always two sides, that they required each other, that each would claim its due, and that each had to wait its turn. His analyses of action, of systems, and of media, as well as his option for an ethic of responsibility, followed from these fundamental beliefs.

The most breathtaking moments in *Action Theory and the Human Condition* come when Parsons, following his logic to its utmost, makes religious affirmations in the role of theorist. As a founder of the contemporary sociology of religion, he long maintained that humans take religion seriously enough that it must somehow be incorporated within an action theory. From the point of view of phenomenological study, it is perhaps possible to "bracket" the truth claims of religion and simply to study the processes by which its plausibility is maintained and its commandments are accommodated to the world (Berger 1969; see also Johnson 1977). But as soon as one recognizes that religion is one of the ways that the human world comes to be constructed, while at the same time one aspires to a comprehensive account of human existence, one must decide what to do with religion. Parsons, insisting always on the two irreducible orders of the ideal and the material, refused to conclude that religion was merely illusory (1973), and he contented himself for a long while with the notion that religion was primarily a matter of normative commitments, the existence, if not the scientific validity or empirical strength, of which everybody recognized (Warner 1978). But his master, Max Weber, taught that the actual content of religious ideas had profoundly shaped world history, and, rather than suppose (as I think Weber did) that such shaping was a world-historical wild card, Parsons here finally accepts the reality of the "non-empirical" into his theory. "Clearly, we think of the telic system, standing as it does in our treatment in a relation of cybernetic super-ordination to the action system, as having to do especially with religion. It is primarily in the religious context that throughout much of cultural history belief in some kind of 'reality' of the non-empirical world has figured prominently. With full recognition of the philosophic difficulties of defining the nature of that reality we wish to affirm our sharing the age-old belief in its existence" (p. 356). In this way, he sought to place nonarbitrary limiting conditions on human action.

Lest this affirmation be thought of as an old man's deathbed confession, it must be said that the philosophic difficulties mentioned include Parsons's acceptance of Kant's claim that the really real world, the *Ding an sich,* is unknowable as such. Parsons adds that this claim holds for the telic as well as the physical system, and we are assured that our knowledge is universally valid by the ordering in the categories of our understanding and by the mysterious ultimate comprehensibility of the world (on which point he repeatedly cites a remark by Albert Einstein). The unknowability

of the outer boundary of the telic system, in turn, means that Parsons's religious affirmation is no more particular than the intriguing suggestion that, for example, the Judaeo-Christian God is to "ultimate ground" (L_L of the human condition) as a particular linguistic utterance is to deep structure (p. 372).

The result of this attempt to affirm both science and religion while avoiding the pitfalls of positivism and relativism is, in my opinion, an ultimately idiographic, interpretative account of Western, and particularly American, civilization (see also Bellah 1980). On the face of it, one is entitled to be doubtful about the universality of the concepts of one who so self-consciously stood at the center of the world he analyzed: a male, academic, liberal Protestant American of the 20th century. Like Hegel, Parsons occupied at least a privileged epistemological position, and he grandly told us what the world looked like from that stance. It is still possible that the world he inhabited will become the only social world in the 21st century. Moreover, the world he pictured was an appealing, if not utopian, one: "Human history is not a morality play in which the good are rewarded and the evil punished, but a struggle for salvation, enlightenment, progress, or community in which many, indeed most, of the participants have been and are caught up in tragic conflicts and dilemmas" (p. 262). Nonetheless, his world of universalism had very particular roots, as he well knew (p. 433); and his differentiated world was the product, in part, of centuries of theorizing about it (p. 340). Thus, Parsons's place in the development of thought will not be that of a Newton, or even of a Mendel (Parsons 1977, pp. 133–34), but of a Hegel (Bershady 1973, p. 12), or even of an Augustine. That is a dazzling achievement, but it is due to Parsons's great capacity to distill our culture.

This achievement cannot be enough for those who want a Newton, and its very foundations will continue to offend dogmatic idealists and materialists, as well as partisans of faiths more particular than Parsons's. In my own work, I am well aware that orthodox Christians have no interest in the kind of help that might be forthcoming from his grand historical view (Norman 1979, pp. 82–83). But those of us who insist on the inherent limits of his thinking do so primarily because he invited extraordinarily close scrutiny and himself insisted that his postulations were the foundation if not the full paradigm of a genuinely transcultural social science. To refuse to accept all of his claims is not to disregard his contributions or to nominate an alternative claimant. Those who have avoided Parsons as his hegemony waned ought to be aware that he continued to publish not only elaborate and challenging theories, but also original and stimulating essays in a great variety of fields in spite of all the disapproval he received. He was by no means as rigid about his theories as some of his more impassioned defenders, and just as Parsons ignored textual fundamentalism and made use for his own purposes of Weber, Durkheim, and Freud, so also he has now become part of our culture, free for appropriation by all.

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Book Reviews

Sociologists, Economists and Democracy. By Brian Barry. Chicago: University of Chicago Press, 1978. Pp. vi+202. $3.95 (paper).

Dennis J. Encarnation
Stanford University

Rationalist theories of political behavior have recently risen in status to that of a new—or, more accurately, rediscovered—paradigm in the systematic study of politics. Brian Barry's short, provocative book played no small part in the debate that precipitated this shift. However, shortly after its first printing in 1970 by Collier-Macmillan, the book became almost as famous for its scarcity as for its lucidity. It is with some relief, therefore, that oft-besieged owners of the out-of-print edition applaud the decision of the University of Chicago Press to reprint the book unchanged (and at a reasonable price).

Without reservation, Barry's treatise is the most lucid and most influential critique of two important, competing perspectives in political analysis: the "sociological" school of Talcott Parsons, Gabriel Almond, and other so-called functionalists; and the "economic" school of Anthony Downs and Mancur Olson, among others. Barry's analysis of the two traditions is highly selective by design; no general survey of studies or of earlier critiques is attempted. Instead, the works of Downs and Olson especially are singled out because they are deemed "exemplar[s] of the virtues of the 'economic' approach to political analysis" (p. 24). Likewise, the works of Parsons and Almond, Harry Eckstein, and S. M. Lipset are considered to be among the best in the "sociological" genre.

Comparing these two schools is no small task. Exponents of the "sociological" approach disagree among themselves on many aspects of nomenclature and analytical method. Moreover, "the 'sociological approach' cannot be set off in a neat point-by-point contrast with the 'economic approach' " (p. 6). Nevertheless, they are two very different perspectives. Their ideological origins, evaluative judgments, assumptions, definitions, analytical structures, methodologies, explanations—even the very questions they pose—are all different. Barry's elucidation of these differences, both within and between schools, is itself an important contribution to modern political analysis.

While comparisons of the two approaches along these lines are intertwined throughout the text, the general plan of *Sociologists, Economists and Democracy* is to treat sequentially specific arguments of specific authors on specific problems. For exponents of the "economic" approach, these prob-

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lems include political participation (chap. 2) and party competition (chaps. 5 and 6); for exponents of the "sociological" approach, the maintenance of democratic government (chaps. 3 and 4). In the first and last two chapters, explicit comparisons across schools are drawn. By basing his evaluation on "how successful our theorists are in coping with the problems they set themselves" (p. 12), Barry is in a good position to assess when a given approach is likely to work and when it is not. The result is a powerful and compelling critique of these authors and the schools they typify.

Differences in each school's assumptions concerning human nature and in their analytical methods illustrate vividly the contrasts Barry is able to explore and assess. Parsons, Almond, and others contend that norms and values are the most important determinants of individual and collective action, and that the sharing of these norms and values is vital to the maintenance of a stable social system. Moreover, Parsons and others adhere to the notion, introduced earlier by Durkheim, that the way to explain societal characteristics like political stability and democracy is to show how these features relate to other aspects of society—the sharing of democratic norms and values among the citizenry, for example—to form part of a functional whole. In his review, Barry rejects outright the notion that shared norms and values are a necessary condition for political stability or democracy; he demonstrates convincingly that studies supporting this linkage are fraught with almost insurmountable difficulties in defining concepts, formulating testable hypotheses, and inferring causation from empirical findings.

The recent movement away from the "sociological" to the "economic" approach can be characterized, according to Barry, as a movement away from the Parsonian view of a man as essentially nonrational, governed by encultured norms and values, and as an acceptance of the view that man is a rational maximizer of his own advantage. Moreover, exponents of the "economic" approach go on to adopt a different method of analysis from that of Barry's "sociologists": the postulation of a number of individual actors with certain ends and a deductive attempt to work out how the actors will behave in situations that present certain alternatives on the assumption that they relate means to ends as efficiently as possible. It is this deductive method and the rigor it imposes, though not always successfully, that Barry most admires in the "economic" approach. His faith in this school's promise is telling, though certainly not blind. He eventually rejects Downs's analysis of the decision to vote and of multiparty competition, and among other early theorists the work of William Riker is especially subject to disdain. Moreover, the text is laced throughout with a single warning: "The constant danger of 'economic' theories is that they can come to 'explain' everything merely by redescribing it. They then fail to be any use in predicting that one thing will happen rather than another" (p. 33). For those later theorists who seek to amend Olson and Downs, Barry's warning is ignored at some cost.

In his discussion of the "economic" approach, Barry offers more than criticism. He actually contributes to the future progress of this school

through his fine tuning of its analytical methodology and his mustering of additional empirical evidence (principally voting data) ignored by the theorists he reviews. For example, in his discussion of collective action Olson assumes that individuals are perfectly informed, and Olson and Downs both assume that individuals are independent decision makers. Barry demonstrates convincingly that a relaxation of these highly restrictive assumptions is consistent with the logic of rational choice. By so doing, he expands the explanatory power of the approach to encompass such politically important coordinating mechanisms as bargaining and entrepreneurial leadership. It is a testament to Barry's contribution that subsequent revisions of Olson and Downs have heeded these suggestions.

For all its lucidity, the book is not without its shortcomings. Although Barry cautions the reader "at the outset against reading too much into our labels" (p. 4), it is nevertheless misleading to identify the theory of rational choice with the "economic" approach. While nearly all economics is rationalist theory, the converse does not hold. Instead, rationalist theories of politics have shown significant achievement in several subject areas—including the two discussed by Barry, theories of political participation and of political institutions—each with its own distinct antecedents and line of development. Moreover, the rapid development of normative and positive theories of rational choice since the book's first printing makes it even more misleading to contend, as Barry does in his new preface, that "today . . . the same canon of 'economists' " (p. vi) applies. Such a claim only detracts from Barry's own contribution to this burgeoning literature.

An even more serious shortcoming of the book is Barry's failure to give any useful indication of how to treat certain issues that fall within the general purview of his "sociologists" while avoiding the methodological and empirical problems he carefully details. For example, in sharp contrast to his treatment of concepts incorporated in the "economic" model, his discussion of values as explanatory variables is much less rewarding. We are generally left with much ambiguity and little guidance on how to proceed. For example, he contends "that 'values' can be distinguished as a subclass of 'goals' " but that this "certainly cannot be done in the anecdotal fashion beloved of writers such as Lipset and Eckstein" (p. 92). He thereby suggests that values can be subsumed under a rationalist means-ends calculus, but how this is to be accomplished remains unclear. Except for an occasional reference to George Homans, Barry seems unaware of the movement in sociology—one of several movements distinct from the neo-Parsonian synthesis—that treats values as integral components of exchange relations. A second avenue left unexplored is how "economists" might treat values; instead, we are warned repeatedly that the introduction of nonmaterial goals (e.g., altruism) "weakens the deductive power" of the model (p. 23). Such a warning ignores the fact that every rational theorist from Downs to the present has stressed that self-interested behavior need not be selfish, and that such behavior need not be irrational unless the individual acts in ways that he knows will not help achieve his goals. In short, Barry

offers little guidance for those sociologists and economists who wish to focus on values.

But these shortcomings are minor in comparison with Barry's several contributions to empirical political theory. If it does nothing else, this book reminds us all that no amassing of data and no mongering of jargon can offset the power of sound logic and lucid prose. In a rising tide of dross, this is again no small accomplishment for Brian Barry.

The Calling of Sociology and Other Essays on the Pursuit of Learning. By Edward Shils. Chicago: University of Chicago Press, 1980. Pp. 498. $25.00.

David Martin
London School of Economics and Political Science

This third volume of essays by Edward Shils comprises reflections on the peculiar kind of enterprise in which we as sociologists are engaged. In *The Calling of Sociology and Other Essays on the Pursuit of Learning*, we have a delineation of our subject matter and a setting forth of the proper objectives, rationale, and right conduct of a quite special venture of the human mind. When Edward Shils writes of vocation he is, of course, placing himself in a Weberian tradition which embodies a kind of broad seriousness and a reflective mode of intellectual self-conception. The idea of a calling is part of what he "professes," and this profession is not wholly realizable so long as we remain narrowly professional. Our opus proprium lies in the open forum of learned pursuits. That is why the key idea of vocation in the first part of the title is filled out by the "pursuit of learning" in the second.

Alongside this discourse on the sociological approach and our professional condition, we are given a set of histories of the subject, some lengthy, some pithy, each designed to illuminate a particular thematic concern. Edward Shils does not write a definitive history, tucking everything into place in a smooth sequence, because that is contrary to his view of things. One may know it all, as he himself undoubtedly does, but it cannot all be put together at one time or hewn according to some governing principle. History, especially maybe the sociological history of sociology, has to be come at time and again to exhibit its protean reality. Presumably, this is why Shils prefers the essay form. His thinking is big enough and various enough to escape the limitation of a book specifically about this or that. The essay is his kind of intellectual economy in every sense of the word "economy." This is not to say, of course, that Shils is a subjectivist for whom society is a convenient object for belles lettres. On the contrary, the search for a more comprehensive and many-faceted objectivity is one of his principal aims.

These histories are variously concerned with the progress of the subject in the public mind and its relationship to policy, and with the different ways in which technique, fact, and theory have been placed at the service

of social criticism and schemes of human betterment. They also trace complicated genealogies and affiliations which have been set up as sociology has embodied itself in criss-crossing traditions and established a base in this or that institutional niche. Inevitably and properly, there is a major focus on the centers of sociological learning in the United States, as, for example, in the 1963 essay "The Contemplation of Society in America." But there is a vast range of comparative reference, particularly in the 1970 essay "Tradition, Ecology and Institution in the History of Sociology," which also traces developments in France, Germany, England, and elsewhere. This brings out the heterogeneity of sociology and the imperfect assimilation of "many coalescent, subsidiary and tributary traditions."

Many of these essays are themselves already part of the history of the subject, especially those which reflect the expansive spirit of the late fifties and early sixties. These documents of the heyday show the fitful, partial, but nevertheless palpable progress of sociology in public acceptance and academic esteem. They also make it clear why and in what way sociologists were aggressive or philistine, too technically narrow or theoretically ambitious, and why and in what way neighboring disciplines were resistant, fearful, contemptuous, or uncomprehending. Yet sociology was incorporated in the normal manners of public debate and slyly insinuated into the substance of history, political science, and educational theory. The 1976 essay "The Pursuit of Knowledge and the Concern for the Common Good" shows how extensive was the subsequent change of atmosphere. After referring back to a great age from the mid-thirties to mid-sixties, and the occasional denunciations that faintly marred it, Shils turns to the spate of internal and external criticism initiated by C. Wright Mills. There is a slightly sharper tone here as he deals with the type of criticism associated with Alvin Gouldner, and with the time when domain assumptions and Kuhnian paradigms were all the rage in postgraduate seminars. This important essay carefully cuts wild relativism down to size and turns the edge of exaggerated imputations.

What all these essays, and especially the much-enlarged revision of "The Calling of Sociology" (1961), are valuable for is their conception of a humanistic discipline. This is, perhaps, especially congenial to British sociologists, small subtribe though they are, because their roots in philosophy and in broad humane learning were never lost in technicality and in professionalism. Whatever the proper concern with deviants and deprived groupings, there is a sociology that never reduces itself or rejects nourishment from letters and high learning. What Shils most of all detests is the *déformation professionelle*, operating with vague and barbarous abstractions and an ignorant indifference to the resources of language. What he most of all seeks is a sociology grounded in careful research but open to an educated public and made gracefully available to any intelligent person who shares its concerns.

This kind of sociology does not desert the varied possibilities of opposition and criticism, but neither is it blindly opposed to tradition and civility and dedicated to the radical snobberies about "the mass" that appeal so readily

to the intelligentsia. On the one hand sociology will always impugn the grounds of human faith, authority, legitimation, and justification; on the other hand there is no need for it to be stuck with the "quasi-Marxist, populistic, rationalistic, anti-authoritarian blindness to the nature and working of tradition." Sociology, says Shils, misses the "ultimately vital" because it insists so much on being religiously unmusical. Sociology is really about the self-understanding of man, and of man conceived as an entity beyond the boundaries of skin and person, with memory and the capacity "to incorporate images of others into an intermittently open self." It is *not* about keeping up with the journals or making the sociological profession the principal point of reference.

What Shils is doing turns in the end around sociology as education, rigorous but not technicist, knowledgeable but not scientistic, critical but not blindly contumacious, humanistic but not subjective, multifaceted but not merely relative, tracing all determinations with an openness to the possibility of option and the richness of sheer being. A sociologist, appropriately educated, is one who knows the modes, means, and types of understanding human relatedness and who comprehends the logics, rationales, links, locks, and networks of action from every viewpoint accessible to human intelligence. That is a kind of comprehensiveness and of comprehending exemplified in Shils's own work.

Scholarship and Partisanship: Essays on Max Weber. By Reinhard Bendix and Guenther Roth. Berkeley and Los Angeles: University of California Press, 1980. Pp. ix+313. $18.50.

Paul Shapiro
New School for Social Research

It is a bibliophile's joy to welcome the reissue of Reinhard Bendix and Guenther Roth's *Partisanship and Scholarship: Essays on Max Weber* (originally published in 1971). This important volume consists of a series of interpretive and historical essays which provide a Weberian theory and praxis for the performance of comparative historical sociology. These essays convincingly remind us that Weber was a proponent of the life of reason and an undaunted critic of irrationalism.

The essays effectively dispel the distorted images spread in academia of Weber as being politically either a harbinger of fascism or the exorcist of Marx's ghost, and intellectually either an evolutionist or a bureaucracy theorist.

If these images of Weber are superficial and inadequate, is there a viable and universal image of Weber still to be found? This volume offers an encompassing and enduring image of Weber as the historian sociologist/nominalist par excellence. A parallel theme here is that of Weber as the moral prophet of the "Social Reality Principle" (a term coined by Benjamin

Nelson) (p. 56). Weber used the social reality principle like Occam's razor, to challenge the prevailing monisms of his time—mechanism, determinism, evolutionism, and others. Weber's present critics, including Marxist and natural law theorists (pp. 58–59, 62–64), are proponents of these older monisms and advocate absolutist, unidirectional views of the universe. They cannot abide the polyvalent universe suggested by Weber's methodological pluralism. Indeed, the warring gods are not dead and still contend among us.

The major empirical task in this volume is the development of an employable Weberian schema for comparative history. The theoretical-procedural grounding for this schema is explored in the essays "Sociological Typology and Historical Explanation" (pp. 109–28) and "The Comparative Analysis of Historical Change" (pp. 207–24). The empirical pudding may be savored in the historical essays "Patrimonialism and Empire Building" (pp. 156–79) and "Japan and the Protestant Ethic" (pp. 108–206). Unfortunately, the once topical essay "Charismatic Leadership" (pp. 170–87) now appears dated and journalistic.

The philosophical vistas of the volume appear imaginatively derived from both Weber's own methodology and the analytic of the sociology of religion. The incorporation of these vistas is effectively displayed in "Weber's Generational Rebellion and Maturation" (pp. 6–33). The Weberian notion of the conflict of worldly life spheres and religious imperatives seems to be used here as a device for exploring political and personal conflicts in Weber's own life. Weber appears as liberating himself from two contradictory strands of the Lutheran accommodation of worldly spheres—pacifist pietism and deferential authoritarianism. His methodological notion of value orientation is employed here as an interpretive tool. Thus, he himself can be interpreted as having a value orientation directed toward religion/world conflicts and accommodations. This essay has the merit of demonstrating the greater explanatory power of sociological biography compared with psychobiography.

The perspectives of Weber's valedictions on science and politics are expanded by Bendix in the rich essay "Sociology and the Distrust of Reason" (pp. 84–105). Bendix here expands on Weber's observation that science is not equipped to provide ethical grounding for the ultimate worth of its works. He further observes that attempts by science to offer and ground itself as a utopian panacea generally fail, and, in consequence, a distrust of reason is generated. This attempt by science to accommodate itself to the secular world violates those norms of value neutrality upon which the truth value of science rests. Science degenerates into scientism, whose failures thereby undermine the truth acceptability of science itself. The parallel to the degeneration of the Protestant ethic should be obvious. The Protestant ethic ironically spawned materialist utilitarianism. The scientific ethic spawns narcissistic scientism. Bendix proposes a stance of "cultivation of judgment and moral sensibility" (p. 104) as a corrective against scientism and irrationalism. Such a stance embodies the components of Weber's ethic of political responsibility, that is, passion, responsibility, and a sense of proportion.

There are two disappointing moments in this fine collection. First, the Nietzschean influence on Weber is not as limited as this volume suggests (pp. 22–25). Weber's two great bearers of cultural responsibility, the scientist and the politician, can be interpreted as being the analogues in a disenchanted world of Nietzsche's bearers of history—the artist, the saint, and the philosopher. Weber's two ethics for scholarship and politics—value neutrality and responsibility—may be derived from a nominalistic inversion of Nietzsche's doctrines of the transvaluation of values and the will to power. Indeed, Weber's valedictory images of the Ice Age and the watchman may have been intended as sobering rejoinders to Nietzsche's egoistic employment of the same images (K. Lowith, *From Hegel to Nietzsche* [New York: Doubleday Anchor, 1967], pp. 319–20). An essay on Weber and Nietzsche in the manner of Bendix's own gem on Weber and Burckhardt is needed. A second disappointment is the abandonment of the suggestive schema for the sociology of sociology based on (s)elective affinity which appears briefly in "Value Neutrality in Germany and the United States" (pp. 35–36).

These criticisms are merely cavils. *Scholarship and Partisanship* provides an engrossing introduction to the corpus of an archpatriarch of social thought. These erudite essays are marked by clarity, historicity, and grace and communicate a tone of sanity and civility. This work should prove fundamental to both Weber scholarship and the history of ideas.

Revivals, Awakenings, and Reform: An Essay on Religion and Social Change in America, 1607–1977. By William G. McLoughlin. Chicago: University of Chicago Press, 1978. Pp. xv+238. $12.50.

Mary Jo Neitz
University of Missouri

William G. McLoughlin is one of the foremost authorities on American revivalism. *Revivals, Awakenings, and Reform* draws on over 30 years of research in this area, beginning with the first great awakening in colonial times and including Billy Graham and the present. This essay, however, attempts to be a great deal more than a timely review of revivalism during a period of increased interest in evangelical religion. McLoughlin uses the theoretical perspectives of other social scientists (most notably Clifford Geertz, A. F. C. Wallace, and Robert Bellah) to present his interpretation of the role of four great awakenings in revitalizing American culture. This amounts to a reinterpretation of American history, or at least the history of American culture.

Following Wallace, McLoughlin is concerned with the power of culture to legitimize institutional structures and to integrate individuals into them. When changes occur in the society, individuals (and families) suffer dislocation until the culture adjusts itself to the new conditions. McLoughlin

argues that "awakenings"—seen as a particular form of Wallace's revitalization movements"—are necessary if the culture is to survive the trauma of social change, and that revivalism has served this purpose in American society. Awakenings have no single cause but begin with a general crisis of legitimacy that occurs when a society finds that its day-to-day behavior has deviated so far from the accepted norms that neither individuals nor groups can sustain the common set of beliefs by which they have been taught they should act (p. 12). The period of transition is confusing and tumultuous, but it ends with a social and intellectual reformulation which reaffirms core beliefs (although it reinterprets them). The result of an awakening is a (re)unified nation.

For McLoughlin the core beliefs of American culture were born in an awakening, the Puritan movement which swept England from 1610 to 1640. The "cultural core" consists of four beliefs: (1) that Americans are a chosen people; (2) that their "manifest destiny" is to lead the world to the millennium; (3) that their democratic institutions, bountiful resources, and concept of a free and morally responsible individual all operate under a higher body of moral laws; and (4) that the prevalent Judeo-Christian ethic promotes the general welfare by allowing each individual the greatest possible opportunity to fulfill his or her potential (pp. xiv, 24–44). According to McLoughlin, later awakenings reworked these core beliefs, updating them to correspond to changes in society—for example, emphasizing the role of the individual to a greater degree during the industrial revolution, and to a lesser degree with constricted opportunities in the 20th century.

According to McLoughlin, then, the first great awakening (1730–60) should be seen not merely as a period of religious turmoil prefacing the American Revolution but as a revitalization of the core (puritan) beliefs which resulted in the unification of the colonists as "Americans" who shared a unique destiny. The second great awakening (1800–1830) helped to define what it meant to be an American and what the manifest destiny was. The third great awakening helped Americans come to terms with evolutionary science and industrial progress and led them into crusades to make the world safe for democracy. Finally, McLoughlin sees us now embroiled in a fourth great awakening (beginning in 1960) leading to a more pluralist vision. Although the exact content cannot be predicted, McLoughlin suggests that it will emphasize the sanctity of the individual and the importance of the general welfare, the sense of the holiness of life, and the view that man is in the world but not of it (p. 215).

McLoughlin's views are somewhat innovative with regard to the first and second awakenings, but it is his analyses of the third and fourth awakenings that really depart from the received wisdom. Asking that we rid ourselves of the "old Protestant definition" of revivalism and awakenings, he presents his own view that an awakening, as distinct from the revivals themselves, is an ideological reorientation in the culture. Therefore, the Moody revivals of the 1870s and 1880s did not constitute an awakening; whereas, by McLoughlin's definition, the period from 1890 to 1920 (characterized by the Billy Sunday revivals, among other things) was an awakening. There are

both gains and losses from this transformation of the definition. On the one hand, it allows McLoughlin to look at changes in American values from a broad perspective rather than restrict himself to a narrowly defined religious phenomenon. On the other hand, our understanding of the religious phenomenon, the revivals themselves, is somewhat muted by the new definition.

There is a difficulty that pervades the literature on revivalism in the 20th century to which McLoughlin's revised definition offers a solution. The first and second great awakenings are seen much as McLoughlin presents them: periods of cultural transformation with a generally progressive impact on the culture as a whole. But starting with Moody the changes coming out of the revivals tended to be organizational rather than ideological; revivals became identified with backward rather than progressive forces in the society. The important questions raised by science and by changes in the social order were not answered by the fundamentalist rhetoric of the revival preachers. In McLoughlin's terms, there was no ideological reorientation. The liberal Protestantism and social progressivism that McLoughlin sees as providing the answers in the third great awakening were quite outside the revival tradition. Discussions of revivalism after the second great awakening, then, bear a tone of disappointment over this loss of forward motion. Researchers express the sense that these later revivals are somehow "inauthentic" religious experience. Whereas the first and second great awakenings are described as the genuine article, later revivals are termed "public spectator sports" or secular rites of the evangelical churches—mass produced for a mass audience in a mass society. McLoughlin avoids this disappointment by claiming that these later revivals, though true revivals, are not true revivalism, not awakenings (although some may constitute a "nativistic phase" in awakenings understood in the proper way).

This disappointment among the historians of revivalism, however, deserves some attention. It reflects a "liberal bias" among intellectuals who dismiss evangelical religion, including revivalism, because they see it as intellectually regressive, politically conservative, and appealing to people who are deprived economically and/or psychologically. One wonders to what extent the judgment of later revivals as inauthentic or as not really being revivals comes from the researchers' identification with liberal social movements.

Since revivals no longer function to unify the culture under a new revised version of the core beliefs but instead cling to old beliefs, McLoughlin looks about for other social movements which will fulfill the function and carry the society forward with a new consensus. The utility of his definition hinges on whether or not we can really talk about ideological reorientations of American culture in the way that he does here. The anthropological models that McLoughlin uses are taken from studies of small, relatively homogeneous societies quite different from American culture even in colonial days. It is arguable that the "puritan core" identified here applied more to certain colonies than to others and perhaps only to the dominant culture within even those colonies to which it applied. American culture has always been made up of numerous subcultures, and this complicates enormously

any discussion of cultural change. Furthermore, the models used here did not originally apply to modern "secular" societies. While the secularization thesis clearly needs to be rethought, McLoughlin's functionalism ignores it, for example, by including in the fourth great awakening everything from Billy Graham to Ginsberg, death-of-god theology, drug culture, and the antiwar movement. For me, then, the redefinition of awakenings is pretty, but ultimately unsatisfying. It provides an interesting model for examining changes in American culture, but it implies a degree of consensus which I am not sure exists, and it ignores some important questions about revivalism and the study of revivalism.

Nonetheless, *Revivals, Awakenings, and Reform* is an important book and deserves to be read by sociologists interested in social movements, religion, social reform, and the sociology of culture. McLoughlin writes well, and in this book he tells the reader a great deal about revivalism. He knows the theological issues addressed in each awakening, and he is sensitive to the sociologists' concern with changes in institutions (especially the economy and the family) and their relation to the theological issues. At a time when evangelicals are once again attempting to influence the national agenda, it is useful to know something about evangelical religion and the historical connections between it and social reform. Although I remain unconvinced by his grand interpretation of the history of American culture, I found this essay valuable for the presentation of the particulars of revivalism within a larger social context.

A Just Measure of Pain: The Penitentiary in the Industrial Revolution, 1750–1850. By Michael Ignatieff. New York: Pantheon Books, 1978. Pp. xiii+257. $12.95.

Paul Rock
London School of Economics and Political Science

A Just Measure of Pain is a highly focused work. It concentrates on developments in the Nonconformist penology of late 18th- and early 19th-century England, exploring how the penitentiary emerged as a special project of Quakers, Independents, Methodists, and Evangelicals. It refers to much else, but its other themes are rather contextual and incompletely detailed. A history of the prison might have encompassed the social organization of crime, control, law, and the state. It might have discussed the evolution of parallel problems, remedies, and institutions. It might have mapped the social world of the English reformers. But *A Just Measure of Pain* is a history of ideas, and those allied topics have been almost entirely bracketed, their bearing and character being defined as not really important enough to complicate seriously the principal argument.

Michael Ignatieff has refrained from analyzing the frameworks, relations, and antecedents of Nonconformist thought. His description is more imme-

diate, touching on that thought only when it affected the biographies and activities of salient reformers. His history of ideas is accordingly a chronicle of personal syntheses and accomplishments. It tends to lack a clear system, but no clear system may have been discernible. It is a scholarly, lucid, and sympathetic account which makes the penitentiary an intelligible outcome of applied theology: Nonconformists "took up the idea of the penitentiary because, as a system of authority and as a machine for the remaking of men, it reflected some of their deepest political, psychological, and religious assumptions" (p. 79). It is that affinity between an institution and a cosmology which supplies the book's major thesis. Ignatieff argues that sectarian Protestantism created an anxiety about salvation. It also provided an answering altruistic impulse which gave transcendental meaning to existence. Thus John Howard was prone to self-vilification and sought purpose and justification in moral works. Others turned to philanthropy as an escape from a total commitment to the secular world of commerce and finance.

Prisons, in their turn, represented a most appropriate target for reform. They were patently ill-organized, oppressive, and inefficient. Instead of saving their inmates, they condemned them to exploitation, physical harm, and spiritual neglect. As important, prisons could serve as a powerful metaphor for man's general condition. They were a microcosm or allegory of the wickedness of mankind, the universality of damnation, and the wait for judgment. It was that sense of shared sinfulness which led some reformers to claim a rough equality with prisoners. They acknowledged a common identity which superseded other divisions.

A collective experience of guilt and its solution were already lodged in Nonconformist organizations. Protestant discipline provided a model for the communal control of others: penitentiaries could become like the meetinghouse or the chapel, an assembly of quiet, reflective, and well-regulated people bent on salvation. Borrowing from the examples set by the Ghent *Maison de Force*, the Amsterdam Rasp House, and the prison of San Michele, the reformers urged the imposition of an austere and monastic regime which would eliminate sloth, vice, disease, and waste.

Ignatieff holds that the idea of the penitentiary became attractive to the state when England underwent a crisis of social control at the end of the 18th century. The American War of Independence had prevented the transportation of convicts; the resort to terror was thought to have failed as a deterrent and a corrective; there was great lawlessness; and prisons became places of mass punishment for the first time. Escapes, typhus, and insurrection made their constitution and management problematic, encouraging the adoption of the promising schemes of Howard, Paul, and the other reformers. The very policy of prison building increased capacity and invited a more frequent use of the custodial sentence. The penitentiary emerged as a central institution, looming, like Pentonville, "over the workers' quarters around it, a massive . . . fortress of the law" (p. 3).

Ignatieff has written a most persuasive analysis of the ideology of Nonconformist moral enterprise. He has emphasized its ambiguity, complexity,

and indefiniteness, alluding repeatedly to debates and dissension in reforming circles. The redesigning of prisons was an emergent and complicated process, at first espoused and then criticized by radicals and Whigs. It was built upon contradictory and confused principles which were never properly resolved. It embodied ideals which were either unrealizable or associated with unpleasant unintended consequences: Solitary confinement could engender madness as well as pious contemplation. Improved hygiene and management could make imprisonment more tolerable than poverty in the world outside.

A Just Measure of Pain is less persuasive when it moves beyond intellectual history. It has surrounded the history of the penitentiary with cursorily examined and sometimes indefensible assumptions. Many of those penumbral arguments are rather poor criminology or poor political sociology. For example, Ignatieff baldly asserts that "the general decline of crime rates after 1855 in England . . . owed little or nothing to the consolidation of the penitentiary system and much more to the general rise in real wages among the working class" (p. 209). Not only is this alleged connection between prosperity and crime unsupported, but it would be difficult to advance unequivocal support. It is mere speculation. Similarly, Ignatieff describes the penitentiary as a response to a significant failure of the policy of terror in the 1780s and to a more general state of alarm about the collapse of social order. What are not apparent and never discussed are whether that policy had ever worked at *any* time and whether that alarm had not always been present. Almost all the social historians of crime have a propensity to regard their own particular period as uniquely troubled by disorder. Such crises are important, but they also require comparative analysis and some qualification by the phenomenology of moral panics. They cannot be listed as if their novelty and causality were self-evident.

As perturbing are declarations that *A Just Measure of Pain* was designed to discover "where the rich and powerful of English life placed the outer limits of their power over the poor" (p. xiii) and that charitable reforms were "integrated, of necessity, into the government's strategy of social control" (p. 164). Ignatieff has been most diligent in his reproduction of Nonconformism. His discussion of "class rule" (p. 162), the state, government, and power is not diligent at all. He introduces those concepts without adequate definition or amplification. On occasion they even appear a little anachronistic. Thus the "state" and the "state's ultimate power" are cited, but, on inspection, it seems that the state was neither very organized nor very powerful. The state was so internally riven and loosely structured that it is not always possible to grasp it as a coherent entity. It could be balked or checked by magistrates who resisted "anything that smacked of direction from Whitehall" (p. 168); by the populace, who would have made it "impossible to introduce [the police] into the streets of London" if they had "served only the employers and the rich" (p. 186); and by the boroughs: "many boroughs had secured exemption from [the] provisions [of the Gaols Act of 1823] altogether, and of the 130 required to submit annual reports to the Home Secretary, only 80 had bothered to do so. The Home Secretary,

in turn, lacked even the power to order the closure of the small town lockups where abuses were worst" (p. 187). The Penitentiary Act of 1779 provided for the establishment of two penitentiaries, but no prison was built because grave constitutional problems arose from the state's attempt to become involved in what was normally a locally administered institution. In short, Nonconformist ideology was supposedly sponsored by a powerful state which adopted strategies of class rule. On examination, Ignatieff's own very explicit arguments prompt a redefinition of those strategies as the fumbling, indecisive reactions of a weak state ill-equipped to police its population. This kind of inconsistency suggests that *A Just Measure of Pain* has imported ideas from inappropriate theories and contexts. The book is unconvincing in its attempt to link the appearance of the penitentiary to the astute maneuvers of an undivided ruling class and an articulate state. But its pivotal theme is well told. The work is an impressive achievement.

Damnation and Deviance: The Protestant Ethic and the Spirit of Failure. By Mordechai Rotenberg. New York: Free Press, 1978. Pp. xxii+232. $12.95.

Jack Katz
University of California, Los Angeles

According to Mordechai Rotenberg, Calvinist beliefs in predestination are at the root of current assumptions about the irreversibility of deviance. Rotenberg works up a parallel between 20th-century notions of inherent deviance—he refers most often to criminality and psychosis—and damnation as understood by 17th-century Puritans. The contemporary legacy of the Protestant ethic is a basically pessimistic psychotherapy which does not cure and a criminal justice system which does not rehabilitate. For a more optimistic perspective on deviance, we can look to the Hassidic ethic, which, in contrast to Calvinism, prohibits an introspective search for the causes of evil and celebrates redemption through involvement with others.

Before examining the details of Rotenberg's argument, one might anticipate a treatment of the questions it inevitably raises. He claims that Puritan perspectives underlie the modern view of deviance. What then will he do with the historical claim advanced by David Rothman that the modern American treatment of deviants, including the mentally ill, criminals, and the poor, took shape in the Jacksonian period in a revolutionary departure from colonial precedents? He claims that Protestant ideas are at the basis of our assumptions about irredeemable deviants. Will he then demonstrate a less profound assumption of irreversible deviance in the Catholic West, say in France and Italy, than in societies like the United States and England in which Protestants numerically predominate? What will he make of the work of Michel Foucault, who finds in the history of French prisons the same experience of rehabilitative failure and the same

themes of psychological ideology that Michael Ignatieff finds in the history of the English prison?

How will Rotenberg show that ideas about deviants historically preceded and shaped the major institutions used in modern society to treat deviants? He knows, of course, that the modern form of prisons and mental hospitals was established before the pioneering psychological and criminological theorists were at work. What will he say about the international movement to construct large institutions of confinement for convicts and the mentally ill, and the widespread reports of the failure of rehabilitation, in the early 19th century? What will he say to those who read the historical record to indicate that, once built, the vast, international, state-sponsored apparatus for confining deviants created an intellectual market for the development of social scientific theories that might rationalize the allocation of institutional resources?

Since Rotenberg is considering the influence of religion on the contemporary definition of social problems, we expect him to explain his focus on the Protestant tradition. How will he interpret the contributions of Jewish intellectual pioneers, especially Freud and Lombroso, as manifestations of the Calvinist spirit? How will he convince readers familiar with the movies of Woody Allen that the Protestant spirit, in contrast to a traditional Jewish ethic, breeds lives haunted by guilt and self-doubt?

Perhaps there is a way of discerning in comparative, historical evidence a relation between Calvinism and contemporary assumptions of irreversible deviance. If so, it is not to be found in *Deviance and Damnation.* Then how does Rotenberg try to convince us? By "the dialectic process of constructive verification . . . an explanatory intellectual system that ties a body of diverse and contradictory facts into an encompassing hypothesis." (p. xix). Summaries of a few chapters should convey the idiosyncratic character of Rotenberg's "dialectic process."

Chapter 1 consists of: a historical review of metaphors of deviance that, in five pages, runs from ancient Greece and Rome, where "treatment . . . was rather humane," to "the advent of [the] . . . inhumane attitude toward deviance" in the Dark Ages, to "the prevalent medical model of deviant behavior" which "cast[s] them [deviants] into irreversible 'sick' roles" (pp. 4–8); a four-page analysis of the "Calvin-Weber predestinal paradigm" (pp. 9–12); a two-page argument of "the possible continuity between medieval witch-hunting, predestination and the social-scientific school of positivism, as well as the impact of all three on criminological-penal theories and practices" (p. 15); and five pages on thematic parallels between Calvinism and psychotherapy (e.g., "the therapist's traditional insistence on the single 'passive-receptive role' . . . can be attributed to the generalized Protestant disbelief in man's ability to change [p. 20]).

Chapter 2 analyzes sermons, other theological writings, and contemporaneous records which describe deviant individuals to document that the Puritans of colonial Massachusetts sorted people into two irreversible categories, damned and elect. The next chapter jumps to the present and to other issues. It attacks the concept of the psychopath as a crude, vague,

and unverified label and then argues for a conceptualization of "forms of insensitivity" that are produced through significant socialization experiences. The relation of this issue to the overall theme of the book is not clear. Perhaps the idea is that a "form of insensitivity" is a less irreversible defect than "psychopathology," a psychotherapeutic term presumably rooted in Calvinism.

For supporting evidence, Rotenberg reports a study in which Israeli prisoners were divided into psychopathic and nonpsychopathic groups according to the diagnosis of either a clinical psychologist or a psychiatrist. The psychopathic prisoners were found to have fewer somatic problems. Rotenberg offers this finding to criticize the label "psychopath" on the grounds that it obscures an insensitivity that is functional in some settings. But the study also (and more obviously) shows Rotenberg's faith that the label "psychopath," as used by professional psychological workers, is not vague and arbitrary. He treats the sorting of the prisoners as reliable and valid.

Chapter 4 begins with a criticism that sociological writing on the "labeling" of deviants has ignored the conditions of self-labeling. (What about Goffman's *Stigma*, Matza's *Becoming Deviant*, and Lemert's classic essay on paranoia?) Rotenberg presents a model (a number of variables like primary and secondary others) with examples drawn largely from his imagination ("a typical upper-middle-class 'nice Jewish boy' who labels himself a radical 'hippie' in order to reduce the strain produced by his 'Jewish Mother's' pressure to become a materialistic, successful scientist, doctor, or businessman" [p. 87]) rather than from ethnography, journalism, or fiction written by others. The relevance of this chapter to the overall theme appears to rest on the claim that there is a " 'Protestant bias' in conventional labeling analysis." Matza, Becker, Kitsuse, and others, by asserting that people who are labeled deviant automatically and irreversibly see themselves as deviant, have embedded Calvinist ideology in modern sociology.

Chapter 5 contains the book's most developed effort to give an empirical basis for the hypothesized relation between Calvinism and notions of irreversible deviance. Israeli and American students were given a "Protestant Ethic" questionnaire. This questionnaire appears essentially to seek conservative political attitudes, not a deeply felt need to prove oneself through worldly success. Is it not possible to disagree with the idea that "if one works hard enough, he is likely to make a good life for himself" (p. 105), and still feel compulsive about work?

For a measure of the dependent variable, the students were given a biographical sketch and asked questions "which aimed at eliciting their spontaneous tendency to attribute retrospectively past success or failure signs to present state" (p. 105). The American and Israeli students differed significantly in their imputations, but "differences between Israelis and Americans on the Protestant Ethic scale . . . were not very great" (p. 109).

Rotenberg then reports another small study which compares the self-labeling of European and Eastern mental patients. There are provocative findings: "All new patients, regardless of ethnic origin, believed significantly

more than old patients in their chances to be cured" (p. 117). But he concedes that one could question the utility of this study for establishing the Calvinist hypothesis: both groups were Jewish.

Rotenberg primarily uses his imagination not, as he claims, to illustrate a type of finding which, if established, would bolster his theory, but to list the damages that might be laid to Calvinism. In chapter 7, we are told that Calvinism caused loneliness and alienating individualism to arise in the West. Nowhere are we asked to consider competing explanations emanating from political, economic, or even religious variables, and there is no shortage of alternative perspectives. Indeed, I would bet that most readers would find more sociological imagination on the influence of religion on modern social science perspectives on social problems, and no less evidence, in John Cuddihy's *Ordeal of Civility* (New York: Basic, 1974), a Catholic's speculation that Jewish identity problems underlie it all.

Suicides. By Jean Baechler. Translated by Barry Cooper. New York: Basic Books, 1979. Pp. xxiii+474. $21.00.

Ronald W. Maris
University of South Carolina

Essentially Jean Baechler has constructed a Weberian ideal typology of suicides and then elaborated and illustrated the meanings of these types by reference to cases from the Human Relations Area Files and other secondary sources. His approach to the study of suicide is a priori, logical, and historical in the tradition of Weber and Douglas. The book begins with the definition that "suicide denotes all behavior that seeks and finds the solution to an existential problem by making an attempt on the life of the subject" (p. 11). It follows from this definition that suicide is a logical solution to life struggles which a minority of the human population find themselves engaged in and otherwise unfit to cope with. Suicides are also viewed as universal, transhistorical, and the products of free choice. Suicides are not crazy or abnormal, even though they are rare. Baechler includes nonfatal suicide attempts in his definition of "suicides." Although external factors (like war or religion) can enter into the decision to commit suicide, the most important factors are "personal," such as genetics, age and sex, and family and psychiatric contexts. Baechler is interested particularly in personality types which utilize exclusive strategies of what he calls "power" or "dependence."

According to Baechler there are exactly 11 ideal subtypes of suicides, each with its own distinctive meaning. These 11 are grouped into four major types: (1) escapist, (2) aggressive, (3) oblative, and (4) ludic. Escapist suicides involve flight (from an intolerable situation), grief (loss), and punishment (expiation for fault). Aggressive suicides involve vengeance (to provoke remorse or community opprobrium), crime (to involve another

in the suicide), blackmail (to put pressure on another), and appeal (to inform friends and neighbors of danger). Oblative suicides involve sacrifice (to save or gain a value higher than one's own life) and transfiguration (to attain a higher state). Finally, ludic suicides involve ordeal (risking life to prove oneself or to solicit the judgment of others) and game (playing with one's own life). All of these meanings can be institutionalized. That is, under certain circumstances a collectivity provides suicide as a solution to life problems. There is an isomorphism of personal meanings and institutionalized meanings of suicides. Thus, all 11 subtypes exist at the institutional level. In fact, institutionalized suicide types tend to confirm the existence of the 11 types at the personal level. Examples of institutionalized suicides include: the ritual suicides of old Eskimos (escape), Malay *amok* (aggressive), the hunger strike (aggressive/blackmail), and the Kamikaze (oblative). Of course, in real-life situations all of the pure types of suicides are mixed. This makes suicide prediction or prevention difficult, even if it were desirable.

I must confess to a fundamental ambivalence about *Suicides*. On the one hand, there is no doubt that the book is thoughtful, thought-provoking, very well written, and interesting from start to finish. It may be tempting to conclude that Baechler did a fine job as far as he went but that he stops short of what needs to be done. But while there certainly have been typologies of suicide before and Baechler comes up with few, if any, new meanings of suicide, none have been done so thoroughly and with such careful attention to definitional detail. The reader will also be pleased with the abundance of historical documentation.

On the other hand, there is something disturbing about *Suicides*. I suspect that this "something" derives from (1) a radical nominalistic reductionism reminiscent of Douglas's *Social Meanings of Suicide* (Princeton, N.J.: Princeton University Press, 1967) and (2) the iconoclastic style in which *Suicides* is written. While I agree with Baechler that much of the statistical analysis of suicides and suicide rates garbles the various meanings and types of suicides, it is not the case that the fault lies with the statistical methods for analyzing aggregate data. The fault results instead from careless or inappropriate use of the methods. For example, modern multivariate analysis techniques could isolate different types of suicides just as well as or better than Baechler's more subjective approach. Theory and methods need to be used hand in hand regardless of one's approach to the study of suicides, but Baechler is inhibited by his dislike of statistics. How else can one explain his not respecting his own data (p. 39), his thought and imagination games (p. 101), and his incurable speculation and a priori reasoning? In reading *Suicides*, I get the uneasy feeling that Baechler's suicide cases (i.e., his data) have almost no bearing on his arguments, in the sense of his theory being falsifiable. This is especially maddening when he has final recourse to the claim that his types of suicide, after all, are ideal, not real. The ultimate challenge in the study of suicide is to predict suicide. Baechler always backs away from this challenge with statements like "scientifically, suicide did not exist" or "each concrete suicide is an unfathomable mystery"

(p. 340) or "this book, if it is read, will not have the slightest influence on suicide" (p. 34). Perhaps if he had known some actual suicides, his perspective might have been different. I do know that nonfatal suicide attempts and completed suicides are not the same behavior and cannot be encompassed in the same definition. In this sense the book and its definition of "suicide" are at least misleading.

Finally, stylistically Baechler shocks ("suicide is not to be found among the mentally ill"), exaggerates (e.g., the evils of statistics or the backward state of the study of suicide), engages in pseudoexactitude ("I have been able to distinguish exactly eleven [types of suicides]"), and uses unnecessarily obscure words (e.g., "ludic," "oblative," "toxicomania," etc.). One would also like to know what the systemic interrelationships of all the personal factors in suicide (see chaps. 9–13) are, how the strategies of power or dependence are different from the other abstractions which Baechler criticizes, why his suicide cases are so short and superficially presented, and how he would go about integrating Phillips's work on the suggestibility of suicide. I could go on. To be sure, there are problems with *Suicides*, some serious ones. Nonetheless, it is provocative, formulates a useful and carefully developed typology of suicides, contains a wealth of well-written historical materials on suicides, and is one of the very few truly *thoughtful* treatises ever written on the subject.

On Clichés: The Supersedure of Meaning by Function in Modernity. By Anton C. Zijderveld. London and Boston: Routledge & Kegan Paul, 1978. Pp. x+129. $15.75.

George C. Lewis
University of the Pacific

I really do not agree with the ideological position Zijderveld develops in *On Clichés*, but how can anyone not love an essay such as this? One that wistfully points out that there seem to be no more "white spots" on the sociological map (like the uncharted realms of Africa across which were always penned "Here Be There Lions") or "black holes" in the fabric of social reality (such as are stimulating contemporary astronomy to produce some of its most exciting work ever).

Instead, contemporary sociology seems to Zijderveld to be little more than a cliché writ large across the social sciences. From his point of view, even the movements of the 1960s and 1970s, such as Marxism, Freudianism, phenomenology, existentialism, and Wittgenstein's brand of analytic philosophy, are not the fresh approaches their converts bruit them to be, but instead a retreat from the neopositivist clichés of the 1950s to clichés of a different methodological persuasion—ones that, in the end, do not really present a new perspective, but are as tired and unexciting as those they

elected to supplant. As Zijderveld points out, these ideas "only seem new to us, because our consciousness has increasingly become a-historical. We possess a very short memory and thus repeat old ideas and notions without actually realizing how worn-out and hackneyed they are" (p. 3). To coin a phrase, there is nothing new under that old sociological sun! No white spots, no black holes, it just keeps rollin' along . . . .

Well, it does us good to hear this sort of thing from time to time. There is more truth involved than many of us like to admit. But it certainly is not the whole story—even for Zijderveld, who uses his concern for the state of sociology as a springboard from which to generate his thesis on the function of the cliché in modern society—a thesis I am sure he would not consider part of the cliché-ridden landscape he sadly shakes his head over in the introductory pages of this essay. (Well, on second thought, perhaps he *would* concede it. Zijderveld seems to have a fine edge of irony in his writing—except when he gets too involved in grinding ideological axes.)

This study of the cliché is Zijderveld's second example of what he labels "cultural analysis," an approach he first presented in *The Abstract Society* (New York: Doubleday, 1970) and which he outlines in a brief appendix to the present essay (pp. 106–13). Basically, cultural analysis is the application of Weber's method of *verstehen* (interpreted in its traditional sense) to the process of modernization in society. With a clearly conservative bent, cultural analysis examines the changes in "values, norms, motives and meanings under the impact of industrialization, urbanization, the rise of science, technology and Capitalism (and its adversary: Marxism), and the rise of bureaucracy. . . . Whereas 'modern' sociologists seem to live and work pre-reflectively within the context of modernity, cultural sociologists ask questions about it. . . . When in a fully modernized society, like the USA in the 1950s and Western Europe in the 1960s, people begin to ask questions as to the quality of life, 'cultural sociology' begins to regain its lost relevance!" (p. 109). In *The Abstract Society*, this perspective was used to argue that contemporary man is endangered by the process of modernization—that he or she is gradually torn apart into what Zijderveld termed a *homus internus* (modern subjectivist) and a *homo externus* (functional role player). This is a position that has been taken by many, including non-sociologists like the late 1960s British rock group King Crimson, whose "21st Century Schizoid Man" makes a similar point (albeit a bit less subtly).

In turning to the cliché in this study, Zijderveld defines it most generally as a "traditional form of human expression (in words, thoughts, emotions, gestures, acts) which—due to repetitive use in social life—has lost its original, often ingenious heuristic power. Although it thus fails positively to contribute meaning to social interactions and communication, it does function socially, since it manages to stimulate behavior (cognition, emotion, volition, action), while it avoids reflection on meanings" (p. 10). In a word, the cliché is highly functional in patching together our fractured, atomistic, role-playing society, even though—by virtue of its functional transformation to a cliché—it has lost the ability to give meaning to the *homus internus* that it began with. As he says elsewhere in the essay,

clichés resemble food that is easy to swallow and easy to digest, without chewing as it were (p. 14).

Clichés, then, can range from McDonald's hamburgers to teenage telephone talk; from the Warsaw Concerto to oft-quoted lines of William Shakespeare ("to be or not to be"); from phrases like "God-given right" (used as a political tool of rhetoric) to the act of student protesters occupying a building in the 1970s (a highly charged symbolic act in the 1960s); and from scientific paradigms to populist pap. "Chunks of stale experience" (p. 16), such clichés move us closer to the situation which, as Zijderveld points out, Weber warned us of—in which human life itself becomes a cliché. "Clichés resemble institutions in all this very much. In fact, we could view clichés as micro-institutions, while the institutions of modernized society tend to grow into macro-clichés" (p. 17).

Zijderveld sees modernity as the result of a supersedure of meaning by function which has led to the emergence of our contemporary "clichégenic society." During modernization, he argues, there is a breakdown of the traditional intermediary structures (family, church, etc.) whose functions were to convey and define meaning between the individual and the larger institutions of society. The individual is left directly exposed to these institutions which impose functional demands, and the "meaning void" created by the destruction of the intermediary structures is filled by clichés. Thus, in a society characterized by "cognitive vagueness, emotional instability and moral uncertainty" (p. 53), clichés provide clarity, stability, and certainty as fixed points of recognition. Like John Wayne, clichés come to the rescue every time.

Zijderveld then proceeds to examine "clichés unbound," which is his closer look at the social and political functions of clichés in modern society. He includes sections on clichés as "knots of communication," as responses to embarrassment and precariousness, as propagandistic tools, and as survivals of magic. Finally he discusses "clichés bound"—stratagems to relativize the power of clichés. He focuses on the areas of religion, politics, science, humor, and aesthetics and concludes with a short section on the stranger as cliché-buster. (The stranger, or novice, sees clichés with fresh eyes, appreciates their original meaning, and strings them together delightedly and endlessly, much to the discomfort of us natives who, because of the enthusiastic stranger, have had called to our attention the tyranny the cliché holds over us all.) These chapters, with their "mini-analyses" of the ways clichés are bound and unbound in modern society, are to me the freshest and most fascinating material of the essay. One does not have to accept Zijderveld's analysis of modern society to get a great deal from what is contained herein.

Along the way, he inserts a chapter on "clichés and boredom," which purports to apply the argument to our modern conception of linear time (vs. the more traditional and subjective experience of time found in "non-modern" societies). Unfortunately, the aim of this chapter seems to be more to discount the meaning of the ideologies of rebellion and revolt of the 1960s as cliché-ridden responses to boredom than to present a balanced

interpretation of the thesis. This chapter is really the only sour note for me in the essay.

One thing that more than balances it, however, is watching Zijderveld struggle throughout the essay with the obvious problem of writing a work containing original meaning by means of the very symbols he is in the process of explaining as clichés which have lost their meaning. He handles this challenge very well and even seems to relish the irony of it all. It is fun to see him deal with it as he develops his major arguments.

In sum, at first glance and overall, Zijderveld does not seem to be saying much we have not already heard from the disgruntled mass society theorists of the 1950s. His is a clearly traditional position, and one that many social scientists, myself included, turned from long ago. Yet he approaches his subject matter from a truly fresh perspective. Ironically, his very concept of the trite cliché in this analysis breathes new meaning into an old debate—and does so just as the sociopolitical tone of this country is more than ready for such a fresh attack on modernity.

A postscript: I have called Zijderveld's work an "essay" throughout this review. This is because, without appendix and notes, it is only 105 pages long. Annoyingly, Routledge & Kegan Paul have *bound* their 16-page catalog to the end of this essay—which pads it out to 135 pages. They are selling this for $15.75. Although I am in sympathy with the economic position of book publishers today, and realize books cost much more in Europe than they do in America, 15 cents per page does seem a bit steep—even for an essay as enjoyable as this.

Language and Control. By Roger Fowler, Robert Hodge, Gunther Kress, and Tony Trew. London and Boston: Routledge & Kegan Paul, 1979. Pp. 224. $22.50.

Language as Ideology. By Gunther Kress and Robert Hodge. London and Boston: Routledge & Kegan Paul, 1979. Pp. x+163. $15.00.

Stephen O. Murray
Language Behavior Research Laboratory, University of California, Berkeley

When students on their own develop ideas that have been elaborated or even empirically established, good teachers encourage them to focus on the validity of the ideas rather than discourage them by pointing out that everything has been said before—and better—by others. Conceivably this approach could be extended to books by full-fledged professionals, although they are usually expected to have a more extensive command of the existing literature. However, when authors claim for themselves an original theory of language in society (as do Roger Fowler et al. in *Language and Control* [LC], p. 4) resting on ignorance and present themselves as "strongly critical of the dominant currents within the disciplines" (LC, p. 5) when they do not seem to have discovered the water, let alone currents in it, questions of scholarship cry out.

A bibliography of relevant work of which these reinventers of various wheels are ignorant would far exceed the space alloted for this review, so I shall only mention some of the topics considered along with the names of some of those unknown to the authors of these two books who have said more, better, and before: bureaucratic formal rationality (Weber), co-occurrence (ethnography of communication), discourse analysis (developments after the early 1950s), focus (Chafe), functions of language (Jakobson, Hymes), hedges (Lakoff), ideology (Mannheim, Habermas, Foucault), the logic of nonstandard dialects (Bloomfieldians, Haugen, Labov), news routines (Gans, Gitlin, Lester, Molotch, Tuchman), reification (Marx), rhetoric (Burke), syntactic uses of intonation (Bolinger, Pike, Gumperz), talking to children (Brown, Ferguson), and writing as control (Lévi-Strauss, Ong, Goody). Their ignorance, though considerable, is not quite total, for they are familiar with some work by transformational grammarians—Halliday, Whorf, and Orwell—but given what they have not read, the decision by the authors of *Language and Control* not to include a bibliography is understandable.

So their scholarship is shoddy; what about their theory of language? They are so intoxicated by their "discovery" that conveying information is not the sole function of language and that there is modality (outside the verb system) in English that they regard all language use as mystification (i.e., never conveying information) and every feature of language as modal. A central tenet of sociolinguistics (clear even in the secondary sources cited) is that language is plurifunctional. Information is not its sole function, nor is control by obfuscation. One major function, completely neglected by Fowler et al., is to create and preserve smooth social relations (between equals as well as unequals). This frequently requires sacrificing clarity to politeness. Although Fowler et al. might be shocked by the notion, Goffman long ago demonstrated that even superordinates are constrained by politeness in interaction.

Gunther Kress and Fowler (LC, p. 64) "regard conversations as peculiar kinds of interviews," speech and writing as conspiracies to hide actors. I (actor) consider such simple-minded conflict theory silly. Moreover, their sample of two interview segments is not a sufficient basis for generalizing about interviews, let alone about conversation. If their analysis of the production of news began by considering news personnel interviewing powerful persons rather than with already constituted wire service accounts, they might have had to reconsider the "irresistible" power inherent in the interviewer role. Similarly, conversational analysis of social scientists interviewing elite persons would reveal that interviewees can ask questions and control topics, as the tapes of my interviews of elite scientists certainly do.

I find it dismaying to have to report that the general theory proposed in these two books is as incomplete as their (authors') scholarship, because they are concerned with some questions currently at the research front in sociolinguistics, for example, modality (beyond the verb system), the creation of cohesion in discourse (beyond the sentence), and the effects of literacy on thought and world view. *Language as Ideology* (LI), by Gunther

Kress and Robert Hodge, includes stimulating ideas about the first of these. Both volumes include provocative analyses of what is going on in sentences, but, with the possible exception of Tony Trew's analysis of news stories (LC, pp. 94–156), little about the relations between sentences, even in literary texts. The irritatingly ignorant and pretentious claim that "the linguistic analyses in this book differ from conventional linguistics and sociolinguistics in taking as their subject real, socially situated and usually complete texts" (LC, p. 195, echoed in LI, p. 14) is false. In these two volumes there are a total of three *segments* of interviews, one speech therapy client narrative, and no examples at all of naturally occurring speech. In contrast, the last is central to work in "conventional sociolinguistics." Furthermore, such "conventional" work provides comparisons of speech in various languages and cultures, whereas Fowler et al. attempt to address Whorfian questions and questions about how language *can* be used with texts from only one language, British English.

I am uncertain about what should be said about their analyses. I suspect that many other sociological readers will also be troubled by questions about how examples were chosen and what the warrant for their interpretations might be. While I find many of their interpretations suspect, I am uncomfortable with substituting my intuitions for theirs and mindful that this problem is not unique to these analysts. One means of alleviating the problems of contesting gut feelings about transcripts is to include interactants' interpretations in the analysis (as Labov and Gumperz have done). For instance, in analyzing nominalization and agent deletion in my phrase (above) "questions of scholarship cry out," analysts telling the world what I meant would ask me what I meant in transforming the kernel sentence "I question and decry their scholarship" (and I would reply that rather than trying to obscure that the opinion was mine, I was taking seriously my role of reviewer and reporting what [I thought] any reader familiar with the literature would conclude, while also seeking resonance with the metaphorical "cries" for revenge of those who have gone before). The producer's interpretation need not be regarded as definitive or even as privileged, but it is important evidence about intent and often helpful in choosing among analysts' intuitions.

Believing strongly that linguistic analyses of real speech (and documents) can illuminate traditional sociological topics (e.g., interaction, power, ideology, socialization, stereotyping, etc.), knowing that many sociologists are at least as unfamiliar with the techniques and results of sociolinguistics as are Fowler et al., and fearing that Gresham's law may apply to ideas (the actors being Routledge & Kegan Paul), I strongly urge those unfamiliar with sociolinguistics *not* to use these volumes as an introduction. Those not so characterizable may profit from the discussion of modality in LI, and those concerned with the production of news (and concomitant reproduction of ideology) may find Trew's work (by far the soundest contribution in either volume) methodologically stimulating and interesting—vitiated though it is (like everything else in these volumes) by what Mannheim more than half a century ago termed "partial perspectivism."

Gender Advertisements. By Erving Goffman. Cambridge, Mass.: Harvard University Press, 1979. Pp. ix+84. $12.50.

Sherri Cavan
San Francisco State University

In the past, students of social life have looked to novels and paintings for insight into the patterned variation of human culture. In *Gender Advertisements*, Erving Goffman extends this tradition of analysis to the posed illustrations of contemporary advertising, a genre he felicitously names "commercial realism."

Goffman sees commercial realism as a way of portraying life. It is a conventionalized transformation of reality created by contemporary advertisers for consumption by a buying public. In commercial realism a plausible vision of reality is contrived to resemble the glimpsed world—those passing views of strangers we get from the corner of our eye as we hurry about our personal everyday life.

Goffman treats "social portraits" as displays of self that are also produced for consumption by others. They are poses people take to signal, among many other things, their alignment to the prevailing codes of masculinity and femininity. The audience who read the semiotic cues are inescapably drawn to judgments of classification, insofar as they subscribe to the "doctrine of naturalness" (the belief that essential behavioral signs are emitted from human beings in the same way light or form is emitted from inanimate objects).

Ultimately Goffman defines gender displays as conventionalized portraits of sex that give others the impression of "femininity" and "masculinity," although, alas, he focuses only on the former; and of the various ways in which "femininity" can be displayed, he focuses only on the idealized bourgeois image of heterosexual sex.

Goffman argues that this idealized bourgeois image of heterosexual sex is drawn from parent-child imagery: the "male" is fashioned after the parent, the "female" fashioned after the child. From the "natural" (in this case, biological) asymmetry of parent-child imagery, dominance and subordination are "naturally" (in this case, ideologically) incorporated into masculine and feminine displays of gender.

How gender displays are fashioned is illustrated by some 500 reproductions of slides or pictures, mostly of women, some of men, and a few of children and dogs for comparison. Most of these images are drawn from commercial magazine advertisements of the 1970s.

In extending the scope of study from novels and paintings to the illustrations of commercial realism, Goffman also wishes to let the visual image stand for the visual image. He wants to dispense with descriptive narrative beyond what is necessary to frame the image at hand properly. This is possible—and perhaps even desirable—if the images in question are clear, focused, and uniformly cropped. Unfortunately, the images in *Gender*

Advertisements are of variable quality. Color, which is in fact an important part of contemporary commercial realism, is omitted, so contrasts are diminished and information is lost. Some examples are so murky you can barely distinguish which is the man and which is the woman. Subtle expressions are obscured when they are most important (e.g., to illustrate guying). In some of the illustrations, all the words are cropped off, while in others only part of the words are cropped. You know something was written there, but you cannot make out what it was. One could argue that words have an iconic role in commercial realism and hence they should be included as part of the scenic properties of the picture, if that was in fact what the creator of the picture intended (see p. 16).

Despite the disservice Goffman's published presentation does to this aspect of his argument, *Gender Advertisements* still recommends itself for its pictures. Through them the author has helped expand the domain of sociological research into the pictorial archives, a methodological innovation as powerful as the interview schedule and not nearly as reactive. Methodological purists are bound to be disappointed by what Goffman has to say about the particular techniques he uses; and in fact, if one wished to fault the book it would be easy to say that its author's procedures of discovery were cursory and his presentation less than rigorous. However, if instead of allocating fault one wished to go on to further discovery and more refined presentation, as well as address the question of proof, a host of possibilities suggest themselves. *Gender Advertisements* is composed of testable propositions. The demonstrable proof of Goffman's assertions about certain patterned regularities in gender displays can be tested directly by taking varied samples of pictorial archives, identifying the trait, and observing its distribution. Or again, the stylistic conventions of commercial realism can be compared with the stylistic conventions of family portraits; or conventions of commercial realism can be examined in different historic settings; or the conventions of family portraits can be compared over time. Again, commercial realism relies on more than advertisements of gender to get its message across. It also portrays age, race, occupation, social class, home life, play, and so on; and each conventionalization gives testimony to the ideology that justifies its construction. These displays are worthy of study in their own right. In sum, *Gender Advertisements* breaks methodological ground for sociologists; I hope that others will explore the pictorial archives and that from this collective venture, more systematic techniques of discovery, presentation and proof will emerge.

Finally, *Gender Advertisements* recommends itself for making philosophic questions respectable again. Picture frames pose philosophic problems. They are bits of reality framed by larger bits and composed of smaller ones. Multiple realms of meaning are represented by their tangible surface. Is this a bride? or a picture of a bride? or a model posing as a bride for a picture? Well, it is all of these and a lot of other "real things" too. Theories must be constructed for each order of meaning. Bits of reality are explained; some part of their interrelationship is comprehended, even if the big picture eludes us.

Politics and Cinema. By Andrew Sarris. New York: Columbia University Press, 1978. Pp. vi+215. $12.95.

Todd Gitlin
University of California, Berkeley

In this latest collection of his film pieces, almost all gathered from the *Village Voice*, Andrew Sarris continues to play the critical bad boy who loves to flog the sophisticates. Although he is capable of getting off an astute observation about a particular film, and thereby helping us see it more sharply, Sarris usually does not so much review films as record his own perambulations through popular culture. Indeed, from this collection the reader will learn more about Sarris's whereabouts, contacts, connections, prides, and prejudices than about anything else. Sometimes he even reviews his own previous foresight. Still, his little homilies are refreshing—until they become wearing. He is the brash, astute, self-obsessed tour guide who too often slips into tales of his greatest tours.

In keeping with the profusion of his first-person mannerisms, Sarris is, as he says, a "liberal skeptic" (p. 75). He is "too formal for the journalists, and too flippant for the academics" (p. 156). Week to week in his column, his forays make for lively reading. If you are going to hear the same voice holding forth regularly in your local café, you might well want it to be an opinionated voice which knows how to trumpet an engaging phrase. Once you have calibrated the pitch of that voice, as it were, you know how to form an opinion of an unseen film; you learn how to make allowances for the consistency of that critical monotone.

But the difficulty comes when the liberal skeptic's scattered pieces are assembled in one place and lay claim to a point of view. For what emerges is a deep self-contradiction in Sarris's approach to films. His delight in the style of individual *auteurs* is tempered, though in an unacknowledged way, by his ideological commitments. In his introduction to *Politics and Cinema* he

> concede[s] that my aesthetics were more deeply rooted in my politics than I had at first imagined. Whether I regard myself as a Christian, a liberal, a skeptic, a centrist, or a dedicated democrat, I tend to prefer tinkering with society to smashing it to smithereens. I choose to live with the injustices and inequalities of bourgeois capitalism for fear of losing the music of individual voices. Marxism, as the professed inheritor of historical forces, threatens the integrity of historical scholarship. When I look at the cinema that has been produced by the capitalistic system, and compare it with the cinema that has been produced under the aegis of Marxism, I conclude that the status quo is not only more pleasurable than what is likely to follow it after the revolution, but that the very flawed system in which I matured turns out to be essential to my continued existence. [p. 7]

This is refreshingly blunt. The "relatively pluralistic aesthetic" that follows is presumably anchored by the critic's willingness to honor the filmmaker's project: "I have never believed that it is the critic's function to dictate the

subject matter of the creator. If I want a certain movie to be made I should make it myself, and not hector someone else into making it" (p. 6).

But Sarris's "relatively pluralistic aesthetic" becomes rather more exclusive when it confronts real films. There it turns out that he has really aligned himself with Lionel Trilling's dismay about "adversary culture" but will not come straight out and say so. Reluctant to take full responsibility for his politics, or to vouch for the necessity of politics in the first place, he contravenes a wholehearted pluralism while taking refuge in an absurdity like: "Art and Revolution. Choose One. I choose Art" (p. 77)—as if alternatives to revolution were not politics! But to remain politically consistent, he does indeed have to "dictate the subject matter" and the filmmaker's approach to it. For example, writing about *Dog Day Afternoon*, he perfectly fairly takes the director to task for the ideological loading of the film: "If Hitler's story, for example, is told entirely from Hitler's point of view then the Jews can be blamed for causing Hitler to have sleepless nights. And if Hitler were to be played soulfully by Al Pacino then history would truly be turned on its head by histrionics" (p. 33). Yet a few pages later, discussing *Lenny*, he is weaving back to the *auteur's* creative prerogatives: "A film is a film is a film, and it should be considered as a self-enclosed entity. Anything to avoid the addled literalism of the dreary realists who confuse mimesis with mimicry" (p. 54).

The point is that Sarris is a straight-out moralist who ought to be direct about it. But he is hampered, ironically, by the very individualist culture he enshrines with *auteur* theory. He is as political as Bertolucci or Costa-Gavras, but in a culture that still regards ideology as a social disease he is not so blunt about it. When the liberal skeptic comes down on Susan Sontag for the "leering leftism" (p. 107) of her "Fascinating Fascism" polemic against Leni Riefenstahl (*New York Review of Books* [February 6, 1975]), he ends up giving Riefenstahl the benefit of the doubt while ignoring Sontag's considerable evidence about Riefenstahl's Nazi commitments—"if only," he writes, "because I cannot find enough evidence of malignity and hatred in her art to make her seem as evil as her political masters. And, anyway, she never claimed to be working for British intelligence while she was making *Triumph of the Will*" (pp. 111–12). Well, Costa-Gavras never claimed to be defending the CIA in *State of Siege*, but that does not keep Sarris from savaging that left film (deservedly, in many places) for its one-sidedness. As a film historian, Sarris is oddly uncurious about the uses to which *Triumph of the Will* was put in the Third Reich. Suddenly it is pure aesthetics to the rescue, and—again, without confronting Sontag's evidence—he airbrushes Riefenstahl as having been, most likely, "an opportunistic artist who has been both immortalized and imprisoned by the horror of history" (p. 108). Suddenly there is no more ideology, only a piece of cant ready-made for evasion. "Horror of history" indeed—Sarris does not let Costa-Gavras off with that nifty passport to exculpation.

Sarris deplores the "either/or attitude of the sixties" (p. 7), but it is precisely there that he is at home. It is from the left's own overheated melodramas and the counterculture's evasions, oddly enough, that his

criticism derives its verve and perhaps its smugness. Taking off after the left, he prefers easy, if important, targets like Bertolucci and Costa-Gavras and curiously does not even mention Alain Tanner's films of the seventies (*La Salamandre, Le Retour d'Afrique, Le Milieu du Monde, Jonah Who Will Be 25 in the Year 2000*), all of which played in New York during the period covered by *Politics and Cinema*, all of which are full of the left's own ambiguities and complexities. Liberal skepticism, in short, is an interesting position in the hands of a critic who knows that filmmaking is full of choices that carry ideological significance; but Sarris should push his skepticism further, even to questioning whether it suffices.

The Persistence of Freedom: The Sociological Implications of Polish Student Theater. By Jeffrey C. Goldfarb. Boulder, Colo.: Westview Press, 1980. Pp. xiii+159. $18.50.

Arnold W. Foster
State University of New York at Albany

The title of this book indicates that it is dealing with a fundamental problem of human existence. The subtitle points out the area of investigation. At first glance the two do not seem to fit. But they do in Jeffrey Goldfarb's thoughtful treatment of contemporary Polish life. Since control and repression are never total (hence the concept totalitarianism is misleading), where are the areas of freedom in society and what is their significance? In Polish society they are found in science and in the arts, especially in student theater.

In his attempt to understand the reasons for the freedom that exists in student theater, Jeffrey Goldfarb looks first to history since World War II. He finds that those cultural institutions which have indirect ties through their ancillary affiliation with other institutions such as labor unions, the Church, and universities have more freedom than those which have direct ties to Party and government.

Next he looks to cultural norms and social structure. Poland, like other socialist nations, supports both traditional culture and modern socialist culture. Some aspects of traditional culture are in opposition to Communist party goals and serve to undermine them. More specifically, traditional culture supports "independent expression" which allows some leeway for the artist. In addition, the Church represents Polish nationhood and exists as a political force. Although it is as uncomfortable as is the state with free artistic expression, by its opposition to the state it "has strengthened the cultural autonomy of student theaters" (p. 63).

Another factor of importance is the international tradition of freedom for the arts maintained by contact through the media and international conferences. This operates because Poland wants to maintain a good image

abroad. The critical tradition within socialism itself is also active on behalf of the autonomy of the arts.

Art which differs from officially supported socialist realism is called by Goldfarb "socialist expression." There is no direct description of this art. It seems to refer to everything that is not traditional, blatantly international, or socialist realist. Socialist expressionism is influenced by the socialist criticism of Lukács and Fischer (mentioned) and Morawski (not mentioned). It is made by artists who have been educated in a socialist society and operate within its strictures, on topics of concern to people living within a socialist state. It may have some resemblances to socialist humanism, which has been proclaimed in several recent Soviet books on the arts.

Goldfarb's last chapter is a discussion of the usefulness of the dramaturgic model to understand the workings of Communist societies, "because . . . much of the problem of everyday life is saturated with theatricality in the form of Party propaganda" (p. 137). The book ends on an optimistic note by stating that its theme, like the theme of a student play, is that "the drive for freedom, wisdom, and new ideas and ideals cannot be repressed due to the normal actions of less than extraordinary people" (p. 149).

One does not get the full meaning of Goldfarb's book until the last chapter. There, one realizes that two forms of analysis are reinforcing each other. Discussion of a play throws light on the Polish situation, while discussion of the society helps one understand the importance and values of student theater. Drama as a form of reality is examined to help us understand that parallel form of reality called society, and vice versa. They are both constructed by humans living in the world out of available cultural values, and social behavior is often presented as theater, that is, propaganda and spectacle. To use a dual approach such as this is risky unless the researcher is as sensitive to the expressive nuances of drama as he or she is to the value structures of society. Many sociologists who work in the sociology of art believe that the arts can be a useful methodological tool for studying society, and this work is one of a handful which uses that approach. Goldfarb gives an example of how to do this well.

Several important concepts in *The Persistence of Freedom* are not fully explained. The notion of socialist expression needs more specificity. A few concrete examples or a better definition would help. Although student theater is presumably an example, it seems too varied to be drawn together under such a rubric.

Goldfarb's use of the concept of freedom also bothers me. Does he simply mean freedom *from?* Is he assuming that all freedom—political, religious, aesthetic, economic—is equivalent? Could it be that the freedom that exists, that is permitted in Polish society, may simply act as an opiate for the intellectuals? Is student theater really representative of the larger picture?

I miss reference to the aesthetic ethic as described by Max Weber. To what extent do aesthetic values affect the drive for more freedom? Or is the motivation simply political? Certainly people in a Communist state could not proclaim the "art for art's sake" doctrine, but does it exist underneath?

In this book, Goldfarb takes a fresh look at a most important human problem, the struggle for freedom. He has done a remarkable job of untangling a variety of strands having a bearing on the degree of freedom in a Communist state.

Whose Music? A Sociology of Musical Languages. By John Shepherd, Phil Virden, Graham Vulliamy, and Trevor Wishart. New Brunswick, N.J.: Transaction Books, 1980. Pp. xiv+300. $19.95 (cloth); $7.95 (paper).

Paul DiMaggio
Yale University

Whose Music? Not ours, if one believes the authors of this collection. Rather the making of music has been captured by a ruling element and the professions and institutions—music critics and teachers, arts councils, and public broadcasting systems—they control. The volume's eight chapters are well integrated but uneven. At their worst, John Shepherd et al. rehash tired polemics; at their best, they open important avenues for studying the relationship between music and society.

In the first three chapters Shepherd asserts that the significance or meaning of music can be understood only in terms of the experience and meanings of the group or society that creates it. He criticizes absolutist or internalist approaches to music criticism (e.g., Meyer's or Langer's) as rooted in a deplorable dualism—thought versus action, subject versus object—that pervades Western epistemology. This dualism, he argues, undergirds the dichotomy between serious and popular music and blinds establishment musicians and critics to the value of non-Western styles. Shepherd concludes with a technical analysis of preliterate, medieval, and tonal music as reflections of the cognitive and social structures of, respectively, preliterate, feudal, and industrial peoples. The final contribution in part 1 is a well-crafted essay by Trevor Wishart on musical notation, which, he argues, acts as "an historic filter" (p. 135) selecting for such notatable qualities as pitch and rhythm and distracting attention from timbre, acoustic quality, and unrecordable nuances of rhythm or phrasing.

Part 2 consists of several efforts to relate music and social stratification in the modern context. Phil Virden and Wishart, following Shepherd's argument that preliterate music is implicit and modern music explicit (shades of McLuhan's hot and cool media) and drawing on the work of Basil Bernstein, contend that the blues and other working-class and peasant musics represent a restricted code, in contrast to the elaborated code of classical tonal music. Graham Vulliamy analyzes the serious music/popular music dichotomy in terms of class ideology and institutional mechanisms that preserve that dichotomy and goes on to trace, within pop music, the development of a legitimating ideology of progressive rock similar to that of fine-arts music itself. In a second chapter, he uses the techniques of the

"new sociology of education" to argue that primary and secondary music teaching has reinforced the hegemony of Western tonal music by stressing notation, devaluing alternative aesthetic criteria, and identifying musical ability with academic intelligence. In a final chapter, Wishart puts forward a "radical materialist" (p. 233) alternative to the positions of Lukács and Adorno, contending that art has no transcendent significance and that, for the radical musician or critic, "the only valid criterion upon which we can assess his work is its *political effectiveness*" (p. 241).

This necessarily brief summary does justice to neither the subtlety of some arguments nor the emptiness of others. The book is flawed by its two-chapter prolegomenon, a muddy concoction of ethnological anecdotes and warmed-over *Gutenberg Galaxy*. Shepherd, in a claim that might make even Yoko Ono blush, suggests that an acceptance of a non-Western musical aesthetic would threaten "the entire centralized social-intellectual structure of industry society" (p. 44). While his discussion of the social-structural bases of preliterate, pentatonic, and tonal musics is better, it too is flawed, both by implicit but undefended assumptions about the structural nature of cognition and by his inexplicable failure to cite, much less discuss, Weber's *Rational and Social Foundations of Music.*

Given their effort to root the institutional hegemony of European tonal music in Western epistemology, rather than in the interests of status groups in protecting the value of their cultural capital, the authors are ill prepared to explain (although some of them note) the almost total eclipse, in a period of about 50 years, of the Western tonal tradition by popular music based in Afro-American styles and marketed by record companies and their conglomerate owners. Throughout the volume, they decline to specify the nature of the ruling elite or to address empirically the relationship between the taste structure and the stratification system. In part, this may result from lack of consensus. Shepherd's villain is the epistemological basis of Western civilization, while his coauthors are content to indict the contemporary upper class and their academic collaborators. Wishart lays claim to a radical materialist position, while Shepherd condemns the "assignation of a higher rational priority . . . to the political economic infrastructure" (p. 101). In part, this omission seems to represent a disregard for existing data on the social composition of modern audiences. Instead, responsibility for tonal hegemony is attributed to a murky elite whose interest in the matter is unclear if one does not buy the assertion that an unresolved seventh or a legitimated slurred fifth could shake capitalism to its foundations. The effort to use Bernstein's schema to explain differences in musical styles is ingenious, but the analogy between implicit/explicit and restricted/elaborated is inexact, at best. And the suggestion that Motown, or even the blues, can be illuminated by reference to the experience of preliterate tribal societies seems a trifle condescending. In general, the authors' treatment of modern pop music—with the exceptions of Vulliamy's chapter on the rock aesthetic and Virden and Wishart's discussion of the blues—is shallow. We learn little about rhythm and blues, jazz, or reggae but that they are "implicit," nothing about punk or new wave, and nothing about country

music, the latter, presumably, because it is too linear and explicit to fit their theory.

Whose Music? may be important reading for musicologists, critics, music educators, conventional musicians, or Reagan-administration arts-policy braintrusters, although its authors do not preach in a manner calculated to convince the unconverted. Its basic point—that the aesthetic standards used to promote fine-arts music and degrade pop music are arbitrary and maintained by the power of elites and their allies to monopolize the training of musicians, the setting of curricula, the legitimation of musical "art," and the awarding of benefices to composers, performers, and sponsors—is unexceptionable. But, lacking the sustained, rigorous argument and evidence it deserves, the authors' relativism is as much a political artifact as is the elitism they decry, and it should be subject to the same mode of analysis.

Artists in Offices. By Judith Adler. New Brunswick, N.J.: Transaction Books, 1979. Pp. xv+165. $16.95.

Steven C. Dubin
University of Chicago

Judith Adler's study of an attempt in the late 1960s to create and institutionalize an avant-garde art scene calls to mind a somewhat earlier Lawrence Ferlinghetti poem. In this piece, a man is seduced by a woman's suggestion that together they could "really exist." Yet, the next morning the expectations of a durable relationship are abruptly dashed: she is found to have bad teeth and hates poetry (see *A Coney Island of the Mind* [Norfolk, Conn.: New Directions, 1958], p. 22).

Artists in Offices chronicles the establishment, development, and eventual reorganization of the California Institute of the Arts (Cal Arts) from approximately 1967 to 1975. This endeavor was but one of a number of utopian, communitarian-type schemes that their designers hoped could alter basic institutional arrangements at the time. Yet, Cal Arts attempted to accomplish its end through the marriage of very unlikely partners: a board of directors representing an endowment from the rather conservative Disney Foundation, and artists drawn from the entire country hoping to establish a strategic beachhead as "aesthetic revolutionaries." Adler's research and analysis clearly detail the reasons for the relationship going sour. At the same time, however, she conveys the very real sense of excitement and possibility inherent in the initial stages of many such endeavors.

The California Institute of the Arts was an attempt to build a major new cultural center on the base of two previously established commercial art schools. While the artists who were attracted to Cal Arts anticipated having a position of leadership in directing the growth and development of society (similar to Saint-Simon's vision), their failures in this regard highlight the increasingly desacralized nature of their actual role. By being drawn into such institutional structures with greater frequency, artists have taken on

a less marginal and alienated role. Because of this, Adler, like a number of recent observers, is able to regard the artist not as a romantic figure but as a worker involved in a network of relationships. Accordingly, the diminished saliency of the artist's role as an independent craftsman, representing a "pre-industrial" type of production, is emphasized throughout.

Certain other aspects of Adler's work both draw from and add to previous discussions of communitarian movements. For example, she examines the problems of how to maintain boundaries, manage "visitors," and sustain the initial enthusiasm and ideological commitment to the group and its purpose over time. These organizational considerations are crucial in the analysis of any utopian scheme; they assume even greater importance in this instance because of a central feature of the venture. This particular group was not one of those contemporary units which rose up, served certain purposes for a delimited amount of time, and then disbanded, leaving no institutional residue in its wake. Instead, this was an attempt to establish a radically alternative example, challenging typical authoritarian and organizational aspects of institutions, *within* a host institution. The general sense of possibility and "openness" of the era was represented in the Cal Arts example by the belief that unlimited resources were available. The gradual understanding that this was not the case, with a concomitant shift from setting a new example for the larger society to using the system to maximize personal ends, doubled the assault on the sponsor. While at first the school was expected to finance the development of a structure (or "antistructure") that would gradually supplant it, it was later expected to underwrite the building of more individualized empires by those participants sensing the difficulties in realizing their initial commitment.

These conflicts reflected some central tensions inherent in the interaction of the institutional imperatives of establishing and maintaining a school, and the dictates of an expressive, avant-garde, antiauthoritarian ideology. Adler tells us, for example, that while innovative artistic expression may have been pursued initially, only more traditional art products and performances were displayed by the trustees to the public to support fundraising efforts. Over time, then, those art forms attracting resources to the institution got a correspondingly higher degree of support from those in control of determining the distribution of available funds. Under such exigencies, traditional forms quickly supplanted radical methods. Similar accommodations in initial stances against traditional standards of authority, productivity, and professionalism were made when individuals realized that the likelihood of success within the bureaucratic structure would be negatively associated with their commitment to alternative principles. Again, trustees' standards triumphed over artistic ones. And perhaps most important, Adler emphasizes what the passage of time and the establishment of emotional distance have led observers of other contemporary experiments to elaborate: a revolution in consciousness alone is unable to alter the basic structural arrangements of society. These radical ideas may have been seductive, but society generally, and this board of directors in particular, withstood the rhetorical assault.

Artists in Offices is cogently argued and well written. Adler conveys an intimate sense of participation in the events she records; yet her analysis is not clouded by a nostalgia for either what actually transpired or what might have been. In what is a basically solid, intriguing study, however, there are a few problems which deserve to be mentioned.

For example, while artists are being increasingly drawn into working within institutional arrangements similar to those encompassing other workers, few of these settings attract participants with such a sense of promise for personal development as was present in the Cal Arts case. Instead, most artists working within bureaucratic settings enter with a clearer sense of what is expected of them, in a situation where the authority structure and desired standards of production are not disguised by a competing ideology: for example, the situation of contemporary CETA-sponsored artists whose jobs are typically predesignated to provide public service rather than promote avant-garde principles. And further, the tenuousness of the support for the continuation of these projects sharply contrasts with the initial sense of unlimited potential imbuing the Cal Arts scene.

Additionally, Adler emphasizes the impact of new technologies on the arts, and how important access to them can be. Yet the importance of technological advances differentially affects various art fields; video and some musical artists are greatly affected, for example, while other types of artists remain virtually untouched by recent technical developments. In fact, the labor-intensive nature of many of the performing arts has been much discussed as seriously limiting the impact of innovations relative to their importance in other industries. Thus, the stress in this study on academic settings providing necessary resources is of somewhat limited applicability; there is also a failure to consider alternative institutional arrangements which could offer such equipment, for example, artists' cooperatives. And, although she acknowledges at several points that there are job and prestige markets for artists outside academia, parallel and challenging to such settings, Adler's primary emphasis is on the academic setting. This focus prevents her from answering many of the interesting questions she raises in regard to these competing markets.

Finally, the study's title, *Artists in Offices*, is somewhat misleading. Although the discussion of the role of the artist as a worker is important, most central to the study is the confrontation of ideology with social structure in this one experiment. While Adler touches on some key issues, her particular research setting is quite dissimilar to what is currently going on. For many artists confronting the constraints of institutionalized working arrangements at the present time, the price of job security and a more stabilized, professional role in society is clearly set. To expand our initial image, the attraction of the artist to the security of such a marriage with conventional society—not having to search continually for new, supportive partners—has been offset somewhat by the realization that one's cohabitant might be disgusted by certain expressions of personal style and could demand conformity to somewhat alien standards.

The External Control of Organizations: A Resource Dependence Perspective. By Jeffrey Pfeffer and Gerald R. Salancik. New York: Harper & Row, 1978. Pp. xiii+300. $15.95.

William R. Dill
New York University

It has taken a long time for the idea to take hold that you must understand an organization's environment before you can understand the organization. Or, perhaps more accurately, it has taken a long time and many false starts in research and theory building to become confident enough about describing environments to begin to write books in which environmental considerations get their due.

We still do not have adequate tools for environmental analysis, but progress is being made. Chester Barnard's central idea of an organization and its relevant environment as a managed network of "participants," bartering contributions that each can make for material or psychic inducements in return, has attracted new prominence. Much of the current talk about "stakeholders" and "governance" comes from businessmen and business observers who have just discovered the problem of legitimacy and the multifaceted dependencies on outside constituencies that Barnard, as the head of a regulated telephone company, understood in the 1930s.

Organizational design talks much less today about verities than about "contingencies." Historians like Alfred Chandler, looking back on the development of American business, and proponents of strategic planning like Igor Ansoff and Peter Drucker have shed new light on the importance of environment and offered suggestions that can be useful in behavioral research.

For these times, then, Jeffrey Pfeffer and Gerald Salancik's *The External Control of Organizations* is an interesting and broad-ranging contribution. They start from a conviction, based partly on their own research on the performance of mayors, that perhaps as little as 10% of the variance in the performance of an organization depends on the action of its leaders. Most of the variance can be explained only by analysis of the context in which the organization operates. Pfeffer and Salancik are mainly interested in assessing an organization's level of *dependence* on outside groups for resources, and then in tracing how an organization and its leaders assess the nature and significance of that dependence and find ways to reduce resultant uncertainties or detrimental consequences. They have drawn not only on their own research, but also on a broad range of economic and sociological studies and interesting case examples from the recent history of business, government, and not-for-profit organizations.

Both in a chapter entitled "The Organizational Environment and How It Is Known" and in a later section on strategies for scanning the environment, Pfeffer and Salancik emphasize that the environment the researcher must understand is the constructed environment which members of the organization perceive and that this environment often differs greatly from

the reality with which they ought to be dealing. In this focus, they are in harmony with Peter Drucker and with a new book by Dale Zand, *Information, Organization, and Power: Effective Management in the Knowledge Society* (New York: McGraw-Hill, 1981), that access to knowledge is one of the most important sources of managerial power.

The authors' conclusions about how organizations get control over situations where they are greatly dependent on outside actors seem to recommend absorption, cooptation, diversification, and countervailing growth. They argue that one of the main reasons for mergers is to achieve stability against uncertainties of interdependence rather than profitability per se. They view the movement toward joint ventures, external boards of directors which interlock with many other institutions, and involvement in associations and cartels as trends that can be explained in terms of the complexity and difficulty of dependence relationships that an organization faces. They see government regulation as something which organizations seek for protection against the vicissitudes of their environments much as environmental groups do for protection against the organization.

An interesting chapter looks at the question of executive succession—choices and displacements—as a response to environmental dependencies. This is very much in line with work being done by Eugene Fama, Michael Jensen, and others from an economist's perspective.

Pfeffer and Salancik are ingenious in the variety of organizational settings they have examined and in the creativity with which they have used simple questionnaires and public sources to get measures of the environmental variables they are interested in. Since most of what they report, though, are correlation coefficients, questions of cause and effect in the relationships they examine are not clear. As with all correlation studies involving complex and messy data, comforting suggestions about statistical significance are frequently offered for coefficients which, in reality, explain only a very small amount of the variance. One would be more comfortable about the empirical claims if there were fewer separate studies and more systematic replications.

The authors conclude with some tentative recommendations for organizational design: First, correct knowledge about important environmental dependencies calls for organizations to devote more resources to environmental scanning and to have operating people who must make decisions based on such information more actively involved than most are today in the process of collecting and interpreting it. Second, organizations which have grown too dependent in uncontrollable ways on outside groups should consider escaping those dependencies by diversifying into other fields. They are advocates of "the grass is greener," despite evidence in a recent McKinsey and Company study that the most successful businesses follow a "stick to thy last" strategy.

Third, to cope with the conflicting demands that they acknowledge diversification creates, they argue the importance of having "organizational slack" (Cyert and March's term for resources beyond plan and budget to use where you need them) and a loosely coupled, constituency-oriented organizational structure. "Slack" is a product of organizational success,

but it is hard for an organization in trouble with its environment to pull together. Loose coupling is an interesting and important managerial concept, but one which many too casually diversified organizations have found impossible to implement.

Fourth, they urge that there should be protections in organizations against institutionalization of power for executives and improvements in ways to change leaders in response to environmental demands. Here I think they underestimate the degree to which many organizations are experimenting with "offices of the chief executive" to provide pluralistic rather than autocratic, single-focused leadership and the degree to which, even with the diminution of shareholder controls, other environmental groups and an increasingly explicit managerial labor market keep the powers and prices of managers in check.

Founders, Heirs, and Managers: French Industrial Leadership in Transition. By Dean Savage. Beverly Hills, Calif.: Sage Publications, 1979. Pp. 227. $18.00 (cloth); $8.95 (paper).

David Swartz
Boston University

The impact of patterns of corporate ownership on organizational behavior and on the composition of industrial leadership represents an important, albeit neglected, research topic. Traditionally, studies of business elites tend to limit their focus to the characteristics of individuals who occupy leadership positions in the major firms. Emphasis is given to social origins and educational attainment of leaders. Studies by economists and students of organizations, in contrast, tend either to abstract the characteristics of firm leaders from consideration of economic and corporate structure or simply to note them in passing.

Readers interested in an approach to the study of industrial elites that systematically relates elite composition to organizational behavior and industrial structure will welcome *Founders, Heirs, and Managers*, by Dean Savage. Savage charts out an "ecological" approach in which he relates firm leadership to firm demography. Structures, organizations, and individuals interact, each influencing the other. Savage's approach calls for the simultaneous examination of firm behavior, size, ownership, and leadership and permits him to offer some important correctives to prevailing views regarding the sources of firm growth, the role of the family firm, and the nature of managerial recruitment in France.

Sociologists will find particularly interesting Savage's claim that the primary effect of the shift from ownership to managerial control is not on firm performance but on leadership selection and tenure among the very large firms. Moreover, he demonstrates how firm characteristics such as type of ownership explain certain patterns of leadership recruitment, advancement, and tenure better than do social origins and education.

Savage challenges the prevailing view among French planners, civil servants, and big business leaders that attributes the recent economic growth and industrial expansion in France to the rise of a meritocratically selected and well-trained class of industrial managers, on the one hand, and to the development of very large firms that are able to increase productivity through economies of scale, on the other. Further, he offers a modification of the well-known family firm thesis, put forward by David Landes and Jesse Pitts, that would attribute the relatively late modernization and the persistence of a sizable small firm sector in the French economy to a family ownership mentality that places more emphasis on security and stability than on risk taking and expansion.

Drawing on the work of economists, Savage observes that, in spite of postwar industrial concentration through mergers, the overall size distribution of firms has remained relatively stable in France as it has in most Western industrial countries. Large numbers of small and middle-sized firms seem, therefore, to be a permanent part of the modern industrial picture rather than a vanishing legacy of the past. He further notes that there is considerable firm mobility (i.e., entry, expansion, decline, and disappearance) within that stable size distribution. Such stability suggests that one must look to factors other than firm size to explain differential growth rates among firms.

Growth, Savage contends, is better understood in terms of types of firm ownership rather than firm size. A close examination of different ownership types reveals important patterns in firm growth, leadership, recruitment, and careers that are not accounted for by managerial theory. He finds, in particular, that the conventional owner-manager distinction is too general to tap significant differential patterns of growth and leadership selection. In its place, he proposes an expanded typology of ownership that distinguishes four separate categories: founders, heirs, new owners, and managers. Implicit in the typology is the Schumpeterian concern for the impact of leadership on firm behavior, and Savage's founders correspond to Schumpeter's classic example of the entrepreneur.

One key empirical finding of the book is that average growth rates and their variances are differentially distributed according to these categories of ownership. While the standard owner-manager dichotomy reveals no important differences in growth rates (defined in terms of the ratio of employees in 1968 to those in 1958), the expanded typology points up some interesting contrasts. Superior growth rates are found among firms run by founders, whereas those led by heirs and new owners average less growth. Moreover, the record of second-, and especially third-, generation heirs shows sharp drops in growth rates as firms are passed on to subsequent generations. In contrast, managerially directed firms appear on the average more dynamic than inherited ones though somewhat less so than founder-directed firms.

Amounts of dispersion around these average growth curves also vary according to ownership types. (Savage puts the most emphasis on this finding.) The comparatively large variance for firms run by founders and first-generation heirs indicates that some striking successes are to be found

within these two groups. The abrupt shift to much smaller variance in the second, and especially the third, generation suggests that lower growth rates seem characteristic of most firms in these categories. And stable growth seems to be evenly spread across most managerially directed firms.

These findings, according to Savage, suggest that a continuing tradition of entrepreneurship may have a more sizable impact on overall economic growth in France than the rise of a managerial class. Savage contends that current preoccupation with industrial concentration among the largest firms overlooks and underestimates the mobility and vitality of firms at the lower end of the firm size hierarchy where owner-operated firms figure prominently. The decline and stagnation attributed by Landes and Pitts to family-based ownership may be a premature assessment. The findings do indicate that third-generation ownership may be a particularly crisis-laden stage of family ownership, just as Schumpeter predicted. Yet if the older family firms are in trouble—and Savage points out that this may be especially true in the less dynamic sectors such as textiles—firms of founders and their first-generation heirs give evidence of considerable vitality. The trend may be in the direction of first- and second-generation family ownership rather than away from family-based ownership tout court. In any case, the twilight phase of family capitalism, Daniel Bell notwithstanding, has yet to begin in modern industrial France.

Savage goes on to demonstrate the usefulness of his expanded ownership typology for understanding French business leadership recruitment and careers. He suggests that by looking at social origins and education exclusively we miss the important mediating role played by ownership on the selection and careers of firm leaders. Heirs, for example, tend to obtain their leadership positions at a relatively early age and do so through kinship rather than educational credentials. Founders and new owners, in contrast, tend to come from quite diverse though generally less academically prestigious and socially exclusive backgrounds. Managerially directed firms, however, recruit overwhelmingly from the most socially privileged (i.e., professional and high-ranking civil servant) and educationally prestigious (i.e., the *Grandes Ecoles*) milieu. Savage thus finds that in France the shift from ownership to managerial control, considered by many sociologists to be part of a larger trend in advanced industrial societies from ascription to achievement, has actually narrowed the social base of the French business elite.

There are, however, difficulties in Savage's work. He is certainly correct in emphasizing qualitative (e.g., type of ownership) rather than purely quantitative (e.g., firm size or age) measures of differential firm performance. Yet such a strong claim for the special contribution of type of ownership to firm behavior would call for a stricter set of controls for firm size and industrial sector than Savage gives. The relationship between firm growth rates and ownership types should be examined within as well as between size categories. We do not learn, for example, how managerial control compares with that of founders among small- and medium-sized firms where the record of the latter appears so impressive.

Savage tells us that founder-controlled firms tend to be located in the expanding industrial sectors, such as electronics and chemicals, whereas the less dynamic firms run by heirs tend to be found in declining sectors, such as textiles. But if type of ownership and sector are conflated, we need to examine the relative effects of different types of ownership on growth *within* sectors, such as construction, where enough cases in the sample would permit meaningful comparison.

Finally, and more generally, it is somewhat risky to claim that ownership exercises the principal direct effect on firm behavior. Firm growth rates likely stem from many different factors. As Savage admits, leadership effect on firm performance may be more pronounced among the smaller, more manageable firms than among the very large conglomerates. Firm growth rates, moreover, may have less to do with leadership capabilities and goals than with, for example, the market position of the firms (e.g., competitive vs. monopoly), industrial sector, and state policy. Such economic and political factors are likely to set the parameters within which leadership capabilities and goals are expressed.

Furthermore, it would be useful to distinguish firm growth according to whether it results from internal development or from merger among firms, especially since the study was conducted during a time of intense industrial concentration and restructuring in France. And of course, growth rates over more than 10 years, or during times of economic decline or stagnation as well as expansion, might well reveal quite different patterns. These possibilities merit further study. Yet Savage's central argument is certainly correct—that different types of ownership can indeed have important effects on firm behavior and leadership patterns.

Perspectives on Social Network Research. Edited by Paul W. Holland and Samuel Leinhardt. New York: Academic Press, 1979. Pp. x+532. $29.95.

Richard D. Alba
State University of New York at Albany

Over the past 10 years, network analysis has become a sort of shibboleth. Strong expectations have been expressed that network analysis will provide both a means of representing the fabric of social structure in a direct way and a bridge between the study of micro- and macrostructures. This sense of mission has been spurred by the enormous computational and analytic advances of the 1970s, stemming partly from the more systematic use of graph theory and its computational machinery and partly from the development of a novel approach to the study of structure, blockmodeling.

Although published in 1979, *Perspectives on Social Network Research* represents largely the state of the art midway through the decade. Edited by Paul W. Holland and Samuel Leinhardt, it is a collection of papers from a symposium held at Dartmouth College in 1975, which was attended by

many of the well-known network researchers at that time, including non-Americans. The book is large, containing 25 papers written by 29 authors and coauthors. In addition to papers derived directly from the symposium presentations, the book contains a survey paper on blockmodeling, written by John Light and Nicholas Mullins, commissioned after the symposium to represent this approach.

As is inevitably true in a collection of this kind, the book sprawls across a wide terrain, covering some parts fully and others thinly. Some papers survey existing research knowledge, others present new research, and still others comment on the symposium papers or aspects of network research. Given the range of topics, almost any reader will find some papers of interest to him or her, although I should add that the book is stronger on mathematical models than on methods of data analysis. The range of the book is indicated by these few examples: Ove Frank on estimation from snowball samples, Patrick Doreian on structural control models of changes in groups, Joel Levine and William Roy on the study of interlocking directorates, and Richard Roistacher on the management of network data.

As there is no way to do justice to the contents of such a wide-ranging book, I will focus on what I found especially interesting: the book's reflection of unresolved strains in the foundation of network analysis. One that stands out clearly lies in the tug-of-war between microstructural and macrostructural approaches. The origins of network analysis lie in the former, in sociometry, the study of small, well-bounded groups; and the tools of macrostructural analysis generally derive from prototypes invented by the early sociometricians. Despite this common kinship, the book gives the impression that network analysis has not so far provided a bridge between micro- and macrostructural analysis, as its contents generally fall into one or the other domain. The book, incidentally, is more complete on the microstructural side. There are, for example, six papers that are devoted to mathematical and other issues arising from "balance theory," while there is no paper that provides a full exposition of the graph-theoretic tools for analyzing large-scale systems (Everett Rogers's paper on diffusion comes the closest).

If macroanalysis has essentially borrowed its tools from microanalytic prototypes, the question arises whether the tools are in fact appropriate for the study of large-scale structure. The question is made compelling by two valuable contributions in this volume, one by J. A. Barnes and the other by Mark Granovetter. Both are concerned with the problems that arise from the study of extensive and poorly bounded systems. Barnes points out that the findings from many macrostructural studies are vulnerable because of the practical necessity to impose arbitrary boundaries for a study of any kind to be carried out. Granovetter calls attention to a related exigency of large-scale studies: the need to sample. Because the units being sampled are treated analytically as interdependent, the impact of this sampling on the structure found by the analyst is far from clear.

Even more important is Granovetter's claim of a "theory gap" in network analysis. The gap arises from our lack of clarity about the nature and

consequences of "structure." One expression of this gap is the attempt to identify interesting structure by comparing observed structure with the predictions of random baseline models. Because there is no theory to guide the formulation of these models, they may confuse more than they clarify. A second expression lies in the uneasy translation between micro- and macrolevels of discourse. One example Granovetter provides is in terms of balance theory, which applies well to affective relations in small groups but seems inapt for some critical features of large structures, like cross-cutting cleavages. There are, then, dangers in viewing large structures as merely aggregates of smaller ones.

It is hard to know what sort of recommendation to give for this book. Unfortunately, it is very expensive. That fact, together with the age of its contributions and its uneven coverage, makes it a book that not everyone will want to own. Nonetheless, it is a book to which anyone interested in network analysis will want to be able to refer.

Networks and Places: Social Relations in the Urban Setting. By Claude S. Fischer, Robert Max Jackson, C. Ann Stueve, Kathleen Gerson, and Lynne McCallister Jones, with Mark Baldassare. New York: Free Press, 1977. Pp. ix+229. $14.95.

Noah Friedkin
University of California, Santa Barbara

In *Networks and Places*, Claude S. Fischer and his coauthors tackle a set of challenging questions. A comprehensive treatment of the methodological and substantive issues their analysis raises is beyond the scope of a short review. While the book is generally concerned with male friendship relations in urban settings, the variety of perspectives from which these relations are analyzed is impressive. Readers, depending on their interests, will find some chapters more pertinent and provocative than others. Accordingly, I provide a brief description of the concerns of each of the chapters in which empirical results are presented. In addition, I comment on the authors' analysis of network multiplexity and density (an analysis which is central to their overall conclusion) and on the bearing of the data as a whole on their theoretical concern—whether or not a "decline of community" has occurred in modern urban societies.

Empirical results are first presented in chapter 3 and are concerned with the effects of multiplexity and density on the intimacy, frequency of contact, and duration of friendships. Multiplexity is defined as the number of different relations involved in a friendship: kin, co-worker, neighbor, and fellow member of a voluntary organization. Density is defined in terms of the number of friendship ties existing in networks consisting of the respondent and his three best friends. Classical theorists have proposed that a decline of multiplexity and density has occurred in modern urban societies and that, as a consequence, the intimacy and supportiveness of peoples'

social relationships have deteriorated. The authors find little support for this proposal in their findings.

I believe that the authors' analysis of multiplexity is confounded by a lack of control for the geographical distance separating friends. Usually, it is not possible for someone who resides hundreds of miles away to be either a co-worker or a neighbor (two of the roles involved in the definition of multiplexity). Thus, multiplexity will tend to be low among geographically distant friendships. Meanwhile, Fischer et al. point out that intimacy tends to be higher in these friendships than in nearby ones. Consequently, when geographical distance is controlled, a more substantial relationship between multiplexity and the quality of interpersonal ties may emerge, particularly in those dyads whose members are in a position to achieve a multiplex relationship in the terms defined by the authors.

With regard to their negative conclusion about the effect of network density on the quality of interpersonal ties, it is noteworthy that the authors do find that nearby friends tend to be intimate when their networks are dense; however, they discount this finding because geographically distant friends were found to be intimate independently of network density. The issue of the salience of these geographically distant friendships is of obvious importance here (indeed, it is an issue of importance throughout the book). I am unconvinced that the negative finding among geographically distant dyads outweighs the positive finding among proximate friendships.

Chapter 4 consists of an analysis of how friendships are determined by occupational status, economic sector, educational attainment, ethnicity, and age. The results are consistent with other analyses that have found friendship to be more probable among persons who are similar in their socioeconomic position. An attempt is made to explore for specifications of these relationships and to assess the joint effects of the socioeconomic variables on friendships. The chapter is a useful supplement to Edward Laumann's previous analysis of the same data set (*Bonds of Pluralism* [New York: Wiley, 1973]).

Chapter 5 should be read by everyone who picks up this book. It examines how friendships are altered during the course of a life cycle marked by these stages; young single, young husband, young father, middle-aged father, old father, and postparent. The authors recognize the limitations of cross-sectional data to develop conclusions about longitudinal phenomena. Their findings are pretty and show that life-cycle changes are powerful determinants of friendship.

Chapter 6 examines the relationship between neighborhood population density and a variety of measures of residents' personal relationships: number of friends, frequency of contact with friends, visiting with neighbors, intimacy of friendships, multiplexity, and relational density, among others. It appears that population density has a negligible influence on personal relationships, although the authors note that such effects may appear at levels of population density, high or low, that lie outside the range of density values that could be examined. The chapter closes with interesting speculations on how density might affect personal relationships.

In Chapter 7 the authors suggest that a suburbanite encounters fewer rewarding people than a city dweller does in the same distance from their homes and that the interaction costs of maintaining a friendship increase with distance. As a consequence, a suburbanite (who seeks to maximize rewards and minimize costs) will choose persons as friends who live nearby rather than traveling further to maintain more rewarding friendships (under the assumption that persons weight the costs associated with distance higher than the benefits of rewarding interpersonal interactions). Weak effects are found in support of the idea that distance from the city center is positively associated with localism. The authors attribute the weakness of this support to individual adaptations: for example, the willingness of suburbanites to incur greater costs than the city dweller to maintain rewarding friendships.

Chapter 8 is concerned with the nature and causes of peoples' involvement in and subjective feelings toward their neighborhoods. A variety of factors are found to affect peoples' attachment to place, and many interaction effects are described. The authors conclude that previous understandings of peoples' attachment to place are oversimplified. While people vary considerably in how they are attached to their neighborhood, it is found that a substantial level of attachment to place persists among people residing in modern urban settings.

The first part of chapter 9 assays evidence in support of the finding that friendships which are maintained over long distances are more likely to be intimate and supportive ones than ones developed among people who live nearby. The second part of the chapter assays evidence in support of the idea that mobility does not necessarily have any ill effects on persons' well-being.

In the light of the empirical evidence presented in chapters 3–9, the authors conclude that there is little evidence to support the classical theorists who claim that a decline of community has occurred in the form of a deterioration of intimate and supportive social relationships in modern urban settings. To a considerable extent, their conclusion depends on whether geographically distant friendships are as salient for psychological well-being and social integration as proximate friendships are. The authors' argument implies that peoples' psychological well-being and social integration are independent of the spatial distribution of their friendships. However, the evidence on the role of distant friendships is not definitive. It could be argued that intimate friends who are separated by great distances are a part of the problem (i.e., the decline of community) and not its solution (i.e., a community without propinquity). The viewpoint expounded in this book will become compelling only when it is demonstrated in a more direct way that geographically distant friendships may substitute for proximate friendships to maintain a high level of psychological well-being and social integration.

The analysis as a whole is based on the proposition that psychological well-being and social integration are functions of the number of a person's intimate and supportive dyadic relationships. The analysis focuses on dyadic

relationships and on the marginals of the matrix of these dyadic relationships; it ignores or dismisses the arrangement of relationships among persons. Of course, if the structure of indirect connectivity in a social network can be ignored, the account of community phenomena is radically simplified. My understanding of the authors' position is that they do not consider the formal properties of social network structure to be highly salient. An alternative viewpoint is that the levels and types of indirect connectivity in social networks have a considerable impact on individuals' well-being and social integration. Even if it is the case that intimate and supportive ties need not be local ties, it may be that intimate and supportive extralocal ties contribute little to the development of those levels and types of indirect connectivity that are important for individual well-being and social integration.

The Changing Academic Market: General Trends and a Berkeley Case Study. By Neil J. Smelser and Robin Content. Berkeley and Los Angeles: University of California Press, 1980. Pp. xiii+198. $16.00.

Nathan Glazer
Harvard University

The Changing Academic Market is, as far as I know, a unique study: it is a detailed account of the search by the sociology department of the University of California, Berkeley, to fill three positions for assistant professor in 1975–76, written by the then chairman of the department, the distinguished sociologist Neil Smelser, and the assistant to the chairman, Robin Content. The account of the search—which includes everything except the names of the members of the department and the candidates—is placed within the context of a wise and subtle analysis of the changes that have taken place in the past decade in the academic market. These changes are to some degree familiar: fewer jobs and more applicants; increasing political pressures on universities from students, legislatures, and administrative boards (though these pressures were reduced as the decade wore on); and increasing pressures for equity as expressed in the requirements for affirmative action. But their implications for academic search and appointment, while discoursed on and grumbled about by faculty members, have not been analyzed in print before, certainly not with the care and attention to detail and sound research procedures that we find here.

The thesis can be given in a sentence: "Taken together, the institutional changes we have just traced [those pointed to in the preceding paragraph] boil down to a single, paradoxical demand on recruiting academic units: while these units are under pressure to recruit fewer new faculty members, they are simultaneously being asked to devote more time and resources to the recruitment process" (p. 170). Fewer jobs does not mean fewer applicants, since the process of producing students with the Ph.D. degree is a

long one, and it takes quite a while for the supply to respond to the slowdown in demand. (In addition, this is not simply a matter of supply and demand: some people will insist on studying in a field because of an interest that is almost impervious to a forecast of future employment opportunities). At the same time, the political conflicts of the late 1960s and early 1970s left a heritage of division and distrust, one particularly marked in sociology and in the sociology department at Berkeley, which made the achievement of consensus difficult—not that the achievement of consensus in sociology is anywhere easy. But perhaps most important for the search and appointment process was the rise and institutionalization of public procedures for recruitment and of documentation of search and evaluation because of affirmative action requirements.

This institutionalization means similar procedures in all searches—what happened at Berkeley is not unfamiliar to anyone who has participated in a search for faculty in recent years: early advertisements made more difficult by the financial stringencies that make approval of new positions a more elaborate process; supplementation of advertisements by letters and calls to leading departments and other individual contacts to turn up promising candidates; special procedures to make sure that potential minority and female candidates are flushed out; a complex process of review of the folders of hundreds of applicants with their vitae, recommendations, and publications; the selection of a number of applicants to visit the campus to be interviewed by faculty and graduate students and to present formal and informal accounts of their work; and finally the departmental decision and the efforts of the chairman to bag the candidates selected. What is interesting is how uniform these new procedures have become, compared with the exaggerated and caricatured but largely true (for its time) account of Theodore Caplow and Reece McGee of the academic marketplace of the 1960s (*The Academic Marketplace* [New York: Basic, 1958]) with its emphasis on personal networks. We have moved to a more bureaucratic and equitable procedure. But with what results, in terms of who gets jobs?

Hardly any, at least by the evidence of this search. Nor would additional experience from other searches at leading universities in sociology, as far as I know of them, lead to a different result. There were 285 applicants for the three positions, 252 for whom detailed information was available. The three final successful applicants were from Harvard and Chicago. "Towards the end . . . , we developed a somewhat bitter joke that the whole thing could have been done for the cost of two 13-cent stamps to . . . Chicago and . . . Harvard" (p. 176).

Casting the net wide has costs, of course, in money and faculty time, and one of the things Smelser and Content are concerned to do is to show how the whole process can be somewhat rationalized—through regular procedures, the use of mechanical typewriters, placing information on candidates on computer tape, involving representative subsamples of the faculty in each search to help mold consensus, and the like. But there are other costs that cannot be reduced so easily, and perhaps the most striking point in the book is that to cast the net wide, to activate hundreds of young scholars to

throw their all into a process that must arouse their anxiety and that must cast down all but a tiny few, may be a cost that we have not sufficiently weighed in our search for equity. Of the 176 candidates from institutions in the third and fourth categories of doctoral degree-granting institutions (four categories were constructed from American Council on Education evaluations of doctoral programs), only three were considered "strong" and none were selected for interview. Of the 54 candidates in category 1, 18 were considered strong and eight were brought to the campus for interview.

Smelser and Content ask, "How does one reject 99 percent of those who have applied for a position?" They report the "task of rejection proved to be emotionally draining," emphasize the "brutalization" of the candidates, and assert, "ironically, one consequence of initiating an exhaustively thorough, fair, and public search . . . is that it increases the brutalizing aspect, because it subjects so many more to first hope, then uncertainty, then rejection" (pp. 133–34).

It is impossible to imagine any easy alternative, and Smelser and Content do not suggest one. They do argue, however, that it would be more valuable to direct the efforts now poured into affirmative action, efforts that mostly produce huge piles of paper, toward producing more and better black and Hispanic candidates (there is no problem in getting women and Asian candidates, as the figures they give on their search demonstrate), rather than proving to administrators that all available efforts have been undertaken to scour a pool with very few fish.

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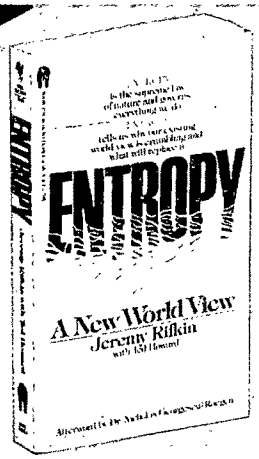
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